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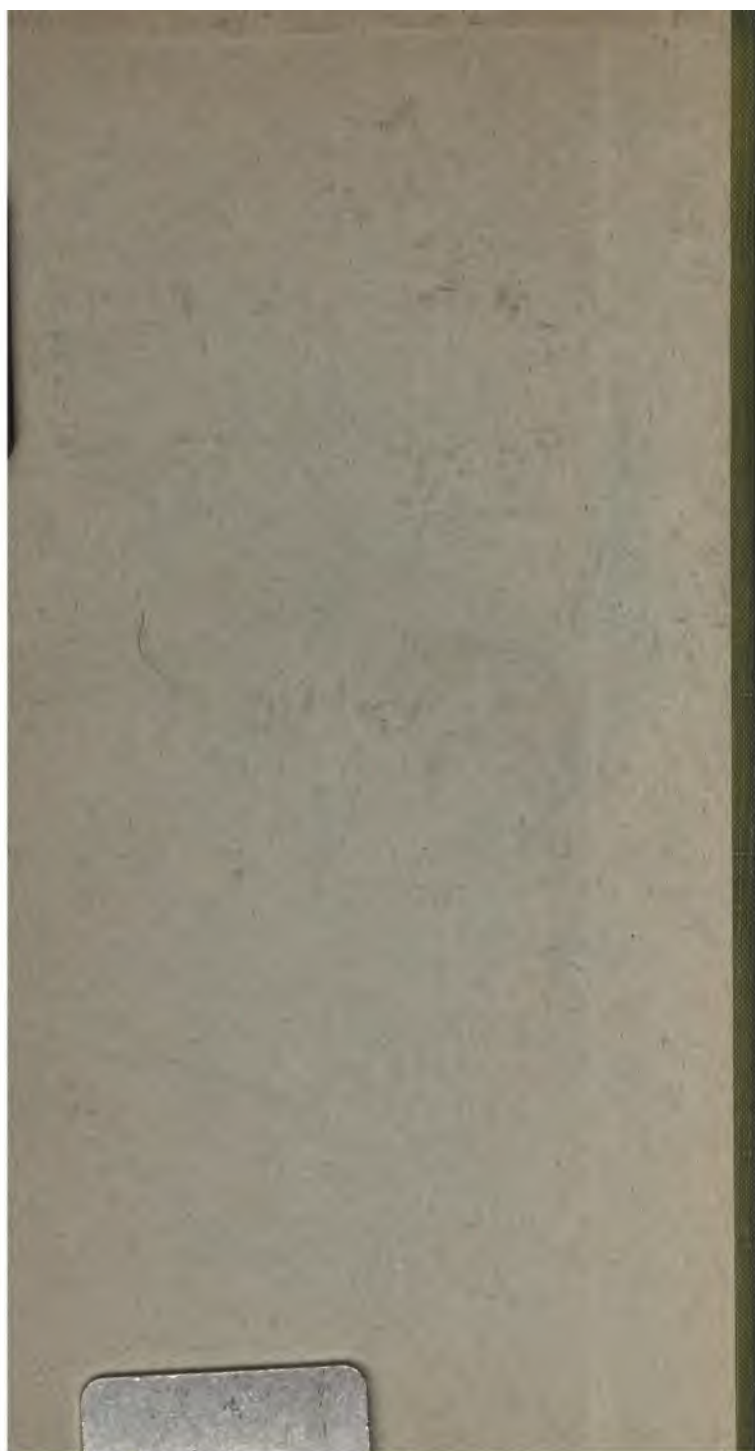
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**ANNALS OF PHILOSOPHY ;**

OR, MAGAZINE OF

**CHEMISTRY, MINERALOGY, MECHANICS,**

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**AGRICULTURE, AND THE ARTS.**

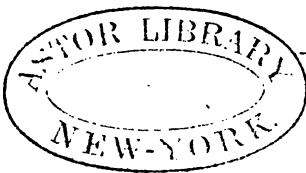
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**BY THOMAS THOMSON, M.D. F.R.S. L. & E. F.L.S. &c.**

**MEMBER OF THE GEOLOGICAL SOCIETY, OF THE WERNERIAN SOCIETY, AND OF THE  
IMPERIAL MEDICO-CHIRURGICAL ACADEMY OF PETERSBURGH.**

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VOL. V

**JANUARY TO JUNE, 1815.**



**London :**

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## NOTICE.

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**T**HIS volume will be found uncommonly rich in new and important scientific facts. It will not probably be often our lot to include so great a number in so small a compass. This is partly owing to the sudden opening of the Continent, which has enriched the historical article commencing the volume with a vast number of new facts, many of them of great importance. The two most prominent papers in a scientific point of view are, 1. The experiments by Berzelius on the composition of organized bodies. It brings a new kingdom under the dominion of chemistry; while the uncommon accuracy of Berzelius has given a degree of perfection to his labours that could hardly have been expected. 2. Gay-Lussac's paper on Iodine. It proves to be a very interesting body; and the new views which its properties lay open greatly increase the interest which the scientific chemist must take in these investigations.

*May, 1815.*

# ERRATA IN VOL. IV.

- Page 420, line 42, *for* and portions of the gneiss, *read* not portions of the gneiss.  
 — 421, — 34, — a granite through gneiss, *read* or granite through gneiss.  
 — 421, — 36, — sea coast, *read* east coast.  
 — 423, — 25, — interrupted, *read* intermixed.  
 — 448, — 34, — pinnæ, *read* donax.

# ERRATA IN VOL. V.

- Page 56, line 12, *for*  $+\sqrt{-1}$ , *read*  $\pm\sqrt{-1}$ .  
 — 56, — 18, —  $y = \sqrt{aa - xx}$ , *read*  $y = \pm\sqrt{aa - xx}$ .  
 — 58, — 22, — ~~experimental~~, *read* exponential.  
 — 59, — 14, —  $\pm 2\pi\sqrt{-1}\left(\frac{36+38}{4}\right)$ , *read*  $\pm 2\pi\sqrt{-1}\left(\frac{26+38}{4}\right)$   
 — 59, — 16, —  $\pm 2\pi\sqrt{-1}\left(\frac{34+34}{4}\right)$ , *read*  $\pm 2\pi\sqrt{-1}\left(\frac{34+34}{4}\right)$   
 — 60, — 25, — 185, *read* 55.

There is an error in the diagram, Plate XXXV, which, referring only to the line that is drawn through the centre, may be corrected without a new plate. The small o at the centre should be a large one, and the consecutive figures *under* the units and *above* them should refer to the circles which they touch. Thus 1 is right, but the following is 1, which should be 1, and so on.

- Page 218, morning observation, May, 1814, *for*  $24^{\circ} 12' 49''$ , *read*  $24^{\circ} 13' 12''$ ; difference *for*  $+ 0' 47''$ , *read*  $1' 10''$ .  
 Page 218, noon observation, Nov. difference *for*  $- 0' 13''$ , *read*  $+ 0' 13''$ .  
 — 218, morning observation, Jan. 1815, *for*  $24^{\circ} 16' 26''$ , *read*  $24^{\circ} 16' 46''$ .  
 — 229, line 4, *for* Macleugh, *read* Macleay.  
 — 397, — 1, — or Changelica, *read* Archangelica.

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# ANNALS OF PHILOSOPHY.

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JANUARY, 1815.

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## ARTICLE I.

*Sketch of the latest Improvements in the Physical Sciences.*  
By Thomas Thomson, M.D. F.R.S.

AFTER an almost total exclusion from the Continent for about seven years, all the kingdoms of Europe have been suddenly thrown open; and it has been in our power, by importing the different foreign journals, to make ourselves acquainted with the various additions which the sciences have received during this eventful period. But these journals amount to so many volumes, that I find myself overwhelmed with matter, and foresee with regret that it will be utterly impossible for me to lay before my readers an historical sketch of the improvements which have been made in all the physical sciences. I shall therefore be under the necessity of confining myself, at least at present, to those sciences which have been cultivated with the greatest ardour, and in which the most important improvements have taken place. These are *Chemistry* and *Mineralogy*. I shall pass more slightly over what has been done in these sciences in Britain and France; because the journals and scientific works of these countries have already become in some measure known to my readers; partly by the contents of the *Annals of Philosophy*, and partly by means of the other London scientific journals. I shall dwell chiefly on what has been done in Germany and the North of Europe; because the languages of these countries are not much cultivated in Britain, and because our connection with them was so completely interrupted, that there is a considerable probability that most of the facts which I shall state are unknown to at least the greater number of my readers.

Next to chemistry and mineralogy, electricity, magnetism, and

optics, have been studied on the Continent with the greatest attention. I shall take a subsequent opportunity of laying the improvements made in these sciences before the reader.

### CHEMISTRY.

This science embraces so vast a field, and is cultivated by so numerous a tribe of votaries, that its annual progress is exceedingly striking and rapid. I shall, for the sake of perspicuity, arrange the facts I have to state under different heads; because I consider any arrangement, even though imperfect, as greatly preferable to no arrangement at all.

#### I. General Principles.

There are two general principles in chemistry of the utmost importance, which have lately attracted a great deal of the attention of chemists, and concerning which various theories have been started which have had considerable influence on the science. These two principles are, 1. The power by which bodies unite chemically. This power is usually known by the name of *affinity*. 2. The proportions in which bodies unite chemically. The facts established relative to this point have received the name of the *atomic theory*. I shall endeavour to lay a short view of the opinions relative to these two principles before my readers.

I. *Affinity*.—In the year 1803 an elaborate set of electrical experiments on the decomposition of salts and other bodies by the Galvanic pile was published by Hisinger and Berzelius.\* This paper was republished in 1806 by the authors in the Swedish language.† In 1803 an abstract of this paper was translated from the German, and published by the French chemists at Paris.‡ Among other conclusions from their experiments, the authors draw the following: Substances are decomposed by electricity according to a determinate law. Oxygen and acids are attracted to the positive pole; while hydrogen, alkalies, earths, and metals, are attracted to the negative pole. This they consider as owing to an affinity subsisting between oxygen, and acids, and positive electricity; and between hydrogen, alkalies, earths and metals, and negative electricity. This important principle was still further developed by Sir Humphry Davy, and placed in a very luminous point of view in his celebrated lecture On some Chemical Agencies of Electricity, published in the Transactions of the Royal Society for 1808, and which gained the prize proposed by Bonaparte for the most important discovery in galvanism. This dissertation deserves, in every point of view, to be considered as the most valuable of all Sir Humphry Davy's philosophical discoveries. His subsequent discoveries were more brilliant, and gave him greater eclat; but they were all derived from this profound dissertation, which pointed out the means of

\* Gehlen's neues allgemeines Journal der Chemie, i. 116.

† Afhandlingar i Fysik, Kemi och Mineralogi, i. 1.

‡ Annales de Chimie, li. 167.

employing galvanism as an instrument of analysis, and the probability by means of it of decomposing many bodies which had previously resisted all the efforts of chemical experiment.

In this dissertation the author showed that bodies which have a chemical affinity for each other are in different states of electricity, the one being negative, and the other positive. Thus when quicklime and oxalic acid are brought in contact, and separated, the acid is found to be negative, and the lime positive. Hence the reason why oxygen and acids are attracted by the negative pole of the battery. They are charged positively; and it is a law in electricity that bodies in different states are attracted by each other. Hydrogen, alkalies, earths, and oxides, are in a positive state of excitement; therefore they are attracted to the negative pole of the battery. Sir H. Davy conceives, with Volta, that there is a peculiar state of electricity, either positive or negative, which belongs to every body; that bodies which have an affinity for each other are in different states, and that the degree of affinity is proportional to the intensity of these different states: or, in other words, the more positive one body is, and the more negative another, the greater is the affinity between them. If we bring them into the same state, that is, render them both positive or both negative, they will no longer remain in combination, but will separate from each other. Hence the reason why common electricity and the galvanic battery decomposes bodies; and it is probable that by its means most bodies may be decomposed, because the affinity between all bodies must be definite, whereas the galvanic intensity may be increased at pleasure almost to any amount. According to this view of the subject, chemical affinity is the same power with the attraction which exists between bodies in different states of electrical excitement; and if any means can be fallen upon to measure this power, we shall at the same time have a measure of chemical affinity.

Such is an outline of the hypothesis of Sir Humphry Davy. Every one, I conceive, who has considered it with attention, must admit the great importance of it as a means of advancing our knowledge of the constituents of bodies, and be aware that it was in consequence of it that Sir H. Davy was led to attempt the decomposition of the alkalies and earths, and to obtain the splendid success with which these fortunate attempts were attended. He will recollect also the facts advanced in support of it by Volta and Mr. Brande, and the happy facility with which it enables us to explain many things that formerly appeared exceedingly obscure.

I am far from pretending to call in question the probability that this hypothesis may ultimately turn out accurate, and think it entitled to the closest attention of every chemist and electrician; but if it be correct, I conceive that the theory of electricity, which at present prevails, will not be able to stand its ground. If negative and positive electricity be qualities inherent in bodies, and continuing in them after they unite together, I cannot for my part conceive the one to consist in a deficiency of electric matter, and the

other in an *excess* of it. Neither can I conceive with M. Dufay, the Abbe Haüy, and some other French electricians, that negative electricity consists in *one fluid*, and positive electricity in *another*, which have an attraction for each other, and neutralize each other when they come in contact. But I can conceive negative and positive electricity to be two attractions inherent in different bodies, which make them unite with each other, and keep them united. But if we adopt this view of the subject, we are not only embarrassed by the difficulty of explaining many electrical phenomena, but we lose the benefit of all the facts stated by Volta, and of most of those advanced by Sir H. Davy in favour of his hypothesis. I myself, therefore, though disposed to think favourably of the Davian hypothesis of chemical affinity, consider it as very far from being sufficiently established to constitute the basis of our researches and our arrangements.

This, however, has been done by Berzelius, who has bestowed much attention on it, and is certainly far better acquainted with the facts brought to light in support of it than I can pretend to be. His view of the subject does not differ much from that of Sir H. Davy; but the additions which he has made when we come to compare them with the present theory of electricity, or with any theory hitherto proposed, are sufficiently puzzling. According to him the acid or alkaline nature of a body depends upon the state of its electricity. If it be permanently *negative*, it is of an acid nature; if it be permanently *positive*, it is alkaline. But when he adds that a body may be positive with respect to one body, and negative with respect to another; though nothing is more easy than to reconcile this with the common doctrine of chemical affinity, it would be somewhat difficult to reconcile it with the theory of electricity. I am far from saying that the thing is impossible; but in order to attempt an explanation of it with success, it would be necessary to make very sweeping changes in the electrical doctrines at present admitted.

Berzelius has given a table of the chemical substances in the order of the intensity of their electricities, beginning with the one attracted most strongly to the positive pole, or the most intensely negative substance; and terminating with the body which is attracted most strongly to the negative pole, and therefore is most intensely positive. The negative intensity diminishes as we proceed downwards in the table, and at last in the centre finally disappears. Then the positive intensity begins; at first very weak, but it gradually increases as we go downwards, and becomes greatest at the lower end of the table. Hence the affinity of the two substances at the two extremes of the table is greatest of all; and as we advance to the middle of the table, that affinity gradually diminishes, and at last disappears. As I do not know the data from which Berzelius constructed his table, I have no means of judging of its accuracy; but I shall give the table in this place, as being well entitled to the attention of the chemical reader. It is doubtless

susceptible of great improvements: but such a table, if it could be constructed with accuracy, would be of prodigious service to the progress of scientific chemistry.

Oxygen,	Columbium,	Cobalt,
Sulphur,	Titanium,	Uranium,
Nitricum,	Zirconium,	Zinc,
Muriatic radicle,	Osmium,	Iron,
Phosphorus,	Bismuth,	Manganese,
Fluoric radicle,	Iridium,	Cerium,
Boron,	Platinum,	Yttrium,
Carbon,	Gold,	Glucinum,
Hydrogen,	Rhodium,	Aluminium,
Arsenic,	Palladium,	Magnesium,
Chromium,	Mercury,	Calcium,
Molybdenum,	Silver,	Strontium,
Tungsten,	Lead,	Barytium,
Antimony,	Tin,	Sodium,
Tellurium,	Nickel,	Potassium,
Silicon,	Copper,	

According to this table, oxygen and potassium have the greatest affinity for each other; and there is very little affinity between iridium, platinum, and gold.

M. Oersted published at Berlin, in 1812, a work entitled *Considerations on the Physical Laws of Chemistry deduced from the new Phenomena*. Like Davy and Berzelius, he has adopted the electrical theory of affinity; but he has not been so reserved as these philosophers. On the contrary, he has pushed matters as far as they will go, and has endeavoured to make his electrical hypothesis complete in all its parts. As this theory has attracted great attention, and gained great eclat in Germany, though I believe it is entirely unknown in Britain, I shall give the outlines of it in this place.

He considers the phenomena of electricity, galvanism, magnetism, heat, light, and chemical affinity, as all depending on the same forces; and he shows that the same cause, which in one case produces electrical actions, occasions in another chemical actions. These actions are produced by two forces; the one *negative*, the other *positive*. These forces are opposite to one another; and by being made to act against one another, may suspend or destroy one another.

Heat is produced by the extinction of the two forces, either in electrical or chemical processes. We may suppose also that the light is derived from the same cause.

Acids which are attracted to the same pole as oxygen possess the same force with that principle; while alkalis and combustible bodies, which are attracted to the opposite pole, possess the opposite force. Oersted arranges chemical substances under two series: the first containing the products of combustion; the second, the

supporters and combustibles. Those of the one series, according to him, do not combine with those of the other; except sulphur and phosphorus, which combine both with the metals and alkalis, and therefore constitute, as it were, the *transition* from the one series to the other. He endeavours to distribute the bodies in these series in a kind of arithmetical progression, beginning with the most combustible bodies, as hydrogen, ammonia, potassium; and continuing to the least combustible, as platinum, rhodium, iridium. If we suppose the series continued farther, we are led to the idea of a body absolutely incombustible. This body must possess in the highest degree the properties which are the opposite to combustibility, and in consequence of the attraction which exists between it and combustible bodies, it must produce the strongest action, which is that of combustion. This incombustible body, in the present state of our knowledge, is *oxygen*.

Between oxygen and iridium the author places carbon, phosphorus, and sulphur. This last substance, though heat makes it pass into the state of a combustible, ought to be considered as a negative body.

The products constitute a similar series, which commences with the most energetic alkalis, and passes to those which are more feeble, till we come to bodies such as alumina, in which that property is balanced by the opposite property having the same force. Then come bodies of a predominating acidity. This is at first feeble in the bodies nearest the point of equilibrium, but it becomes of an extreme activity in the bodies at the greatest distance from that point. We see that each series commences with a certain sum of a property, which diminishes in arithmetical progression, and terminates by leaving the bodies possessed of the opposite property.

Two bodies which belong to the opposite extremes of a series combine with great force; but the energy of this action seems to change their state, since the product of the combination no longer belongs to the same order of affinities. The compounds of oxygen and a combustible body pass into the series of products, as likewise do those of hydrogen and sulphur, of tellurium, and without doubt those of various other bodies. The compound of an acid and alkali no longer belongs to the series of products, but to that of the salts. On the other hand, compounds between bodies at no great distance from each other, as between two acids or two alkalis, do not go out of the series.

This law embraces all the bodies having the same preponderating forces; so, however, that these bodies cannot be confounded. It presents, says the author, under a simple expression this truth, that the same force may exist in a state so different that all its attraction for the opposite force will not be sufficient to make it enter into combination. The two most different states are found in the supporters and the products. The third state is in the neutral salts; but they bear a close resemblance to those products that are placed near the point of equilibrium. Combustibility, then, is the pre-

ponderance of the positive force in a particular state, which M. Oersted calls the *state of supporters*, or of the first class of bodies. Alkalinity presents the same force, but in a different state, which he calls the *state of products*, or of the second class. We do not know the nature of this difference, but we know that it exists, and that the forces are in a state of greater liberty in the first class than in the second.

Combustion sometimes gives us alkaline products, sometimes acid, and sometimes neutral ones. The product, by combining with oxygen, loses either in part or entirely its free positive force, and passes to the order of bodies of the second state. The same thing happens to the negative force of the oxygen. If the body be combustible in a high degree, and does not combine with too much oxygen, the positive force still preserves the preponderance, and the body is alkaline. If, on the contrary, the body be but little combustible, and combine with a great deal of oxygen, the negative force remains preponderant, and the body is acid.

In oxygenated bodies we find of necessity a combustible body and oxygen united together, and constituting a body of the second class. Hence we must conclude that these bodies unite together the two opposite properties, the alkaline and acid, one of which is often rendered insensible by the other. However, in a great number of other combinations, both properties subsist together. This is the case in the oxides of lead, copper, &c. In some bodies we find the positive or negative force in both states at the same time. Thus ammonia possesses both the positive force of the first class and that of the second; while the nitric and oxymuriatic acids possess the negative force both of the first and second class. This is the natural effect of a combination, either feeble or far from the point of saturation, by which a force is reduced to an inferior state. In the combination of tellurium with hydrogen, the negative force of the metal is sufficiently changed by the positive force of the hydrogen to occasion a state of acidity. The positive force contained either in the hydrogen or in the tellurium has not suffered enough of change to come to the state of acid. Hence we find in tellureted hydrogen gas a strong combustibility, and a very distinct acidity, existing together.

It is well known that electricity produces heat. M. Oersted ascribes this effect to a union between the two opposite electric forces; and it is so much the more intense, the greater obstacles the transmission of the electricity meets with, provided that these obstacles may be overcome. The transmission of electricity consists in a series of attractions and repulsions, or in the undulatory movement experienced by the peculiar forces of the body. Chemical union between electro-negative and electro-positive bodies excites always heat, and none of the electrical forces escapes in a state of liberty. However, the increase of conducting power may counter-balance this effect; and even overcome it, and then cold is excited. There exists three principal combinations between the electrical

forces, which are the same as the chemical forces. The first is the combination between the forces themselves; the result of it is the contraction of the two forces, or their reduction to a smaller volume, with the disengagement of light and heat. The second is the combination of a product with a supporter. The result of it likewise is condensation, and the evolution of light and heat, though not to so great an extent as in the first case. The third degree consists in the combination of an alkali with an acid. It is rarely accompanied by the disengagement of light; but always with the disengagement of heat. A condensation likewise takes place unless peculiar causes modify the result. Expansion, which is the effect of a repulsive force in bodies, is most frequently owing to an excess of one of the electric forces. Contraction is the effect of an equilibrium between the forces, and of their mutual extinction.

The forces which produce the electrical and chemical actions of bodies are the same as those which produce the mechanical properties of bodies. Impenetrability depends on the resistance which the expansive power of two forces opposes to a body endeavouring to penetrate the space already occupied by another body. Cohesion is the effect of the two forces, which attract one another. Universal attraction consists in the action at a distance of the two forces upon each other, supposing the expansive power of each force not to extend beyond the surface of bodies.

Such is an abstract of M. Oersted's hypothesis, as far as I have been able to make it out. It is fair, however, to state, that I have not had an opportunity of perusing his own work, but only the abstracts of it published in some of the German journals, and an outline given by Van Mons in his translation of Sir H. Davy's *Elementary Work*. Hence it is very possible that I may not have done the author justice, and that some of the parts of his hypothesis, which appear the most whimsical and absurd, would assume another aspect under the explanations of the author himself. On that account I shall not attempt any critical examination of this hypothesis, which has given the author considerable celebrity in Germany. Its weak parts and inconsistencies are sufficiently obvious to those who have followed the late improvements in electricity and chemistry. The metaphysical part I do not fully understand; nor have I been able to make out whether the author's electrical forces be substances or qualities.

At present, then, it seems to be the prevailing opinion of chemists that chemical affinity is identical with electrical attraction. The opinion possesses much plausibility, and even probability; but much remains to be done before it can be considered as established, and made the foundation of our chemical reasonings.

II. *Proportions in which Bodies combine chemically.*—That the ultimate particles of matter consist of atoms, incapable of farther subdivision, is an opinion which has been pretty generally received among philosophers ever since the time of the Greeks; and since the establishment of the Newtonian philosophy this opinion has

become almost universal. That substances always enter into chemical combination, in determinate proportions which never vary, has been known ever since chemists acquired the art of analysing bodies. Thus carbonate of lime, wherever, or in whatever state, it occurs, is always a compound of 43·2 carbonic acid and 57·8 lime; and sulphate of barytes, of 34·5 sulphuric acid and 65·5 barytes. In like manner, the yellow oxide of lead is always a compound of 100 lead and 7·7 oxygen; and red oxide of mercury, of 100 mercury and 8 oxygen. Sulphuric acid is always composed of three parts of oxygen and two parts of sulphur; and carbonic acid, of 2000 oxygen and 751 carbon. This law is universally admitted by chemists; and, indeed, the more rigorously it has been examined, the more conspicuous and decided have become the proofs in its favour. Even Berthollet, who seems to be an enemy to the atomic theory in the abstract, has admitted that all known compounds unite in determinate proportions; and has endeavoured to reconcile this fact to his own opinions by several highly ingenious, and some rather whimsical, arguments. The few exceptions which he was able to muster up against the law have all disappeared before the more rigid and exact examination of modern analysts.

Mr. Dalton was the first person who ventured to account for this fixedness in chemical proportions. According to him, it is the atoms of bodies that unite together. One atom of a body, *a*, unites with one atom of a body, *b*, or with two atoms of it, or with three, four, &c. atoms of it. The union of one atom of *a* with one atom of *b* produces one compound, the union of one atom of *a* with two atoms of *b* produces another compound, and so on. Each of these compounds, of course, must consist of the same proportions, because the weight of every atom of the same body must of necessity be the same.

We have no means of demonstrating the number of atoms which unite together in this manner in every compound; we must, therefore, have recourse to conjecture. If two bodies unite only in one proportion, it is reasonable to conclude that they unite atom to atom. Hence it is most likely that water is composed of one atom of oxygen and one atom of hydrogen; oxide of silver, of one atom silver and one atom oxygen; and oxide of zinc, of one atom zinc and one atom oxygen.

When a body has the property of uniting with various doses of oxygen, we can then determine the number of atoms which constitute the compounds. Thus manganese unites with four doses of oxygen; and supposing the manganese to be represented by 100, the oxygen of each respective oxide is represented by the numbers 14, 28, 42, 56; but these numbers are to each other as the numbers one, two, three, four. Hence the first oxide is composed of one atom manganese and one atom oxygen; the second, of one atom manganese and two atoms oxygen; the third, of one atom manganese and three atoms oxygen; and the fourth, of one atom manganese and four atoms oxygen. In like manner, as mercury

combines with two doses of oxygen, and forms two oxides, the first composed of 100 mercury and four oxygen, and the second of 100 mercury and eight oxygen, it is obvious that the first must be a compound of one atom mercury and one atom oxygen, and the second of one atom mercury and two atoms oxygen.

Nor is there any difficulty with respect to iron. There are two oxides of that metal: the first composed of 100 iron and 28 oxygen; the second of 100 iron and 42 oxygen. Now as 28 is to 42 as two to three, it follows that the first is a compound of one atom iron and two atoms oxygen; the second, of one atom iron and three atoms oxygen. The same rule holds good with respect to the oxides of nickel and cobalt.

If we know the number of atoms of which a body is combined, and the proportion of the constituents, there is no difficulty in determining the proportional weight of the atoms of which it is composed. Thus if water be composed of one atom of oxygen and one atom of hydrogen, and if the weight of the oxygen in water is to that of the hydrogen as  $7\frac{1}{2}$  to one, then it follows that the weight of an atom of oxygen is to that of an atom of hydrogen as  $7\frac{1}{2}$  to one. If black oxide of mercury be composed of one atom of mercury and one atom of oxygen, and if it be composed of 100 mercury and four oxygen, then an atom of mercury is to the weight of an atom of oxygen as 100 to four, or as 25 to one. If black oxide of iron be composed of one atom iron and two atoms oxygen, and if it consist of 100 iron and 28 oxygen, then an atom of iron is to an atom of oxygen as 100 to 14, or as  $7\cdot142$  to one. Such is the method of determining the weight of an atom of the different substances upon which experiment has hitherto been made. The advantage of such a knowledge is immense; because it gives us the proportions in which the different substances unite together, and even enables us to calculate the proportional constituents of all compound bodies, independent of experiment, and with more accuracy than would result from experiments unless conducted with uncommon precautions.

Hitherto the only persons who have written upon the subject of chemical atoms are Mr. Dalton, Sir Humphry Davy, Dr. Berzelius, Dr. Wollaston, and myself. Mr. Dalton made choice of hydrogen as his unit, because it is the lightest of all the atoms; and Sir H. Davy has followed his example. But as oxygen enters into a much greater number of compounds than any other body, it was chosen by Dr. Wollaston and Dr. Berzelius as the most convenient unit; and in the tables of atoms which I have published in the different volumes of the *Annals of Philosophy*, I have followed their example. Berzelius considers an atom of oxygen to weigh 100, Wollaston makes it weigh 10, and I myself make its weight one. The reader will perceive that these three numbers are the same, the only difference being the position of the decimal point.

The person who has hitherto made the greatest number of experiments upon this important subject is Dr. Berzelius; and he has

considered himself as entitled, by the results which he has obtained, to establish two propositions which he considers as axioms or chemical first principles, and which have a prodigious influence on the whole doctrine. These axioms are the following :—

1. In all compounds of inorganic matter one of the constituents is always in the state of a single atom. According to this axiom, no inorganic compound is ever composed of two atoms of *a* united with three atoms of *b*, or of three atoms of *a* united with four atoms of *b*, &c. ; but always of one atom of *a* united with one, two, three, four, &c. atoms of *b*. This axiom, if it hold good, which Berzelius thinks it will, greatly simplifies the doctrine of atomic combination, as far as inorganic bodies are concerned, and reduces the whole to a state of elementary facility.

2. When an acid unites to a base, the oxygen in the acid is always a multiple of the oxygen in the base by a whole number, and generally by the number denoting the atoms of oxygen in the acid. Thus sulphuric acid contains three atoms of oxygen : 100 parts of it contain 60 oxygen ; and 100 parts of sulphuric acid combine with, and saturate, a quantity of base which contains 20 oxygen. Now 20 multiplied by three, the number of atoms of oxygen in sulphuric acid, makes 60 the quantity of oxygen in 100 of sulphuric acid.

Such are the two axioms of Berzelius, which he has made the foundation of his whole reasoning, and from which he has deduced his rules for determining the proportion of oxygen in bodies, and the number of atoms of which they are composed. If they hold good, and hitherto they have answered wonderfully well, they must be admitted to be of the utmost importance, and to give a facility and elegance to our chemical investigations which could scarcely have been looked for.

Mr. Dalton, the founder of the atomic theory, has not adopted either of these axioms. At the same time he has not advanced any fact in opposition to them ; but only that there is nothing in the atomic theory which necessarily leads to their adoption. This is doubtless true. The axioms are merely empirical, and deductions from analyses. Yet if they hold in all the analyses hitherto made, we cannot well refuse them a good deal of generality ; and the best mode of proceeding seems to be to admit them till some exception to them be discovered.

Berzelius, considering the atomic theory to labour under difficulties, which in the present state of our knowledge we are not able to surmount, has substituted in its place another, which he conceives to be easier and simpler. This may be called the theory of volumes. He conceives bodies to be all in the gaseous state, and embraces the opinion of Gay-Lussac, that gaseous bodies always unite in volumes that are aliquot parts of each other. One volume of one body always unites with one, two, three, &c. volumes of another. How this alteration, which consists merely in the substitution of the word *volume* for *atom*, simplifies the atomic theory, or

removes any of the difficulties under which it labours, is, I own, beyond my comprehension. But Berzelius has deserved so well of chemistry, that he may be indulged in any innocent whim which produces no deterioration.

I should take up too much room were I here to give a table of the weights of the atoms of bodies. I must satisfy myself with referring to the different papers which I have inserted in the *Annals of Philosophy* on the subject, to the paper of Berzelius in the third volume of the *Annals*, in which will be found his table of the weights of an atom of the simple substances, and to Dr. Wollaston's scale of chemical equivalents. The weights given in these three different tables do not always coincide with each other; but in general a very near approach to coincidence will be perceived. In some cases the weights that I have assigned are half those given by Berzelius. The reason of this is obvious; and the circumstance can occasion no difficulty or ambiguity.

## II. Light and Heat.

In the account of the progress of chemistry which I gave at the beginning of last year, I had to state a considerable number of important additions to the doctrine of heat, and its connection with light; but at present this department of the science is nearly barren. M. Berard has repeated and confirmed the experiments of Dr. Herschel on the heating power of the different rays of solar light. He found the greatest heating power at the extremity of the red ray. He likewise repeated the experiments of Wollaston, Ritter, and Böckman, on the deoxidizing power of the solar rays. He found it greatest, as they had done, in the violet ray; and traced it in a diminishing rate to the middle of the spectrum, where it disappeared.

Morichini, a Roman chemist, announced some time ago that, when steel needles are exposed to the action of the violet ray, they are converted into magnets. This experiment has been repeated in France, but has not been attended with success.

## III. Simple Supporters and Combustibles with their Compounds.

There are a considerable number of facts to relate respecting this branch of chemistry.

1. *Iodine*.—This singular substance was discovered some years ago by M. Courtois, a saltpetre manufacturer in Paris. It was first examined by Clement and Desormes, then by Sir H. Davy, and lastly by Gay-Lussac. It is obtained from kelp by a very easy process, which has been described in the *Annals of Philosophy*. French kelp yields it in much greater abundance than British kelp. Hence it would appear that the sea plants that yield it are more abundant in the English Channel than on the east or west coast of Britain. Iodine is in small crystals, which Dr. Wollaston has ascertained to be octahedrons. It has the metallic lustre, and resembles plumbago in colour, though its lustre is considerably

greater. It has a peculiar smell, is very volatile, and very poisonous in its nature when taken internally. Its specific gravity is rather less than four, that of water being one.

Iodine, as far as experiment has hitherto gone, must be considered as a simple substance; and it belongs to the class of supporters, though it is by far the worst supporter known. Its vapour supports the combustion of potassium, and it combines rapidly with phosphorus, evolving much heat, though no light. When iodine is heated, it is volatilized at rather a low temperature, and converted into a beautiful violet-coloured vapour, from which it has received its name. This vapour, as far as I can find, possesses very little elasticity at the temperature of  $212^{\circ}$ .

Iodine combines with chlorine, and forms a peculiar acid. It does not appear to combine with oxygen. With hydrogen it unites, and forms an acid very much resembling muriatic acid in its smell, though I consider it as rather more pungent than that of muriatic acid. It unites with sulphur, phosphorus, and the metals; and forms a class of bodies, analogous to the oxides, called *iodes*. Some of these possess the properties of acids. It combines with alkalis and earths, and forms with them two classes of salts. The first, consisting of iodine and the base, may be considered as analogous to the *iodes*; the second, consisting of iodine, oxygen, and the base, have been called *oxiodes*, and may be considered as analogous to the hyper-oxy muriates. It scarcely possesses the property of separating carbonic acid from the bases, and cannot therefore be united with the carbonates so as to form salts; though these bodies readily dissolve a portion of iodine. An atom of iodine weighs about 12.5.

2. *Chlorine*.—Chlorine has the property of combining with two different proportions of oxygen, and of forming two new acids, which have received the names of *chloric* and *chlorous* acids. The first was discovered by Gay-Lussac; the second, by Sir H. Davy. The Germans have given to chlorine the name of *halogen*.

It is scarcely worth while to notice the experiments of the Dutch chemists L. A. Von Meerten and S. Stratingh on this gas, as I do not perceive any thing new in them. Meerten says that chlorine has the property of converting sulphurous acid into sulphuric acid, and nitrous gas into nitric acid. These were the first two experiments that I tried; when Davy published his paper to show that chlorine is still an undecomposed substance; and I soon procured the fullest evidence that neither of these gases is altered by pure chlorine; but when chlorine contains a mixture of common air, which it usually does, it renders nitrous gas yellow in an instant. Meerten found likewise that ether burns in chlorine gas. This discovery was made many years ago by Cruickshank, and published by him in the last volume of Nicholson's quarto Journal, and by myself in the second and subsequent editions of my System of Chemistry; nor is there any thing new with respect to the burning of the metals in chlorine gas, as far as tried by these chemists.

Christian Frederick Bucholz made a set of experiments, in 1812, to determine the quantity of oxygen which can be obtained from hyper-oxy muriate of potash. His experiments were not attended with much success; but he ascertained that a red heat is necessary to drive off this gas. I have myself repeated this experiment more than once, and obtained a result which approached very near to that previously established by the experiments of Chenevix.

3 *Fluorine*.—Sir H. Davy has published several papers upon this hypothetical basis of fluoric acid; but all attempts to obtain it in a separate state have hitherto failed. Indeed, supposing it to exist, its action upon all other bodies seems necessarily to be so violent that there can be little hopes entertained of ever procuring it except in a state of combination.

4. *Azote*.—The two laws of Berzelius relative to chemical proportions do not hold when applied to the combination of azote with oxygen. The reason of this he conceives to be, that azote is not an element, but a compound of oxygen and an unknown base, to which he has given the name of *nitricum*. He has calculated from his theory the quantity of oxygen which azote must contain; and he shows that when this correction is made, the nitrates, as well as other bodies, come under the dominion of his two laws.

Mr. Miers, of London, had been of opinion for several years that azote is a compound of oxygen and hydrogen, and that the experiments of Girtanner were not so inaccurate as has been supposed. In a paper published in the *Annals of Philosophy*, vol. iii. p. 364, he shows that the supposition, that it is a compound of one atom oxygen and six atoms hydrogen, will tally exactly with the atomic theory, and give the weight of the different atoms into which azote entered the very same as they are at present, supposing azote to be a simple substance. This ingenious paper was sufficient to show us that the opinion of Mr. Miers was neither impossible nor improbable. It was not, however, sufficient to determine the opinions of chemists in favour of an hypothesis of so much importance, that the consequence of admitting it would be an almost entire change in the notions at present entertained respecting chemical combination.

Mr. Miers, sensible of the necessity of direct experimental proof in order to give currency to an opinion of such magnitude, has had recourse to direct experiment, and has published a very curious and valuable paper on the subject in the *Annals of Philosophy*, vol. iv. p. 180 and 260. His object in these experiments was to deprive water of a portion, but not the whole, of its oxygen, and thus to convert it into azote. The experiments of Girtanner were directed to precisely the same view. It occurred to Mr. Miers that sulphureted hydrogen gas would probably answer the purpose. Accordingly he passed a mixture of vapour of water and sulphureted hydrogen gas through a copper tube. In one experiment the whole gas that came over possessed the properties of common air, and was a mixture of 80 azote and 20 oxygen. In another a gas was formed, which Mr. Miers considered as sulphureted azotic gas. In a third

there was formed an acid gas resembling sulphureted hydrogen in smell, but possessing very different properties. Water absorbed twice its bulk of it. With potash it formed a black insoluble compound, not decomposed by any acid. Such were the different results obtained by Mr. Miers. They are highly curious and interesting; but it is obvious that they require to be followed farther before they can be considered as establishing the compound nature of azote, and that it is composed of oxygen and hydrogen. The inconsistency of the results with each other, the new facts brought to view by every repetition of them; and, above all, the circumstance of the gas, in the most decisive of all the experiments, possessing the properties and composition of atmospherical air, lead to suspicions which require to be obviated. It would be requisite, likewise, to account for the sulphur of the sulphureted hydrogen, and to show that the copper tube can have no influence on the decomposition of this gas. I hope, therefore, Mr. Miers will resume his experiments, and prosecute them till he obtains results free from all such anomalies, and leading to conclusions that cannot be controverted. The investigation is indisputably an object of importance; and he has made such progress in it, that he ought to enjoy the reputation that would infallibly result from so interesting a discovery.

5. *Phosphorus*.—The facts respecting phosphorus, published by Thenard in the *Annales de Chimie*, had been almost all anticipated long ago by Proust. Hence I conceive it to be unnecessary to detail them here. Heinrich, in his treatise on the phosphorescence of bodies, has stated some facts respecting the temperature at which phosphorus burns in various circumstances, which perhaps may be worth transcribing. When phosphorus is put into the bottom of a narrow glass tube, it may be heated to  $482^{\circ}$  without taking fire. In the open air phosphorus burns at  $99^{\circ}$ , and in oxygen gas at  $72^{\circ}$ . I cannot avoid observing that these determinations are far from precise. Indeed, nothing definite can be established respecting the combustion of phosphorus, because the degree at which it catches fire depends upon its purity. Pretty pure phosphorus I found did not begin to burn rapidly till heated to the temperature of  $148^{\circ}$ ; but if you keep it long in the temperature of  $99^{\circ}$ , its temperature gradually increases by its slow combustion, and it will, after a certain time, burn rapidly. According to Heinrich, a compound of equal parts phosphorus and sulphur becomes luminous at  $30^{\circ}$ .

6. *Ammonia*.—The important experiment of Berzelius, who converted mercury into an amalgam, by causing the galvanic battery to act upon it when in contact with ammonia, has not yet been cleared up in a satisfactory manner. It follows from it that ammonia contains a substance of a metallic nature as its base, and that when this base is deprived of oxygen by the influence of the galvanic battery the metal amalgamates with mercury. On the other hand, the analysis of ammonia by means of electricity, and the resolution of it into hydrogen and azote without the least trace of

oxygen, is equally decisive. These two sets of experiments seem at first sight incompatible with each other, and show us that there is something connected with the nature of azote still unknown. The fact that no substance constitutes a saline base, or is capable of uniting with acids, and forming salts, unless it contains oxygen (ammonia alone excepted), is a strong analogical argument in favour of the existence of oxygen in ammonia. It has accordingly induced Berzelius to embrace that opinion; and I must acknowledge that it is difficult to resist so very general an analogy. The anomaly respecting the composition of ammonia will be removed completely whenever it is proved in a satisfactory manner that azote is a compound of oxygen and hydrogen. This circumstance renders that question of still greater importance than it otherwise would be.

7. *Sulphuret of Carbon*.—The properties and constituents of this singular substance, as determined by Drs. Berzelius and Marcet, were stated in our sketch of chemistry given at the beginning of last year. Since that time Berzelius has published additional observations on the combination of sulphuret of carbon with the bases. (*Annals of Philosophy*, iii. 186.) To these compounds he has given the name of *carbo-sulphurets*. The following table exhibits the colour of the precipitates obtained by mixing different metallic salts with a solution of sulphuret of carbon in potash:—

Muriate of cerium . . . . .	White, or yellowish white.
Sulphate of manganese . . . .	Greenish-grey.
Sulphate of zinc . . . . .	White.
Permuriate of iron . . . . .	Dark red.
Submuriate of antimony . . . .	Orange.
Muriate of tin . . . . .	Pale orange, then brown.
Nitrate of cobalt . . . . .	Dark olive-green, at last black.
Nitrate of lead . . . . .	A fine scarlet.
Nitrate of copper . . . . .	Dark brown.
Promuriate of mercury . . . .	Black.
Permuriate of mercury . . . .	Orange.
Muriate of silver . . . . .	Reddish brown.

Dr. Brewster has found that sulphuret of carbon exceeds all fluid bodies in refractive power, and that in this respect it even surpasses flint glass, topaz, and tourmaline. In dispersive power it exceeds every fluid substance except oil of cassia, holding an intermediate place between phosphorus and balsam of Tolu.

8. *Potassureted Hydrogen Gas*.—Sementini, of Naples, published, about two years ago, a dissertation on potassium, in which he relates his experiments on potassureted hydrogen gas, which was discovered by Davy during his experiments on the metal of potash. The following are all the facts that I can find in these experiments: 1. Potassureted hydrogen gas is heavier than pure hydrogen, and lighter than phosphoreted hydrogen gas. 2. It takes fire, with a kind of explosion, when it comes in contact with the air emitting in alkaline odour. The explosion is louder when the gas comes in

contact with oxygen or chlorine gases. 3. Electricity causes it to expand, and throws down the greatest part of the potassium. 4. Though kept in contact with water, it does not lose the whole, but only a part, of its potassium. Hence Sementini concludes that hydrogen has the property of combining with two proportions of potassium.

9. *Nitrous Oxide*.—From the experiments of Professor Pfaff, of Kiel, it appears that when the nitrate of ammonia employed to prepare this gas contains sal ammoniac, as is commonly the case, the gas which comes over is the same as that noticed by Proust and Vauquelin, which has a peculiar taste and odour, and acts with violence upon the lungs. Hence the method of preventing the occurrence of this foreign gas is obvious.

9. *Sulphureted Azotic Gas*.—A controversy has been carried on in Germany for some years respecting the existence of this gas. It was first announced by Gimbernat as existing in the mineral waters of Aix-la-Chapelle. Then appeared the analysis of these waters by Dr. Reaumont, and by Messrs. Monheim and Lausberg; the experiments of Westrumb on the same subject; and the attempts of Berzelius and Hedenberg to form sulphureted azotic gas artificially, which were not attended with success. I do not consider it as necessary to enter into the particulars of this controversy. I shall merely state what I conceive to be the result of it. No person has ever succeeded in forming sulphureted azotic gas artificially. The waters of Aix-la-Chapelle seem to contain both azotic gas and sulphureted hydrogen gas. The sulphureted azotic gas of Gimbernat seems to have been nothing else than a mixture of these two gases.

#### IV. Metals.

The most indefatigable experimenter of late years on the metallic oxides is Professor Berzelius. For the greater part of his results I must refer to his dissertation On the Cause of Chemical Proportions, published in the third volume of the *Annals of Philosophy*, where almost the whole of his results will be found; but as the details of the experiments are frequently omitted in that dissertation, and as I am aware, from conversations which I have had on the subject with different young chemists, that some of these details would be very acceptable, I shall take the present opportunity of stating some of those which seem most important in this place.

1. *Oxides of Gold*.—The facility with which gold parts with its oxygen is well known, and every chemist is aware of the impossibility of obtaining the oxides of this metal in a state of purity. Berzelius' method of estimating the quantity of oxygen in the peroxide of gold, which alone can be subjected to direct examination, was very simple and ingenious, but required considerable dexterity. He dissolved a given quantity of gold in aqua regia, evaporated the solution gently to dryness, to get rid of the excess of acid, and then redissolved the salt in water. He now ascertained, with rigid accuracy, how much mercury was necessary to precipitate the gold in

the metallic state; for it is evident that this mercury must have united with exactly the quantity of oxygen which the gold contained. In one experiment, 14.29 of mercury precipitated 9.355 of gold; in another, 9.95 of mercury precipitated 6.557 of gold. According to these experiments, it appears that 15.912 of gold, when in the state of peroxide, contain just as much oxygen as 24.24 of mercury, when in the state of peroxide. But according to the experiments of Sefstrom, whom Berzelius follows, peroxide of mercury is composed of 100 mercury + 7.9 oxygen. Hence he reckons the peroxide of gold a compound of

Gold .....	89.225 .....	100.000
Oxygen .....	10.775 .....	12.077
	<hr/>	100.000

If we consider the red oxide of mercury as composed of 100 metal + 8 oxygen, as I have done in my table inserted in the second volume of the *Annals of Philosophy*, in that case we obtain the composition of peroxide of gold as follows:—

Gold .....	89.137 .....	100.000
Oxygen .....	10.863 .....	12.187
	<hr/>	100.000

This last statement agrees better with the experiments of Oberkampf than that of Berzelius. Oberkampf found sulphuret of gold composed of 100 gold and 24.39 sulphur. Now if an atom of sulphur weigh twice as much as an atom of oxygen, it follows that peroxide of gold ought to be composed of 100 gold + 12.195 oxygen.

When muriate of gold is exposed to a moderate heat upon a sand-bath, as long as it gives out chlorine gas it assumes a strong yellow colour, and becomes insoluble in water; or if the heat has not been continued long enough, only the portion of muriate of gold which is unaltered is dissolved, while that which has acquired a strong yellow colour remains undissolved. If heat be applied to this substance, or if it be simply exposed to the light of day, it is decomposed, and converted into metallic gold and permuriate of gold. According to Berzelius, the straw-coloured substance is a promuriate of gold. Heat or light deprives one portion of the protoxide of the whole of its oxygen, this oxygen unites with the remaining protoxide, and, converting it into peroxide, permuriate of gold is again formed. Now he found by experiment that in this case two-thirds of the gold are reduced to the metallic state, while one-third is converted into peroxide; so that the protoxide of gold contains just one-third of the oxygen in the peroxide.

I may just observe, that these conclusions are not quite free from objections. I myself think it not unlikely that no oxygen is present in either of these salts. This, however, does not hinder the calcu-

lations of Berzelius from approaching to accuracy; though it were to be wished that experiments on the oxides were made in a less exceptionable manner than by combining them with muriatic acid.

2. *Oxides of Platinum.*—To determine the composition of protoxide of platinum, Berzelius proceeded in the following manner. He dissolved a quantity of pure platinum powder in nitro-muriatic acid, evaporated the solution to dryness, to get rid of the excess of acid, and then exposed the salt upon a sand-bath till all evolution of chlorine was at an end. The salt thus treated had an olive-green colour, and was promuriate of platinum. To ascertain its composition, he decomposed it by a red heat. Ten parts of it thus treated left 7·33 of metallic platinum, while 2·67 parts of chlorine gas made their escape. Now he assumed that the chlorine gas is a compound of muriatic acid and oxygen, and that the 2·67 of it contain just the quantity of oxygen necessary to convert 7·33 parts of platinum into protoxide; but 100 parts of chlorine gas, according to Berzelius, are composed of 100 muriatic acid and 29·454 oxygen; therefore 2·67 contain 0·6075; and protoxide of platinum is composed of

Platinum .....	92·35 .....	100
Oxygen .....	7·65 .....	8·287

Berzelius determined the oxygen in the peroxide of platinum in the same way as he did that in the peroxide of gold. He ascertained how much mercury was necessary to precipitate a given quantity of platinum in the metallic state. The result was, that 100 mercury precipitate 48·23 of platinum. He therefore considers the peroxide of platinum as composed of

Platinum .....	85·93 .....	100
Oxygen .....	14·07 .....	16·38
		<hr/> 100·00

But if we reckon the peroxide of mercury to contain 100 metal + 8 oxygen, which I believe to be very near the truth, then peroxide of platinum is composed as follows:—

Platinum.....	85·773 .....	100
Oxygen.....	14·227 .....	16·587
		<hr/> 100·000

Now this gives the quantity of oxygen very nearly double that which was found in the protoxide.

3. *Oxide of Palladium.*—Berzelius could only find one oxide of palladium. He employed the same method to analyse it that succeeded with him in the analyses of the peroxides of gold and platinum. He ascertained how much mercury is necessary to decompose a given quantity of muriate of palladium, and reduce that

substance to the metallic state. He found that 100 mercury are equivalent to 55.6 of palladium. Hence he considers the oxide of palladium as composed of

Palladium .....	87.56 .....	100
Oxygen .....	12.44 .....	14.209
		<hr/>
		100.00

This will require a small correction, if we consider peroxide of mercury as composed of 100 metal and eight oxygen.

4. *Oxides of Rhodium*.—The experiments of Professor Berzelius on the oxides of this metal being detailed in the *Annals of Philosophy*, iii. 252, I shall here state merely the results which he obtained. He found three oxides of this metal: the first composed of one atom metal + one atom oxygen; the second, of one atom metal + two atoms oxygen; and the third, of one atom metal + three atoms oxygen; and one atom of rhodium weighs 14.903. Hence the oxides are composed respectively of

	Metal.	
Protoxide .....	100 +	6.71
Deutoxide .....	100 +	13.42
Peroxide .....	100 +	20.13

5. *Tungsten*.—There is hardly any metallic substance that occasions greater difficulties to practical chemists than tungsten, both on account of the want of a good process to obtain its peroxide in a state of purity, and on account of the very high temperature which is requisite to melt the tungsten after it has been reduced to the metallic state. The experiments of the El Luyarts upon wolfram are known to every chemist; as are likewise the elaborate experiments on the same metal of Vauquelin and Hecht. Messrs. Allan and Aikin succeeded in reducing it to the metallic state, and verified its great specific gravity as determined by the Spanish chemists. An elaborate set of experiments on this metal were published some years ago by Bucholz. He ascertained that the methods hitherto employed by chemists for procuring pure tungstate of ammonia do not succeed; and he verified the great specific gravity of this metal, having obtained it in grains of the specific gravity 17.4. Now this is the mean of 17.6 given by the El Luyarts, and 17.2 given by Allan and Aikin. But as I mean very speedily to publish a translation of the experiments of Bucholz in the *Annals of Philosophy*, I do not consider it as necessary to enter into farther particulars respecting them in this place.

6. *Platinum*.—The difficulty of reducing this metal to the malleable state, and its great importance in the construction of chemical vessels, are well known. Hitherto it has only been done for sale in Paris and in London. Parisian platinum is by far the dearest; as far as my experience goes, it serves very well for

chemical purposes. Its specific gravity is in general not so high as our London platinum, though I once had a crucible made in Paris above 21 in specific gravity. M. Leithner, who has the charge of the porcelain manufactory at Vienna, has lately proposed a new method of rendering platinum malleable. It is exceedingly simple, and appears to answer well enough in a small scale, though it is not adapted for the construction of large vessels. It consists in making up the fine powder of pure platinum into a paste with oil of turpentine, and laying it in coats upon paper, allowing one coat to dry before another is applied, and continuing to add coats till the layer of platinum is of sufficient thickness. When this is done upon porcelain, and the vessel afterwards exposed to the temperature of from  $14^{\circ}$  to  $18^{\circ}$  Wedgwood, the platinum adheres, and may be burnished. When laid upon paper, and then exposed to a strong heat gradually raised, a sheet of platinum remains, which may be hammered, and converted into any shape that is wanted.

7. *Palladium and Rhodium*.—Vauquelin's method of separating these metals from crude platina, and obtaining them in a state of purity, has been given so lately in the *Annals of Philosophy*, that I do not think it necessary to repeat it here. It is sufficiently complicated; and, as far at least as palladium is concerned, does not seem nearly so easy as the method previously given by Dr. Wollaston by means of prussiate of mercury, though it is possible that Vauquelin's process may yield a greater quantity.

8. *Gilding on Steel*.—Gehlen has tried the following method of gilding on steel, and found it to answer. The steel is to be in the first place polished; the part to be gilt is to be rendered rough by means of nitric acid; the steel is then to be dipped into the solution containing the gold; the gold adheres to the rough part of the steel, and may be burnished.

9. *Phosphuret of Copper*.—If we believe Dobereiner, phosphuret of copper, prepared by Sage's method, contains not only copper and phosphorus, but likewise calcium. To this last metal, in his opinion, the analogy of the phosphuret to steel is owing.

10. *Separation of Gold and Silver*.—Professor Schnaubert, of Moscow, has lately made several attempts to separate silver from gold by boiling the alloy in sulphuric acid; this acid dissolves the silver, and leaves the gold. The process, though by no means brought to a state of perfection, promises at present to be attended with success. The great difference between the price of sulphuric acid and nitric acid, which is usually employed for the purpose, renders it desirable that this process, which I conceive originated with Mr. Keir, should be subjected to farther trials.

11. *Zinc*.—From my analysis of *blende*, published in the *Annals of Philosophy*, iv. 89, we may conclude that oxide of zinc is composed of 100 metal + 24.42 oxygen; and sulphuret of zinc of 100 metal + 48.84 sulphur; and an atom of zinc weighs 4.095.

12. *Antimony*.—From my analysis of sulphuret of antimony, (*Ibid.* p. 95,) it follows that it is composed of 100 metal + 35.556

sulphur. If we suppose it a compound of one atom metal + two atoms sulphur, then an atom of antimony will weigh 11.249.

### V. Re-agents.

I shall state under this head the result of some experiments undertaken by different persons to determine the best re-agents for detecting the presence of different bodies in solution.

1. *Mercury*.—Professor Pfaff, of Kiel, has made a set of experiments on the best means of detecting mercury in solution, especially when in the state of corrosive sublimate. The following are the general conclusions which he considers himself as warranted to draw from these experiments:—

The experiments hitherto made on the action of sulphureted hydrogen on solutions of mercury, are in contradiction with each other, and of course insufficient to answer the purpose for which they were intended.

Water impregnated with sulphureted hydrogen is the most delicate test of the presence of corrosive sublimate and pernitrate of mercury; for it discovers these salts, though diluted with 40,000 times their bulk of water, and though they do not exceed the quantity of  $\frac{1}{10}$  or  $\frac{1}{100}$  part of a grain.

The action of this liquid upon solutions of mercury is distinguished from its action on all other metals by this circumstance—if there be present in the solution any peroxide of mercury, the precipitate, which is at first in brownish or blackish flocks, becomes very speedily white. The addition of the smallest quantity of peroxide of mercury will render the black precipitate white, provided it be agitated.

Protoxide of mercury in all cases produces a black precipitate with sulphureted hydrogen.

If a small piece of copper coin be put into the solution suspected to contain mercury, it will be covered with a white coating, or at least with white streaks; which, when rubbed, acquire the metallic lustre. By this method Mr. Pfaff was able to detect the presence of  $\frac{1}{10}$  of a grain of corrosive sublimate when dissolved in 20,000 times its weight of water.

2. *Muriatic Acid*.—Mr. Meyer, of Stettin, has made some curious observations on the delicacy of nitrate of silver, pronitrate of mercury, pernitrate of mercury, and the solution of subpernitrate of mercury in water, as tests for the discovery of muriatic acid. He found nitrate of silver the most delicate of these four salts. It detected one part of muriatic acid dissolved (in the state of common salt) in 118,664,000 parts of water; but upon this very dilute solution the other three salts had no effect. A solution of one part acid in 56,882,000 parts of water was not affected by the mercurial salts. A solution of one part muriatic acid in 28,416,000 parts of water was rendered slightly opalescent by the pronitrate of mercury, but not altered by the two other mercurial salts. A solution of one part of muriatic acid in 14,208,000 parts of water

was rendered opalescent, both by the pronitrate and pernitrate of mercury, but not by the third mercurial salt; but this third salt produced a slight opalescence in a solution of one part of muriatic acid in 3,552,000 parts of water.

3. *Arsenic*.—Some discussions have taken place in Germany respecting the best test for white arsenic. Gärtner observed that the formation of Scheele's green, by pouring cuprated ammonia into the liquid containing the white arsenic, does not in certain circumstances take place. Schweigger recommends sulphureted hydrogen, which is certainly a most delicate test of arsenic, in consequence of the fine yellow precipitate which it forms. The employment of nitrate of silver as a test for arsenic does not seem to be known in Germany. This test, first pointed out by Mr. Hume, but much simplified and improved by Dr. Marcet, is certainly very delicate; and, when the precautions suggested by Dr. Marcet are attended to, does not seem liable to ambiguity.

4. *Manganese*.—Pfaff has published a set of experiments on the method of separating manganese from iron, and has shown that all the methods hitherto proposed on the Continent are imperfect. Bergman's process has been long given up by all chemists. Vauquelin's, by means of bicarbonate of potash, he found likewise unsuccessful. The method by means of tartrate of potash is incomplete, because the tartrate of potash-and-manganese is soluble in water. Dr. John's method, by oxalate of ammonia, was likewise unsuccessful. Nor did he succeed better by means of the succinates, benzoates, or phosphates. Mr. Hatchett's method of separating the iron, by means of ammonia, was not tried. I find that by means of it I can very easily procure perfectly pure oxide of manganese. Whether it would succeed equally in forming an accurate separation of manganese and iron for the purposes of analysis, I have not tried; though I think it probable that it might be used for that purpose with advantage, supposing iron and manganese to be the only substances in the solution upon which we operate.

5. *Iron*.—Mr. Porrett has recommended triple prussiate of potash, or *ferrureted chyazate of potash*, as he calls it, as the best method of throwing down iron, and ascertaining its quantity. The precautions necessary to be attended to are the following. The ferrureted chyazite must be pure. There must be no excess of acid in the solution, or as little as possible. All other substances precipitated by the ferrureted chyazate must be previously removed. The ferrureted chyazate must not be added in excess, or as little so as possible. The solution being boiled, and the Prussian blue separated and weighed, every hundred parts of it must be considered as equivalent to 34.235 of peroxide of iron in the solution.

I have no doubt that when no manganese is present the ferrureted chyazate may be employed with considerable accuracy to detect the quantity of iron in any substance under analysis; but as it most frequently happens in mineral analysis that iron and manganese are

mixed together in unknown proportions, ferrureted chyzate of potash cannot be employed in such cases till a good method is discovered of separating manganese from iron. Gehlen's method is the best hitherto proposed; though it only answers when the whole of the iron is in the state of peroxide.

I may mention here, for the sake of our British manufacturers of sulphuric acid, that E. W. Martius announced in 1811, in a German journal, that he had found white arsenic in a glass carboy of English sulphuric acid. The arsenic had separated from the acid, and formed a crust in the inside of the glass. As the notice merely states the fact of the separation of white arsenic, I conceive it possible that Martius might have been mistaken. If his statement be accurate, the arsenic must have made its way into the carboy by some odd accident or other; for I think it hardly possible that it should have been mixed with the sulphur before its combustion.

## VI. *Acids.*

1. *Formic Acid.*—The reader is probably aware that this acid, which exists ready formed in red ants, was originally discovered by Ray and Fisher; that it was first accurately examined by Margnauf; that Arvidson and Oern published a very complete set of experiments on it in 1777. It continued to be considered as a peculiar acid till Fourcroy and Vauquelin published a set of experiments on it in 1803, and drew as a conclusion from them that it is not a peculiar acid, but a mixture of the acetic and malic acids. This dissertation convinced all the French chemists; and induced me in the second edition of my System of Chemistry to expunge it from the list of acids. But Suerzon having published a new set of experiments on it, in 1805, showed that the premises of the French chemists were insufficient to warrant their conclusions, and that formic acid possessed striking and essential properties which distinguished it from acetic acid. I suggested in my System the propriety of a more rigid examination of the *formates* than had hitherto taken place. This seems to have induced Gehlen to undertake a laborious set of experiments on the subject. He observes in the outset, that if I had been acquainted with the experiments of Arvidson and Oern, and of Richter, on the *formates*, I should probably have been satisfied, without requiring any farther proofs. I regret that it has never been in my power to peruse either of the works alluded to by Gehlen. My knowledge of the experiments of Arvidson and Oern is derived from the account of them given by Keir in his Dictionary, and by Bergman in his Treatise on Elective Attractions. Gehlen's experiments are quite decisive; but they are, unfortunately, too long to be detailed here. He prepared, in the first place, formate of copper, from which he separated the formic acid by distilling it with sulphuric acid in a retort. He compared the pure formic acid thus obtained with acetic acid, procured from acetate of lead by a similar process.

Thus prepared, it has an acid, and peculiar taste and smell, quite different from that of acetic acid. When cooled down sufficiently, it becomes solid, but does not crystallize. Its specific gravity was 1.1168; and, when diluted with its own weight of water, the specific gravity becomes 1.060; and when with twice its weight of water, it becomes 1.0296. In all these respects acetic acid is very different. It likewise requires different proportions of bases to neutralize it. Gehlen describes minutely the formates of copper and barytes, and compares them with the acetates of the same bases. They differ in colour, solubility, form of crystals, and all their other properties, from each other.

2. *Ferrureted Chyazic Acid*.—Mr. Porrett has discovered two new acids, and rendered it probable that many more exist. Ferrureted chyazic acid is obtained from the salt formerly called *triple prussiate of barytes*. This salt is dissolved in water, and as much sulphuric acid added as is just sufficient to neutralize the barytes present. The mixture, being agitated in a phial, is set aside for some time. Sulphate of barytes precipitates, and the ferrureted chyazic acid remains in solution in the liquid. Its properties are as follows:—

It has a pale lemon colour; but no smell. It is decomposed by a gentle heat, or by exposure to a strong light. Prussic acid is then formed, and white triple prussiate of iron, which is soon changed into Prussian blue. It separates acetic acid from all its combinations. It combines with the different bases, and forms the salts formerly called triple prussiates. This acid is composed of four constituents; namely, black oxide of iron, carbon, hydrogen, and azote; or perhaps it would be as well to consider it as composed of five constituents, oxygen, iron, carbon, hydrogen, and azote. It would be a curious, but very difficult problem, to determine the proportions in which these different constituents are united in this complicated acid. How many atoms of each enters into it. From the great permanency of this acid when compared with some of the vegetable and animal acids, one would be disposed to suspect that its composition is very simple. Perhaps an atom of each constituent would not be very far from the truth.

According to Mr. Porrett, ferrureted chyazate of potash is composed of

Black oxide of iron .. 17.26	} Ferrureted chyazic acid .. 47.66
Prussic acid ..... 30.40	
Potash .....	39.34
Water .....	13.00
	<hr/> 100.00

This analysis cannot be reconciled with the supposition of ferrureted chyazic acid being a compound of an atom of each of its constituents. If we suppose the salt composed of an atom of acid and an atom of base, as is usual with the salts of potash, then an

atom of the acid would weigh only 7.268, which is considerably less than an atom of each of the constituents, which together weigh 10.829. If, on the other hand, we take the proportion of black oxide of iron and prussic acid as a criterion, the atom of acid ought to weigh 22.485, and it might be a compound of one iron + one oxygen + six carbon + five azote + six hydrogen; besides which many other numbers might be chosen. These anomalies render Mr. Porrett's analysis of the ferrureted chyazate of potash somewhat doubtful.

Ferrureted chyazate of barytes he found composed of

Ferrureted chyazic acid .....	34.31
Barytes .....	49.10
Water .....	16.59
	<hr/>
	100.00

Ferrureted chyazate of iron is composed of

Black oxide of iron ..19.33	} Ferrureted chyazic acid..	53.380
Prussic acid.....34.05		
Peroxide of iron .....		34.235
Water .....		12.385
		<hr/>
		100.000

3. *Sulphureted Chyazic Acid*.—Mr. Porrett discovered this acid in 1808, by boiling three or four parts of Prussian blue in powder with one part of sulphuret of potash, and a sufficient quantity of water. The new acid is gradually formed, and neutralizes the potash in the solution. Various other processes were attended with success. To obtain the acid from this solution in a state of purity, Mr. Porrett employed the following method:—Add sulphuric acid till the liquid acquires a decidedly sour taste: then keep it for some time nearly at the boiling point. When cold, add to it a little black oxide of manganese, which will turn it to a beautiful crimson colour. Filter the liquid, and add to it a solution containing two parts of sulphate of copper and three parts of prosulphate of iron, till the crimson colour disappears. A copious white precipitate falls, composed of protoxide of copper combined with sulphureted chyazic acid. Boil the precipitate in a solution of potash, which separates the acid, and leaves the oxide of copper. Mix the potash solution with sulphuric acid, and distil. The sulphureted chyazic acid comes over into the reservoir. It is still mixed with some sulphuric acid, from which it may be freed by carbonate of barytes.

The acid thus obtained is colourless, has a strong smell, analogous to that of acetic acid, and its specific gravity is 1.022. At a boiling temperature it dissolves a little sulphur. This acid consists of two-thirds of its weight of sulphur and one-third of the constituents of prussic acid. The salts which it forms have the following properties:—

Sulphureted chyzate of potash : a deliquescent salt, soluble in alcohol.

- \_\_\_\_\_ of soda : ditto, crystallizes in rhombs.
- \_\_\_\_\_ of lime : ditto, soluble in alcohol, from which it may be obtained in needle-form crystals.
- \_\_\_\_\_ of ammonia : ditto, not crystallizable.
- \_\_\_\_\_ of magnesia : ditto, when dried has a mica-ceous appearance.
- \_\_\_\_\_ of alumina : crystallizes in octahedra, which do not deliquesce.
- \_\_\_\_\_ of barytes : a deliquescent salt, crystallizing in long slender prisms of a brilliant white. It is composed of

Sulphureted chyzic acid .....	30.1
Barytes .....	69.9
	<hr/> 100.0

- \_\_\_\_\_ of strontian : a deliquescent salt, crystallizing in long slender prisms in groups radiating like zeolite.
- \_\_\_\_\_ of silver : a white insoluble powder.
- \_\_\_\_\_ of mercury : a white insoluble powder.
- \_\_\_\_\_ of potash and prussiate of mercury : a brilliant silvery lustre ; very soluble in hot, and little soluble in cold, water.
- \_\_\_\_\_ of protoxide of copper : a white powder, insoluble in water. Its constituents are,

Sulphureted chyzic acid .....	36.855
Protoxide of copper .....	63.145
	<hr/> 100.000

- \_\_\_\_\_ of peroxide of copper : a bright pea-green liquid.
- \_\_\_\_\_ of protoxide of lead : a soluble salt in obtuse rhombs.
- \_\_\_\_\_ of protoxide of iron : a colourless and very soluble salt.
- \_\_\_\_\_ of peroxide of iron : a beautiful crimson salt, very deliquescent.

The sulphureted chyzates of tin, bismuth, manganese, zinc, cobalt, nickel, palladium, uranium, molybdenum, and chromium, are very soluble.

4. *Fuming Sulphuric Acid*.—The singular qualities of the fuming sulphuric acid manufactured at Nordhausen, in Germany, from green vitriol, have long attracted the attention of chemists, and various solutions of the anomalies which it presents have been given. Fourcroy's explanation of it, by affirming that it is a mixture of sulphuric and sulphurous acids, has been pretty generally

succeeded to ; yet it appears that it is not the true one. Mr. Vogel, apothecary at Bayreuth, has lately published a very elaborate set of experiments on this acid, which, as far as they go, appear decisive. The following are the facts which he has established by his experiments : Fuming sulphuric acid contains no sulphurous acid ; nor can it be formed by uniting these two acids together, nor by distilling a mixture of sulphur and sulphuric acid. Fuming sulphuric acid attracts no oxygen, nor does it produce any alteration on atmospherical air. When mixed with water, it is converted into common sulphuric acid. When combined with bases, it forms common sulphates. It dissolves some sulphur, and acquires a brown, green, or blue colour, according to the proportion of sulphur which it holds in solution. It combines, likewise, with phosphorus. Vogel considers it as common sulphuric acid united with some imponderable substance, and brought by its means to a more powerful acid state. The direct consequence from his experiments seems to be, that it is sulphuric acid free from water. There is, however, a fact stated by Dobereiner, in a very long dissertation which he has published on the action of the different kinds of sulphuric acid on nitric acid, which, if accurate, would require an explanation. He says that when a mixture of fuming sulphuric acid and nitric acid is heated, the nitric acid is decomposed into nitrous gas and oxygen gas, but no such change is produced by heating a mixture of common sulphuric acid and nitric acid. I do not perceive very clearly how this fact was ascertained. Supposing nitrous gas and oxygen gas to be evolved together out of the liquid, they could not be collected, for they would instantly combine, and form nitrous acid ; but supposing the fact correct, I can conceive it to be owing to this circumstance. The fuming sulphuric acid deprives the nitric acid of the whole of its water. Now in this state it is probably much more easily decomposed than when it contains water. Common sulphuric acid will not produce this effect so completely, because it is already combined with an atom of water.

5. *Prussic Acid*.—Mr. Bergeman, apothecary in Berlin, discovered, in 1811, that the bark of the *prunus padus* contained a notable quantity of prussic acid. Water distilled from this bark proved fatal to animals when taken internally.

6. *Acetic Acid*.—The following very extraordinary experiment was made by Nasse, one of the members of the Imperial Academy of St. Petersburg, and is related by him in a letter to Professor John. Take a glass vessel and fill it with a mixture of equal bulks of carbonic acid gas and common air, and put into it a little water so as hardly to cover the bottom of the vessel. Stop it up, and lay it aside for some months, shaking it occasionally. Then open it, and leave it for some weeks with the mouth slightly covered. Acetic acid will be perceived formed in it, both by the taste and smell. Nasse obtained his carbonic acid by the action of dilute sulphuric acid on Carrara marble. Here is the formation of acetic acid without the presence of any animal or vegetable substance: nothing else

than carbonic acid, common air, and water. The experiment would deserve a careful repetition. If correct, how much light would it not throw on the nature of acetic acid?

7. *Arsenious Acid*.—Various and discordant statements have been published respecting the solubility of white arsenic in water. The result of Klaproth's trials was lately published in the *Annals of Philosophy*. Since that time a still more elaborate set of experiments on the same subject has been published by Bucholz. His results agree more nearly with those of Klaproth than with any other; though there are several anomalies in his experiments which are sufficiently puzzling. I have long been of opinion that the white oxide of arsenic exists in two states; namely, in the state of pure oxide, and in the state of hydrate of arsenic. When first prepared, it is transparent and colourless, like glass; but it gradually becomes white and opaque, and puts on the appearance of enamel. The glass I conceive to be the pure oxide of arsenic; the enamel, to be a hydrate. Now if this opinion be well founded, we may expect to find a difference in the solubility of white arsenic in these two states. I think it probable that several of the anomalies are owing to chemists not having hitherto attended to this difference of state.

## VII. Salts.

This is always one of the most prolific departments of chemistry, on account of the great number of salts, and the importance of being acquainted with their properties; but this historical sketch has already swelled so much, that I shall omit all the salts treated of in the *Annals of Philosophy* during the last year.

1. *Calomel*.—Mr. Jewel's improvement in the manufacture of calomel, by making it pass in the state of vapour into water, is known, I presume, to most of my readers; having been made known to the public by Mr. Luke Howard, in whose manufactory it took place, about four years ago.

2. *Oxalates*.—Vogel, of Bayreuth, has published two very elaborate, and I conceive very accurate, sets of experiments on the analysis of several of the oxalates. I cannot attempt in this place to do more than give a bare table of his results. What makes these experiments more valuable, is their agreement with the views of Berzelius respecting the composition of salts; though when Vogel made his experiments (at least the set of them first published,) it does not appear that he was acquainted with Berzelius' opinions on the subject.

A hundred parts of oxalic acid require for saturation a quantity of base which contains 21.2 parts of oxygen; or in other words, in the neutral oxalates the acid contains three times as much oxygen as the base.

When binoxalate of potash is poured upon carbonate of copper, a solution takes place, and two salts are formed, distinguished from

each other by their crystalline shape; the one crystallizing in needles, the other in tables. The constituents of the first of these salts are,

Peroxide of copper .....	20.50
Potash .....	25.04
Oxalic acid .....	36.46
Water .....	18.00
	<hr/>
	100.00

The constituents of the second are,

Peroxide of copper .....	22.5
Potash .....	27.0
Oxalic acid .....	40.5
Water .....	10.0
	<hr/>
	100.0

Thus they differ in their water of crystallization; the one containing double the quantity of the other.

Oxalate of lime is composed of	{ Acid .....	49.5	56.25
	{ Lime .....	38.5	43.75
	{ Water .....	12.0	
		<hr/>	100.00
		100.0	

Oxalate of potash .....	{ Acid .....	43.06	43
	{ Potash .....	56.77	57
	{ Water .....	0.17	
		<hr/>	100
		100.00	

Binoxalate of potash .....	{ Acid .....	55.93	64.02
	{ Potash .....	31.44	35.98
	{ Water .....	12.63	
		<hr/>	100.00
		100.00	

Oxalate of soda-and-copper ....	{ Acid .....	46.48	
	{ Soda .....	19.02	
	{ Peroxide of copper .....	23.50	
	{ Water .....	11.00	
		<hr/>	100.00

Dry oxalate of soda .....	{ Acid .....	54.77	
	{ Soda .....	45.23	
		<hr/>	100.00

Foliated oxalate of ammonia- and-copper .....	{	Acid .....	47·5	
		Ammonia .....	10·5	
		Peroxide of copper ...	25·0	
		Water .....	17·0	
				100·0
Effloresced oxalate of ammo- nia-and-copper .....	{	Acid .....	36·00	
		Ammonia .....	16·29	
		Peroxide of copper ...	39·00	
		Water .....	8·71	
				100·00
Pulverulent oxalate of ammo- nia-and-copper .....	{	Acid .....	43·00	
		Ammonia .....	9·72	
		Peroxide of copper ...	45·58	
		Water .....	1·70	
				100·00
Sulphate of potash- and-copper .....	{	Acid .....	72·15	36·075
		Potash .....	42·85	21·425
		Peroxide of copper ...	36·00	18·000
		Water .....	49·00	24·500
			200·00	100·000

3. *Chromates*.—Dr. John has published a paper on the chromates, in which he describes the properties of twelve chromates hitherto unknown, or nearly so. As this paper is short, and cannot well be abridged, I have inserted a translation of it in the last number of the *Annals of Philosophy*, to which I refer the reader.

4. *Sal-ammoniac*.—Before concluding this department, I may take the opportunity of mentioning that Mr. Trimmer informs me that the sand employed for mixing with the clay by the London brick-makers is brought from below Woolwich. It probably, therefore, contains some common salt, derived from the sea water with which it is washed. This may be the source of the muriatic acid which goes to the formation of the sal-ammoniac that sublimes during the burning of the bricks; but if this be the case, Nature employs during this process some method of decomposing common salt at present unknown to manufacturers; but which it might be well worth their while to endeavour to trace experimentally, unless we suppose the whole acid to come from the muriate of magnesia.

#### VIII. Composition of Alcohol and Ether.

M. de Saussure has given us a new, and seemingly very accurate,

analysis of alcohol and sulphuric ether. The constituents of alcohol are as follows :—

Carbon .....	51.98
Oxygen .....	34.32
Hydrogen .....	13.70
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	100.00

This result seems to show that alcohol is composed of

	Atoms.
Oxygen .....	1
Carbon .....	2
Hydrogen .....	3
	<hr/>
	6

An atom of it, according to this statement, would weigh 2.898. This composition is very simple, considering that the number of constituents of alcohol amounts to three.

The constituents of sulphuric ether are,

Carbon .....	67.98
Oxygen .....	17.62
Hydrogen .....	14.40
	<hr/>
	100.00

This result seems to show that sulphuric ether is composed of

	Atoms.
Oxygen .....	1
Carbon .....	5
Hydrogen .....	6
	<hr/>
	12

So that, if the analysis of Saussure be correct, sulphuric ether contains twice as many atoms in its composition as alcohol. The weight of an integrant particle of it, according to this statement, would be 5.547.

### IX. Analysis of Waters.

Bouillon-Lagrange and Vogel have published an elaborate analysis of the sea-water that washes the different coasts of France. The following table exhibits the results of their analyses :—

	Weight.	Residue of Evapo- ration.	Carbonic Acid Gas.	Common Salt.	Muriate of Mag- nesia.	Sulphate of Mag- nesia.	Carbonates of Lime and Mag- nesia.	Sulphate of Lime.
	Gram.	Gr.	Gr.	Gr.	Gr.	Gr.	Gr.	Gr.
Water of the Channel.....	1000	36	0.23	25.10	3.50	5.78	0.20	0.15
Water of the Atlantic.....	1000	38	0.23	25.10	3.50	5.78	0.20	0.15
Water of the Mediterranean.	1000	41	0.11	25.10	3.25	6.25	0.15	0.15

Berzelius has published an analysis of a very complicated mineral water in Stockholm. Its constituents per Swedish kanne are as follows :—

	Grains.
Muriate of soda .....	21 $\frac{1}{2}$
Nitrate of potash .....	18
Sulphate of potash .....	$\frac{1}{7}$
Nitrate of lime .....	24
Nitrate of magnesia .....	4 $\frac{3}{4}$
Carbonate of lime .....	15
Sulphuric acid .....	7
Carbonate of magnesia .....	1 $\frac{1}{4}$
Carbonate of iron .....	$\frac{3}{7}$
Silica .....	$\frac{3}{7}$
Extractive matter and loss .....	$\frac{3}{7}$
	<hr/>
	92 $\frac{3}{4}$

According to the analysis of Mr. Funke, apothecary at Linz, on the Rhine, the mineral water of Tönnestein, on the Rhine, contains the following constituents in five pounds weight of it :—

	Grains.
Carbonate of iron .....	$\frac{1}{2}$
Carbonate of lime .....	45
Carbonate of soda .....	36 $\frac{1}{2}$
Muriate of soda .....	4 $\frac{3}{4}$
Sulphate of soda .....	4
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	90 $\frac{1}{2}$

Five pounds weight of the mineral water of Heilbrunn contains, according to the same analyst,

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	Grains.
Carbonate of soda .....	54
Sulphate of soda .....	6½
Muriate of soda .....	24
Carbonate of lime .....	55½
Carbonate of magnesia .....	2
Carbonate of iron .....	1
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The same chemist obtained the following substances from five pounds of the mineral water of Obermenning :—

	Grains.
Carbonate of soda .....	4
Muriate of soda .....	3½
Sulphate of soda .....	4
Carbonate of lime .....	10
Carbonate of iron .....	4
	<hr/>
	25½

Finally, the mineral water of Heppingen gave the same chemist the following ingredients. He operated, as before, upon five pounds of the water :—

	Grains.
Carbonate of iron, could not be weighed.	
Carbonate of lime .....	6½
Carbonate of soda .....	31
Carbonate of magnesia .....	12
Sulphate of soda .....	10½
Muriate of soda .....	15
	<hr/>
	75

### X. Vegetable Substances.

The field of vegetable chemistry is so vast, and still so imperfectly explored, that it is always a very prolific department. I must at present confine myself within as narrow limits as possible, and avoid indulging reflections even when they obtrude themselves upon me.

1. *Thibet Caoutchouc*.—This is a reddish elastic substance, which is used as beads in the Mediterranean. I have seen strings of beads repeatedly brought to this country from Malta, and once made a few trials on one of these beads, sufficient to show that it was a vegetable substance, and that it had some resemblance to the oils in its chemical properties. It has been particularly examined by John and Bucholz. Neither alcohol, ether, nor oils, dissolve it completely, though they produce some effect on it. Potash ley dissolves

it, and an acid separates it from the solution in the state of an oil. It dissolves in sulphuric and nitric acids. When heated, it does not melt; but it soon loses its red colour; it seems to me to be very analogous to linseed oil boiled to dryness. Nothing is known respecting its natural history. It is probably a natural production. It is said to be much employed in Thibet. Whether what is used in the Mediterranean comes from Thibet I do not know.

2. *Cajeput Oil*.—This oil has lately acquired considerable celebrity in some parts of England, as a most effectual remedy for rheumatism when applied externally to the diseased part by friction. It was first made known in Europe by the Dutch. According to Murray, it was first brought to Holland about the beginning of the eighteenth century. In 1719 it made an article in the *Materia Medica* in some of the German Pharmacopœas. It is doubtless an essential oil. Dr. Roxburgh has lately laid a very accurate account of the tree which yields this oil before the Linnæan Society. He cultivated these trees at Calcutta for about ten years; so that the doubts still remaining respecting the nature of the plant from which this oil comes may be considered as removed.

3. *Sugar from Starch*.—Kirchhoff's discovery of the method of converting starch into sugar was no sooner known in Germany than it occasioned a great many publications on the subject, and numerous attempts to improve the process, and render this new sugar a substitute for common sugar. These attempts were natural at a time when the mistaken policy of Bonaparte had shut out Europe from all access to foreign countries, and precluded the supply of common sugar, except at an enormous price. I cannot find, however, that any of these attempts were attended with success; or that much additional chemical information was even added to Kirchhoff's original discovery. This discovery was owing to an accident. In consequence of the war between Great Britain and Russia, the Russians found it difficult to procure gum. Kirchhoff's object was to render starch a substitute for gum. He thought that he would improve it by boiling it in weak sulphuric acid. He gradually lengthened the boiling process, in expectation of rendering the starch more completely gum. The result was, that it acquired a sweet taste, and the properties of sugar. Schrader has shown that by increasing the quantity of sulphuric acid the length of time necessary for boiling may be shortened. Thus five or six parts of sulphuric acid to 100 parts of starch require only six or eight hours boiling. Nasse found that nitric acid and muriatic acid produce this change on starch as well as sulphuric acid. Starch sugar is not so sweet as common sugar; but I have seen it as white, and very like common loaf sugar in appearance.

4. *Sap of the Acer Campestre*.—In the year 1811 Professor Scherer, of Vienna, examined the sap of the *acer campestre*, or common maple, with a view, I presume, to the sugar which might be extracted from it. This sap, when in small quantities, was

colourless, like water : but when collected in greater abundance, it was imperfectly transparent, and had a milky appearance. It had a sweet taste, and its specific gravity varied. It contained an albuminous substance, which was precipitated in flocks by heating the liquid. There was likewise another substance which precipitated in flocks, that seemed to be different from the albumen ; but the most remarkable constituent was a salt, which crystallized sometimes in small prisms, sometimes in plates. It was white, translucent, and had little lustre. 1000 parts of cold water dissolved nine parts of this salt; 1000 parts of boiling water dissolved 17 parts. This salt was composed of lime and a peculiar vegetable acid, bearing some resemblance to the moroxylic acid of Klaproth. Scherer calls it *maple acid*. We might distinguish it, if it be a peculiar acid, by the name of *aceric acid* ; for Scherer's name, *feldahorn acid*, cannot with propriety be adopted into other languages.

5. *Conium Maculatum*.—Schrader has subjected *conium maculatum*, common hemlock, to a chemical analysis. The following are the ingredients which he obtained from a thousand grains in weight of the plant :—

Extractive .....	27·3
Gummy extract.....	35·2
Resin .....	1·5
Albumen .....	3·1
Green fecula .....	8

He obtained also the following salts from the same quantity of the plant :—

Phosphate of lime .....	46
Phosphate of magnesia .....	30·1
Carbonate of lime .....	27·4
Carbonate of magnesia .....	20·2
Carbonate of potash .....	115·0
Sulphate of potash .....	10·8
Muriate of potash .....	4·2

6. *Brassica Oleracea Viridis*.—To Schrader we are indebted likewise for a chemical analysis of the *brassica oleracea viridis*, a variety of the sea cabbage. From 1000 parts of this plant he obtained the following substances :—

Extractive .....	23·4
Gummy extract .....	28·9
Resin .....	0·5
Albumen .....	2·9
Green fecula .....	6·3

The salts which 1000 parts of this plant yielded were the following :—

Phosphate of lime .....	71·4
Phosphate of magnesia .....	8·2
Carbonate of lime .....	40·1
Carbonate of magnesia .....	35·6
Carbonate of potash .....	107·0
Sulphate of potash .....	73·2
Muriate of potash .....	15·2

7. *Gums*.—Dr. John has published the result of his experiments on some varieties of gum which exude spontaneously from different trees in Germany.

*a. Plum-gum*.—The variety of plum known by the name of *Myrobala*, which is yellow and round, when pricked by insects or chopped, gives out a white clear sap, which gradually hardens into gum. The constituents of this gum are,

Gum arabic .....	12·5
Cerasin .....	87·5
	<hr/>
	100·0

By *cerasin* Dr. John means a soft gelatinous matter which remains undissolved when the gum is treated with water. It is equally insoluble in alcohol; but water acidulated with sulphuric acid dissolves it in a boiling temperature. This substance has been observed by Vauquelin, and by other chemists. It exists in the cherry-tree gum of this country.

*b. Gum from the Stem of the Prunus Avium*.—Its constituents, according to John, are,

Cerasin .....	80
Gum .....	20
	<hr/>
	100

Besides some traces of lime and potash in combination with an acid.

*c. Sweet Cherry-Tree Gum*.—Its constituents are as follows:—

Gum .....	97
Phosphate of lime .....	} 3
Lime combined with a vegetable acid ..	
Potash combined with a vegetable acid ..	
	<hr/>
	100

I do not exactly know what Dr. John means by the *sweet cherry*. The sweetest cherry which I know is the small black cherry which grows wild abundantly in some parts of Scotland, and in that country is called *geen*. The gum from this tree I have examined, and know that it contains a portion of Dr. John's *cerasin*. His *sweet cherry*, therefore, probably is the fruit of another tree.

8. *Starch*.—Some years ago Bouillon Lagrange made the observation that when starch is roasted it acquires the property of dissolving in cold water. Professor Döbereiner, of Jena, has published a set of

experiments on this subject. I perused them with pleasure, because I found them very similar to a set of experiments which I made myself on the same subject ten years ago, part of which were published in Nicholson's Journal, and part in a Report on Malting, Brewing, and Distilling, printed by order of the House of Commons; but as this Report can scarcely be said to be published, as it was never on sale, the facts contained in it have never become generally known.

Döbereiner found that when starch was roasted till its colour became grey, it was only partially soluble in cold water, and the solution was nearly colourless; but when roasted till it became yellow, it dissolved completely in cold water, and the solution was dark coloured. Both these solutions exhibited the same properties when examined by re-agents.

They were precipitated by alcohol. Infusion of nutgalls produced a copious precipitate, which was readily dissolved on heating the liquid, and appeared again when the liquid cooled. Barytes water likewise occasioned a precipitate, which was completely soluble in acetic acid. It was precipitated also by nitrate of mercury; but not by silicated potash, permuriate of iron, nitrate of silver, or corrosive sublimate; nor by nitrate of lead, hydro-sulphuret of potash, or alum. It slowly decomposes nitric acid. When treated with very dilute sulphuric acid, it is not converted into sugar. It does not readily ferment.

Döbereiner likewise made observations on the quantity of starch still to be found in beer; on the difference between the starch of raw grain and of malt. I have not room here to state the particulars; but know from previous experience that the facts which he states are tolerably accurate.

9. *Cucumber*.—Dr. John has subjected the cucumber to a chemical analysis. The following were the constituents which he procured from 600 grains of it:—

Water .....	582.80
Matter similar to the fungin of mushrooms ..	3.20
Albumen .....	0.80
Resin .....	0.25
Extractive with a sweet substance .....	9.95
Gluten .....	} 3.00
Phosphate of lime .....	
Phosphate of potash .....	
Phosphoric acid .....	
A salt with base of ammonia .....	
Malic acid united to a base .....	
Sulphate of potash .....	
Muriate of potash .....	}
Phosphate of iron .....	
Aroma .....	

---

600.00

600 parts of the husk of the cucumber contain 90 parts of dry matter; the constituents of which bear a resemblance to those of the interior part.

10. *Potatoes*.—Lampadius has made comparative experiments on four different varieties of potatoe; namely, the Peruvian potatoe brought from America by Humboldt, and planted in Germany; the English potatoe; the onion potatoe;\* and the Voichtland potatoe. The following were the proportions of the different constituents yielded by 100 lbs. of the respective sorts of potatoe.

1. *Peruvian Potatoe.*

	lbs.	oz.
Starch .....	15	0
Fibrous matter .....	5	8
Albumen .....	1	28
Extractive .....	1	28
Water .....	76	0
	<hr/>	
	100	0

2. *English Potatoe.*

	lbs.	oz.	dr.	gr.
Starch .....	12	20	1	8
Fibrous matter .....	6	26	3	4
Albumen .....	1	1	1	2
Extractive .....	1	22	2	4
Water .....	77	16	1	48
	<hr/>			
	100	0	0	0

3. *Onion Potatoe.*

	lbs.	oz.	dr.	gr.
Starch .....	18	24	0	0
Fibrous matter .....	8	12	0	0
Albumen .....	0	28	0	0
Extractive .....	1	21	1	2
Water .....	70	10	2	58
	<hr/>			
	100	0	0	0

4. *Voichtland Potatoe.*

	lbs.	oz.	dr.	gr.
Starch .....	15	13	1	2
Fibrous matter .....	7	4	0	0
Albumen .....	1	8	0	0
Extractive .....	1	30	2	4
Water .....	74	8	0	54
	<hr/>			
	100	0	0	0

\* I do not know the variety which is known in Germany by the name of *Zwiebel Kartoffel*.—T.

It is not easy to say what kind is meant by the name of English potatoe. There are many varieties in England, differing very much in their properties. In general the potatoes raised in the north of England are better than those raised in the south; though to this rule there may be exceptions.

### XI. *Animal Substances.*

Dr. Gordon's experiments showing the evolution of heat during the coagulation of the blood (*Annals of Philosophy*, iv. 139,) constitute a pleasing fact, because they confirm the general law of the evolution of heat when bodies change from a liquid to a solid form. Dr. Prout's farther experiments on the quantity of carbonic acid evolved by respiration at different times of the day, and as affected by various circumstances, and the confirmation they have received from the recent experiments of Dr. Fife, are deserving of the attention of physiologists, and are well calculated to throw additional light on this still mysterious function, so essential to life, that it cannot be interrupted; yet, as far as we can find, serving merely to remove a little carbon from the blood.

1. *Air in the Swimming Bladders of Fishes.*—All chemical readers are acquainted with Biot's experiments to determine the composition of the air in the swimming bladders of fishes, published in the *Memoirs d'Arcueil*, and with the unexpected result that the proportion of oxygen in these bladders increases with the depth at which the fish live. Since the publication of that paper an elaborate set of experiments on the same subject has been published by Configliachi, an Italian Professor. He has confirmed and extended the facts ascertained by his predecessors. One of the most curious parts of his paper is a table of the proportion of oxygen gas found by him in sea-water of different depths. The following is a copy of that table:—

Depth in Metres.	Proportion of Oxygen.
50 .....	28·7
100 .....	28·8
150 .....	28·5
200 .....	27·9
250 .....	28·4
300 .....	28·7
350 .....	29·0
400 .....	28·5
450 .....	27·8
500 .....	28·1
550 .....	28·4
600 .....	28·3
650 .....	28·3
700 .....	28·2

2. *Urine.*—Professor Wurzer has published a set of experiments

on a remarkable urine emitted by a man of the age of 33. He had been afflicted with a gonorrhœa virulenta ever since the age of 24. In consequence of cold, to which he had been exposed, his breasts swelled, and a few days after he passed the remarkable urine which drew the attention of Professor Wurzer. It was milk-white, and contained in it a notable quantity of a matter which possessed exactly the properties of *curd*.

3. *Magnesia in Human Bones*.—Fourcroy and Vauquelin first discovered the presence of magnesia in the bones of inferior animals; but they could detect none in human bones. For this they assigned a physiological reason. In man the magnesia is carried off in the urine; but this is not the case in the inferior animals. Hence it appears in the bones of the latter, but not of the former. Berzelius repeated the experiments of Fourcroy and Vauquelin, and could not by their method detect any magnesia in human bones; but he gives another process, by which he assures us he detected it. Professor Hildebrandt, of Erlangen, has lately resumed this subject, and repeated the experiments of Fourcroy and Vauquelin. Like them, he could detect no magnesia in human bones, and therefore concludes that Berzelius was mistaken. Now I must acknowledge that this mode of proceeding appears very extraordinary. To repeat a process, which, Berzelius himself admits, does not yield any magnesia, and then to conclude that Berzelius is mistaken because the experiment turns out just as he foretold it would. To refute Berzelius' conclusion, it would be necessary to repeat the experiment as he describes it, or to show experimentally that the experiment is incapable of repetition.

4. *Urinary Calculus of a Horse*.—A urinary calculus of a horse analysed by Professor Wurzer was found to contain the following constituents:—

Carbonate of lime .....	66
Phosphate of lime .....	20·05
Carbonate of magnesia .....	4·06
Red oxide of iron .....	0·005
Animal matter .....	9·885
	<hr/>
	100·000

5. *Milk*.—In 1813 C. F. Schwarz published an inaugural dissertation at Kiel on the analysis of milk. From 1000 parts of cow's milk he obtained the following substances:—

Phosphate of lime .....	1·805
Phosphate of magnesia .....	0·170
Phosphate of iron .....	0·032
Phosphate of soda .....	0·225
Muriate of potash .....	1·350
Lactate of soda .....	0·115
	<hr/>
	3·697

1000 parts of human milk contain,

Phosphate of lime .....	2·500
Phosphate of magnesia .....	0·500
Phosphate of iron .....	0·007
Phosphate of soda .....	0·400
Muriate of potash .....	0·700
Lactate of soda .....	0·300
	<hr/>
	4·407

Such is as concise a sketch as I have been able to draw up of the improvements which chemistry has undergone of late years, chiefly in those countries with which we have had the least intercourse. I have omitted some important facts altogether, because I intend to insert translations of the papers containing them in the succeeding numbers of the *Annals of Philosophy*, as speedily as I can find room for them.

## II. MINERALOGY.

This branch of science is divided into two parts; namely, Oryctognosy and Geognosy. It will be convenient to take each separately.

### I. Oryctognosy.

Under this head I include the description and analysis of minerals.

1. *Native Carbonate of Magnesia*.—The carbonate of magnesia from India, of which I gave an account in the *Annals of Philosophy*, iv. 155, deserves to be mentioned, because it agrees nearly in its composition with the conite of the Germans, though it differs from it in its external characters. It is composed of

Carbonate of magnesia .....	72
Carbonate of lime .....	28
	<hr/>
	100

This does not differ much from a compound of two integrant particles of carbonate of magnesia and one integrant particle of carbonate of lime.

2. The mineral called haüyne is hitherto scarcely known in Britain. It is an inhabitant of volcanic rocks; and has hitherto been found only in Italy, Auvergne, and at Andernach. According to Gmelin, (*Annals of Philosophy*, iv. 193,) its constituents are,

Silica .....	35·48
Alumina .....	18·87
Sulphate of lime .....	21·73
Lime .....	2·66
Oxide of iron .....	1·16
Potash .....	15·45
Water .....	1·20
Sulphureted hydrogen and loss .....	3·45
	<hr/>
	100·00

My analysis of asbestous actinolite (*Annals of Philosophy*, 19,) gives a great number of constituents, and on that account very satisfactory. I do not mean to say that the numbers are inaccurate; for the experiments were carefully made, and of them repeated three times; but they lead to the suspicion the specimen was contaminated with foreign bodies; yet to the was pure, and consisted entirely of a congeries of crystals. I the mineral composed as follows:—

Silica .....	33·4
Alumina .....	28·2
Lime .....	1·046
Magnesia .....	0·6
Oxide of iron .....	17·15
———— manganese .....	7·2
Tungstic acid ? .....	3·84
Copper .....	1·0
Soda ..	3·8
Moisture .....	1·7
Loss .....	2·064
	<hr/> 100·000

*Green Granular Actinolite.*—This is a mineral which occurs in a vein at Pacher-Alps, near Marburg, in amorphous form, where it constitutes a rock with grey and red quartz. It has been confounded with smaragdite or diallage; but Werner has separated it from this mineral. The following, according to Werner, are the distinguishing characters of the two species:—

<i>Green Smaragdite.</i>	<i>Green Granular Actinolite.</i>
Color, pearly.	Lustre, glassy.
Structure foliated, cleavage single.	Foliated, cleavage double.
Fracture uneven.	Cross fracture splintery.
	Semihard.
Is brittle.	Brittle.

Werner found the specific gravity of green granular actinolite to be 2·8. It was composed of the following constituents:—

Silica .....	56
Magnesia .....	18·5
Lime .....	15·5
Alumina .....	3·25
Oxide of iron .....	4·75
———— chromium .....	1·
———— manganese, a trace.	
Loss .....	1·
	<hr/> 100·00

5. *Schorl*.—Professor Bernhardt being of opinion that common schorl, and several other species of minerals, might be constituted into a genus, to which he proposed to give the name of tourmaline, requested Professor Bucholz to make an analysis of common schorl to serve as a sample of this new genus. Bucholz accordingly made three very careful analyses, two of schorl from St. Gotthard, and one of schorl from Tyrol. The result was as follows:—

Silica .....	35·000
Alumina .....	31·500
Magnesia .....	5·938
Lime .....	0·062
Oxide of iron .....	6·125
Silica containing iron .....	0·125
Manganese, a trace.	
Water .....	5·000
Potash .....	1·333
Loss .....	14·917
	<hr/>
	100·000

This loss he was not able to account for. He could neither find any more alkali, nor any volatile matter whatever. The most probable substance seems to be fluoric acid. I do not perceive that he sought for it particularly.

6. *Arragonite*.—The discovery that arragonite contains about four per cent. of carbonate of strontian, recently made by Professor Stromeyer, is of considerable importance, by removing an anomaly which hitherto had existed in mineralogy; namely, the same chemical composition with a difference of external characters. It shows us also how much analysts ought to be on their guard, and how easy it is for the most skilful experimenters to overlook ingredients that have a marked effect upon the external characters of minerals.

6. *Schorlous Beryl*.—This mineral was analysed by Bucholz in 1804, who detected in it 17 per cent. of fluoric acid. Since that time Vauquelin (Ann. de Chim. lxvii. 247,) and Klaproth (Beitrag, v. 57,) analysed this mineral. The former found five per cent. of fluoric acid; the second, four per cent. This great discordance induced Bucholz to repeat his experiments, and his new analysis agrees very nearly with his former one. He found the constituents as follows:—

Silica .....	35
Alumina .....	48
Oxide of iron .....	00·5
Water .....	1
Fluoric acid .....	16·5
	<hr/>
	101·0

*Prehnite*.—Two varieties of prehnite have been analysed by Gehlen: the first, from Fassuthale, had a specific gravity of 2·917; the second, from Ratschinkes, was 2·924. The first variety was composed of

Silica .....	42·875
Alumina .....	21·50
Lime .....	26·50
Oxide of iron .....	3·00
———— manganese .....	0·25
Magnesia, a trace.	
Volatile matter .....	4·625
Soda .....	0·27
Loss .....	·98
	<hr/>
	100·000

The constituents of the second variety were,

Silica .....	43·00
Alumina .....	23·25
Lime .....	26·00
Oxide of iron .....	2·00
———— manganese .....	0·25
Magnesia, a trace.	
Volatile matter .....	4·00
Loss .....	1·5
	<hr/>
	100·00

Another mineral found in a dolomite rock at Hafnerszell, which Gehlen considered as a variety of prehnite, was likewise analysed by Gehlen.

This mineral is amorphous. Its colour is light greenish-grey passing into yellow. It appears foliated, or rather radiated, and exhibits a two-fold cleavage parallel to the faces of a four-sided angular prism. Its fracture is splintery; its specific gravity, 2·6. It gives sparks with steel, scratches glass, but is itself scratched by quartz. It is difficultly frangible. Lustre pearly, considerable. Translucent. Phosphoresces when heated. Becomes weakly electric by heat. Its constituents were,

Silica .....	54·50
Alumina .....	25·25
Lime .....	10·05
Magnesia .....	3·25
Oxide of iron .....	1·00
———— manganese, a trace.	
Soda .....	5·94
	<hr/>
	99·99

*Conite*.—The mineral called *conite*, described by Schumacher,

from Iceland, and by different other mineralogists, seems to me to be very nearly connected, if not quite the same, with what I have called native carbonate of magnesia from India; though its external characters, according to the following description, are not quite the same. This difference I ascribe to the absence of carbonate of iron in the Indian mineral.

Conite has a light flesh-red colour. It is amorphous, and externally covered with a coating of iron ochre. Fracture sometimes fine-grained, uneven; sometimes imperfectly conchoidal. No lustre; scratches glass; opaque; brittle; sp. gr. 3·000.

Its constituents, according to the analysis of Dr. John, are as follows:—

Carbonate of magnesia .....	67·5
— lime .....	28·0
— iron .....	3·5
Water .....	1
Sulphate of lime ? a trace.	

100·0

9. *Zeolite*.—Haiiy's division of the zeolite into stilbite and mesotype is well known; and likewise his union of the natrolite with the mesotype. Gehlen analysed two specimens of each of these genera. The following was the result:—

	Stilbite.		Mesotype.	
Silica .....	55·072	55·615	53·392	54·40
Alumina .....	16·584	16·681	19·620	19·70
Lime .....	7·584	8·170	1·750	1·61
Soda with some potash ..	1·500	1·536	14·696	15·09
Water .....	19·300	19·300	9·710	9·63
	100·040	101·302	99·168	100·63

Gehlen has rendered it probable that the mineral analysed by Vauquelin under the name of mesotype pyramidée, and even the minerals tried by Haiiy before the blow-pipe, were not mesotypes, but stilbites. We see from the preceding analysis that the two minerals constitute two distinct species, well marked by the proportion of their constituents. Stilbite contains twice the water in mesotype. Mesotype contains much less lime, but much more alkali.

10. *Boracite*.—About five years ago boracite was found in abundance by Professor Steffens, in a gypsum mountain near Segeberg, in Holstein. The crystals are very small, and consist either of perfect cubes, or of cubes with their angles truncated. According to the analysis of Professor Pfaff, this boracite is composed of

Boracic acid .....	6
Magnesia .....	3½
Oxide of iron .....	1½
Silica .....	½
Loss .....	1½
	<hr/>
	11

Thus it is free from lime, and therefore verifies the previously received opinion that boracite is essentially a *borate of magnesia*.

11. *Vitreous Black Oxide of Iron*.—This is a mineral mentioned by Haüy in his *Tableau Comparatif*, p. 98 and 274, as a black vitreous substance found in the department of the Lower Rhine. It scratches glass, and has a specific gravity of 3·2. When heated to redness, it becomes magnetic. Vauquelin found it composed of oxide of iron 80·25, water 15, silica 3·75. Bucholz has lately analysed a small quantity of this mineral. He found its composition as follows:—

Oxide of iron .....	68·5
Silica .....	10·0
Oxide of manganese .....	5·5
Loss .....	16·0
	<hr/>
	100·0

If we consider this loss as water, which seems from the analysis of Vauquelin to be the case, the mineral will be a hydrate of iron and manganese united with silica.

12. *Sulphuret of Cobalt*.—This mineral occurs in Sweden, at Nya Bastnäs, near Riddarhyttan. Hisinger found its constituents as follows:—

Cobalt .....	43·20
Copper .....	14·40
Iron .....	3·53
Sulphur .....	38·50
Earthy matter .....	0·33
	<hr/>
	99·96

13. *Native Yellow Oxide of Lead*.—This rare mineral has been lately examined by Dr. John. Its characters are the following:—

External colour, between sulphur and lemon-yellow; internal, between honey-yellow and Aurora red.

Amorphous.

Fracture, earthy. In some places there is a tendency to the foliated fracture.

External lustre, dull; internal, semi-metallic.

Opake.

Semihard.

Brittle. Moderately easily frangible.

Specific gravity 8·000.

Its constituents are as follows :—

Lead .....	82·6923
Carbonic acid .....	3·8462
Oxide of iron and lime ....	0·4808
Copper, a trace.	
Silica mixed with iron ....	2·4039
Oxygen .....	10·5768
	<hr/>
	100·0000

14. *Supposed Native Minium from Kall, in the Roer Department.*—I am not sure whether this be the native minium of Mr. Smithson, as I have not his dissertation at hand. Its characters are as follows :—

Colour, brownish-red. Amorphous.

Fracture fine-grained, uneven.

External lustre, dull; internal, glimmering.

Opake; soft; easily frangible; sp. gravity, 4·000.

Dr. John found its constituents as follows :—

Lead .....	44·15
Carbonic acid .....	10·00
Water .....	4·00
Lime and oxide of iron .....	0·50
Insoluble matter { Silica .....	29·00
{ Alumina .....	5·25
{ Oxide of iron .....	3·00
Oxygen .....	4·10
	<hr/>
	100·00

By comparing this analysis with the preceding, it is obvious that the oxide in this mineral is not the red, but the yellow oxide of lead, and that it owes its red colour to the oxide of iron which it contains. The proportion of oxygen is obviously over-rated in both analyses.

15. *Spinell.*—A specimen of spinell, from Oker, in Sudermanland, in Sweden, analysed by Berzelius, gave the following constituents :—

Alumina .....	72·25
Silica .....	5·48
Magnesia .....	14·63
Oxide of iron .....	4·26
Water .....	1·83
Loss .....	1·55
	<hr/>
	100·00

16. *Red Silicious Ore of Manganese.*—A specimen of this ore from Longbanshyttan, in Wermeland, in Sweden, analysed by Berzelius, was composed of

Black oxide of manganese . . .	52·60
Silica . . . . .	39·60
Oxide of iron . . . . .	4·60
Lime . . . . .	1·50
Water . . . . .	2·75

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101·25

The excess of weight was owing to the manganese in the ore containing less oxygen than the black oxide, in which state it was obtained by analysis.

17. *Sodalite*.—Ekeberg analysed a mineral from Hefselkulla, in the province of Nerike, in Sweden, which, from his description of it, seems to have some resemblance to sodalite; but its constituents differ materially. It occurs in an iron-mine mixed with quartz. Its colour is greenish-grey; lustre, pearly and inconsiderable; principal fracture foliated with a two-fold cleavage; cross fracture granular, uneven; translucent on the edges; it scratches glass, but is scratched by steel; difficultly frangible; specific gravity 2·746. Its constituents were,

Silica . . . . .	46
Alumina . . . . .	28·75
Magnesia . . . . .	13·50
Oxide of iron . . . . .	0·75
Water . . . . .	2·25
Soda . . . . .	5·25
Loss . . . . .	3·50

---

100·00

18. *Black Garnet*.—A specimen of black garnet from the iron-mine of Svappavara, in Torneo Lappmark, analysed by Hisinger, gave the following constituents:—

Silica . . . . .	34·53
Lime . . . . .	24·36
Alumina . . . . .	1·00
Oxide of iron . . . . .	36·05
Volatile matter . . . . .	0·50
Loss . . . . .	3·56

---

100·00

19. *Sparry Iron Ore*.—A specimen of this mineral, from Rid-darhyttan, in Wermeland, in Sweden, analysed by Hisinger, was composed of

Red oxide of iron . . . . .	63·25
Oxide of manganese . . . . .	3·00
Lime . . . . .	1·00
Carbonic acid . . . . .	30·00
Water . . . . .	1·75

---

99·00

20. *Scapolite*.—A specimen of scapolite, from Sudermanland, in Sweden, analysed by Berzelius, was composed of

Silica .....	61·50
Alumina .....	25·75
Lime .....	3·00
Magnesia .....	0·75
Oxide of manganese .....	1·50
— iron .....	1·50
Water .....	5·00
	<hr/>
	99·00

21. *Cerite*.—This mineral, according to the last analysis of Hisinger, is composed of

Oxide of cerium .....	68·59
Silica .....	18·00
Lime .....	1·25
Oxide of iron .....	2·00
Water and carbonic acid ....	9·60
	<hr/>
	99·44

22. *Spodumene*.—Spodumene, from Utön, according to the same analyst, is composed of

Silica .....	63·40	67·50
Alumina .....	29·40	27·00
Oxide of iron .....	8·00	3·00
Lime .....	0·75	0·63
Volatile matter .....	0·53	0·53
Loss .....	2·92	1·34
	<hr/>	<hr/>
	100·00	100·00

The last of these analyses is by Berzelius.

23. I shall now subjoin a table of a number of Swedish minerals analysed by Hisinger :—

Lepidolite from Utön ....	{	Silica .....	61·60
		Alumina .....	20·61
		Lime .....	1·60
		Oxide of manganese .....	0·50
		— iron, a trace.	
		Potash .....	9·16
		Volatile matter .....	1·86
		Loss .....	4·67
			<hr/>
			100·00

Malacolite, from Longbanshyttan .....	{	Silica .....	54.18
		Lime .....	22.72
		Magnesia .....	17.81
		Oxide of iron .....	2.18
		———— manganese .....	1.45
		Volatile matter .....	1.20
			99.54
Serpentine, from Bojmosmine, in Norberg .....	{	Magnesia .....	37.24
		Silica .....	32.00
		Lime .....	10.60
		Alumina .....	0.50
		Oxide of iron .....	0.60
		Volatile matter .....	14.16
		Loss .....	4.90
			100.00
A black mineral from the iron mine of Gillinge, in Sudermanland. Sp. gr. 3.045 ..	{	Oxide of iron .....	51.50
		Silica .....	27.50
		Alumina .....	5.50
		Oxide of manganese .....	0.77
		Magnesia, a trace.	
		Volatile matter .....	11.75
Loss .....	2.98		
			100.00
A violet stone from Borkhult. Sp. gr. 2.8 .....	{	Silica .....	46.40
		Alumina .....	29.00
		Lime .....	17.14
		Oxide of iron .....	0.70
		Volatile matter .....	3.20
		Loss .....	3.56
			100.00
Bitter spar from Ljusnedal, in Herjedal .....	{	Lime .....	29.8
		Magnesia .....	21.6
		Oxide of iron .....	1.0
		Carbonic acid .....	47.6
			100.0
Mealy zeolite from Fahlun ..	{	Silica .....	60.
		Alumina .....	15.60
		Lime .....	8.00
		Oxide of iron .....	1.80
		Volatile matter .....	11.60
		Loss .....	3.00
			100.00

Iridescent iron ore from Grengesberg .....	{	Red oxide of iron .....	94.38
		Phosphate of lime .....	2.75
		Magnesia .....	0.16
		Stoney matter .....	1.25
		Volatile matter .....	0.50
			<hr/> 99.04

24. *Swine-stone*.—Hisinger and Berzelius have analysed various specimens of Swedish swine-stone. The following are the results of their labours :—

their labours :—	
Transparent swine-stone from Garphyttan, in Nerike ....	{ Carbonate of lime ..... 99.1 _____ magnesia ... _____ manganese.. } 0.9 _____ iron ..... }
<hr/> 100.0	

Black sparry swine-stone from the same place .....	{	Carbonate of lime .....	95.0
		_____ manganese ..	1.5
		_____ magnesia ..	
		_____ iron .....	
		Alum-slate and iron pyrites ..	3.5
			<hr/> 100.0

Prismatic swine-stone .....	{	Carbonate of lime .....	98.6
		_____ magnesia ...	0.9
		_____ manganese..	
		_____ iron .....	
		Alum-slate .....	.05
			<hr/> 100.0

Prismatic swine-stone from Kinnekulle .....	{	Carbonate of lime .....	97.25
		_____ manganese..	1.25
		_____ magnesia ..	
		_____ iron .....	
		Alum-slate .....	1.50
			<hr/> 100.00

Gehlen has proposed two alterations in the present mode of analysing minerals ; and, in his analysis of prehnite, he has shown that they may be employed with advantage. 1. He substitutes *carbonate of soda*, instead of caustic alkali, for the original fusion of the mineral in a platinum crucible. He found that this method answers even in the analysis of corundum. 2. He substitutes carbonate of barytes for nitrate of barytes when our object is to obtain the fixed alkali which we suppose to exist in any mineral. He has found that this carbonate readily acts as a flux to minerals, and that it answers better than the nitrate of the same earth.



Fig.1.

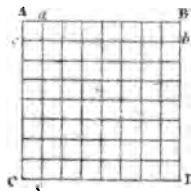
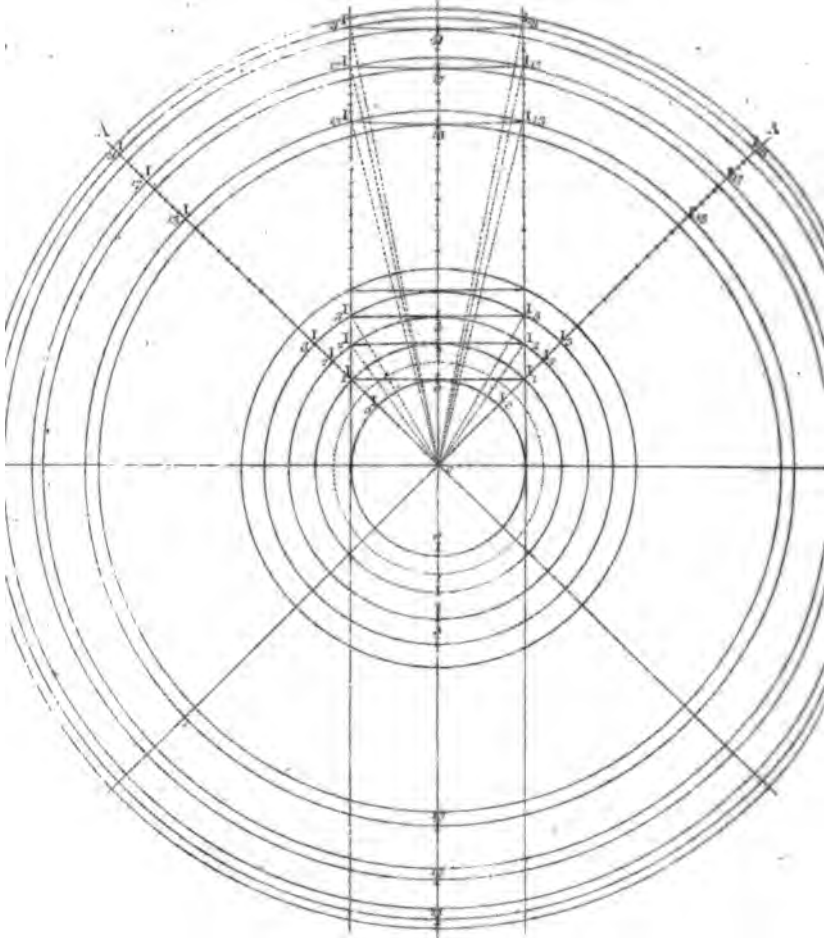


Fig.2

## II. *Geognosy*.

I ought now to give a sketch of the recent improvements in this branch of science, which of late years has become a fashionable object of study in Great Britain ; but I have already extended this article to such an enormous, and I fear improper length, that I must, however reluctantly, stop short here. The great object of geognosts on the Continent at present seems to be to trace to their utmost extent the formations discovered in the neighbourhood of Paris ; and to extend as much as possible the transition formations in those countries hitherto considered as primitive. In this country we have no fewer than three geological societies, the Wernerian, the London, and the Cornwall. The first two have recently published each a volume of Transactions. I shall give an analysis of each of these books as speedily as possible. They contain almost all the important geological facts that have been lately ascertained in Great Britain.

## ARTICLE II.

*Solution of a Problem of Col. Silas Titus. (See Wallis's Algebra, Chap. 60.)* By the Abbé Buée.\*

SIR,

HAVING for many years considered the different algebraical methods for the solution of arithmetical problems by approximation to be deficient in their fundamental principles, I have been led to mistrust the whole science of algebra as generally taught, and am convinced that if we place an implicit faith in it we shall be involved in the most revolting absurdities. Pell's problem (see Wallis's Algebra, chap. 60, 62, &c.), and all those which can only be resolved by approximation, are examples of this kind. The absurdity belonging to the solution of these sort of problems is to represent numbers of which we know not the fundamental unity. In speculation this absurdity is not felt ; but we easily perceive it when we quit speculation, and are engaged in questions respecting real beings, such as men. In this case the solution gives for units fractions as much smaller as the approximation is farther extended. If, then, the real unit be a man, the solution gives for unit a fraction of a man, which goes on always diminishing, and by that means becomes more and more absurd. In general the *speculative*

\* The following curious solution of a well-known problem was sent by the Abbé Buée to a mathematical gentleman in London, who declines communicating his name to the public. Though I do not participate in the Abbé's objections to algebraic approximation, yet I conceive the solution of the problem itself to be so curious as to be well entitled to the attention of mathematicians ; and on that account I agreed without hesitation to insert it in the *Annals of Philosophy*. The letter which serves as an introduction to the problem is written by the Abbé Buée.—T.

unit is the QUOTIENT of a number divided by itself, while the real unit is the product of a number multiplied by the inverse of that number. Let  $n$  be any number,  $n$  divided by  $n$  is a speculative unit, and  $n \times \frac{1}{n}$  is a real unit, as a rectangle would be. If, then,  $\frac{1}{n}$  be the supposed unit, it is necessary, in order to obtain a real unit, to multiply  $\frac{1}{n}$  by  $n$ ; consequently, the nearer we approach one side, the farther we recede on the other.

The following solution, which for the first time is given of Pell's problem, is the only one exempt from this absurdity:—

*Problem.*

The following equations being proposed, viz.

$$a^2 + b c = 16 \dots (1)$$

$$b^2 + a c = 17 \dots (2)$$

$$c^2 + a b = 18 \dots (3)$$

To find  $a, b, c$ , (See Wallis's Algebra, chap. 60, 62, &c.) let there be a series of concentric circles, (Plate XXVII. fig. 1.)  
 $\begin{array}{cccccccccccccccccccc} 1 & 1 & 1, & 1 & 1 & 1, & 1 & 1 & 1 \dots 1 & 1 & 1 \dots 1 & 1 & 1 \dots 1 & 1 & 1, & 1 & 1, \\ 0 & 0 & 0 & 1 & 1 & 1 & 2 & 2 & 2 & 13 & 13 & 13 & 17 & 17 & 17 & 21 & 21 & 21 \end{array}$

them be so described that we have  $\bigcirc 1 = \begin{array}{ccc} 1 & 1 & 1 \\ 0 & 0 & 1 \end{array} = \begin{array}{ccc} 1 & 1 & 1 \\ 1 & 2 & 3 \end{array} =$

&c. = 1.

If we make  $a =$  the sector  $\bigcirc \begin{array}{ccc} 1 & 1 & 1 \\ 13 & 13 & 13 \end{array} \dots (4)$

$b =$  the sector  $\bigcirc \begin{array}{ccc} 1 & 1 & 1 \\ 17 & 17 & 17 \end{array} \dots (5)$

$c =$  the sector  $\bigcirc \begin{array}{ccc} 1 & 1 & 1 \\ 21 & 21 & 21 \end{array} \dots (6)$

Then if the areas of these sectors be substituted for  $a, b, c$ , in the equation (1), (2), (3), according to the following method, their differences will reduce these equations to identical ones.

*Demonstration.*

This demonstration is founded upon a remarkable property of the concentric circles of this figure. This property is, that the areas of each of the rings intercepted between two consecutive circumferences are equal to the area of the central circle. If we take the area of the central circle for an unit, the areas of each of the rings will be = 1.

To prove this, let  $\bigcirc 1$  be the radius of the central circle, we may

easily perceive that the radii of the successive circles are the hypothenuses of right angled triangles, whose sides are,

1. The radius of the preceding circle. 2. A constant tangent equal to the radius of the central circle. The series of radii will be then expressed by  $\bigcirc 1 (\sqrt{1}, \sqrt{2}, \sqrt{3}, \sqrt{4}, \&c.) \dots (7)$

Now the circumferences of circles are constantly proportional to their radii: if, then, we designate by  $2\pi$  the ratio of the circumference to its radius, the series of circumferences will be expressed by the progression  $(\bigcirc 1)^2 \times 2\pi [\sqrt{1}, \sqrt{2}, \sqrt{3}, \sqrt{4}, \&c.] \dots (8)$

But the areas of circles are as the squares of their radii; so that the series of circles will be expressed by the progression  $(\bigcirc 1)^2 \cdot 2\pi$

$[1, 2, 3, 4, \&c.] \dots (9)$

In this series (by taking away the common factor  $(\bigcirc 1)^2 \cdot 2\pi$ ) 1

expresses the area of the central circle; 2, 3, 4, &c. express those of the successive circles. If we take the difference of each of the areas of these consecutive circles, we shall have the areas of the rings. Now these differences are constantly equal to 1; consequently the areas of the rings are = 1.

It follows from the above conclusions that if we reckon the central circle for the first ring, the series of rings will be expressed by the common *ordinals*, 1st, 2d, 3d, 4th, &c. .... (10) while the series of circles are expressed by the *absolute* numbers 1, 2, 3, 4, &c. .... (11)

These ordinal numbers follow the *direct* or *inverse* order: when they follow the *direct* order, the areas of the rings are *positive*; when they follow the *inverse* order, they are *negative*. These areas are constantly = 1, and are represented by the equation

$$+1 = e^{\pm 2k\pi\sqrt{-1}} \dots (12)$$

( $k$  being any positive whole number.)

$$\text{And the negative areas by } -1 = e^{\pm (2k+1)\pi\sqrt{-1}} \dots (13)$$

These equations, which are fundamental, I thus demonstrate: in every system of logarithms the logs. are exponents, and *these exponents are ordinal numbers*, because they are the indexes of the terms of a geometrical progression whose first term is 1. Now from the principles demonstrated by Euler (Introductio in Analysin Infinitorum, Cap. VIII. No. 39,) we may prove the truth of the two following equations:—

$$\text{Log. } (+1) = \pm 2k\pi\sqrt{-1} \dots (14)$$

$$\text{Log. } (-1) = \pm 2(k+1)\pi\sqrt{-1} \dots (15)$$

(See Lacaille's *Leçons de Mathématiques*, Nos. 833, 834, 835.)

Let  $e$  be the base of the hyperbolic logarithm: we have  $1 = \log. e \dots (16)$

$$\text{Consequently } \log. (+1) = \pm 2k\pi\sqrt{-1} \log. e \dots (17)$$

$$\text{and } \log. (-1) = \pm 2(k+1)\pi\sqrt{-1} \log. e \dots (18)$$

From the above we easily derive the equations (12), (13). The following is an explanation of these equations:  $k$  can only be an

ordinal number, because it is the only variable factor of the exponent  $\pm 2 k \pi \sqrt{-1}$ . Thus  $2 k \pi$  represents *that* circumference whose place is designated by  $k$ .

$\pm \sqrt{-1}$  is a sign of *impossibility*, because it expresses a quantity greater than a *maximum*, and less than a *minimum*; but the area of each concentric ring is out of that circle which serves as a nucleus, and it is the diameter of *that* circle which is a *maximum*. The diameter, then, of the exterior circle of this ring is greater than a *maximum*. The whole area of the ring which exceeds this diameter is then proved imaginary, which shows that the sign  $\pm \sqrt{-1}$  belongs absolutely to it. It now remains to explain the sign  $\pm$ . A ring contains two circumferences of circles; to wit, an external and an internal. Now  $2 \pi$  only expresses one; the sign  $\pm$  causes it to express two; which I thus prove:—

Let  $y y = a a - x x$  .....(19)  
be the equation to a circle: if we take the value of  $y$  we shall have

$y = \sqrt{a a - x x}$  .....(20)

Here the double sign indicates two *ordinates of an equal length drawn from any particular point of the diameter on each side of it*. The positive ordinates, designated by  $+$ , extend only to half the circle; and the negative ordinates designated by  $-$ , extend to the other half: in order to obtain the ordinates which extend to the whole circle, we must unite the two signs, as in  $\pm$ . Now when this sign is accompanied by  $\sqrt{-1}$ , it does not mean  $+$  or  $-$ , but  $+$  and  $-$ ; because the imaginary quantities always go in pairs, and they cannot be separated without an absurdity, as I will prove.

Thus let  $1\ 1\ 1$  be the tangent to the central circle. This tangent

is the *greatest* ordinate which can be drawn to the exterior circle *without entering into the central circle*  $1\ 1\ 1$ : its middle point is at

the same time the *smallest* of those which can be drawn in the interior circle, since it is reduced to this point,  $1$ , in which the two

ordinates coincide, the two ordinates,  $1\ 1$  and  $1\ 1$  having then the

point  $1$ , which is common to both, and are connected by that

point. Thus they form a continued right line, which is expressed by  $\pm \sqrt{-1}$ . If we refer this expression to the interior circumference, we have  $\pm \sqrt{-1} = 0$ , which is not imaginary, because then it is the sign  $\sqrt{-1}$ , which ought to be considered as  $0$ . If, on the contrary, we refer it to the extreme circumference, we have  $\pm \sqrt{-1} = + \sqrt{-1} - \sqrt{-1}$ , which only ceases to be imaginary at the two points  $1\ 1$ , which coincide with this extreme

circumference. To apply this principle to the double sine of the expression  $\pm 2 k \pi \sqrt{-1}$ , let us divide into two equal parts the

part 1 1 of the diameter 1 1, which is intersected between the two

circumferences of the ring which extends beyond the central circle; and through the point of division let us draw the dotted concentric circle; the circumference of the dotted circle will be an *arithmetical mean proportional* between the two extreme circumferences of the ring. If we take this dotted circumference as a line of abscissa, it is clear it will cut all the sections of the diameters intercepted between the extreme circumference into two equal parts. Each of these half parts will be equal ordinates drawn on each side of the circumference of the dotted circle, this circumference being taken as a *line of abscissa*, and the two extreme circumferences will be the curves described by means of these ordinates. As all these ordinates are imaginary, they have only two real points, which are their two extremes: one of these two extremes is a point in the dotted circle, and the other is a point in one of the circles already described: these three circles are then composed only of insulated points, the points of the dotted circle are double, and those of the circle described are simple.  $\pm 2 \pi \sqrt{-1}$  expresses the sum of the points of the dotted circle; that is to say,  $+ 2 \pi \sqrt{-1}$  is the sum of the points of the exterior circle, and  $- 2 \pi \sqrt{-1}$ , the sum of the points of the interior circle. Resuming all this explanation, we find  $\pm 2 k \pi \sqrt{-1}$  is the sign of the description of two concentric circles forming a ring by assuming for a line of abscissa a third concentric circle whose circumference is an *arithmetical proportional mean* between the circumferences to be described, the same as  $\pm \sqrt{a a - x x}$  is the sign of the description of a simple circumference by taking its diameter for a line of the abscissa.

This granted, in order to resolve the equations (1), (2), (3), I begin by multiplying their second member by the second member of equation (12), which gives me

$$a^2 + b c = 16 e^{\pm 2 \pi \sqrt{-1}} = \text{the area* of the 16th circle,} \\ (\text{fig. 1}) \dots\dots\dots (22)$$

$$b^2 + a c = 17 e^{\pm 2 \pi \sqrt{-1}} = \text{the area of the 17th circle.} \dots (23)$$

$$e^2 + a b = 18 e^{\pm 2 \pi \sqrt{-1}} = \text{the area of the 18th circle.} \dots (24)$$

I assume for the roots of these equations,

\* The idea of my giving one area for the root of another area, may perhaps be cavilled at; but when we consider that the root of the area of the square A B C D (fig. 2) can only be the area of a rectangle, such as A a C c or A c B b, the explanation will appear clear.

$$a = e^{\pm \frac{1}{2}} \cdot 2\pi \sqrt{-1} = \text{area of the 13th quadrant} \dots\dots\dots (25)$$

$$b = e^{\pm \frac{1}{2}} \cdot 2\pi \sqrt{-1} = \text{area of the 17th quadrant} \dots\dots\dots (26)$$

$$c = e^{\pm \frac{1}{2}} \cdot 2\pi \sqrt{-1} = \text{area of the 21st quadrant} \dots\dots\dots (27)$$

To prove that these are the *true roots* of equations (22), (23), (24), I substitute these roots for  $a, b, c$ ; then take the differential\*, considering the sign  $\pm$  as the differential sign. These substitutions give me

$$e^{\pm \frac{1}{2}} \cdot 2\pi \sqrt{-1} + e^{\pm \frac{(17+21)}{4}} \cdot 2\pi \sqrt{-1} = 16 e^{\pm 2\pi \sqrt{-1}} \quad (28)$$

$$e^{\pm \frac{1}{2}} \cdot 2\pi \sqrt{-1} + e^{\pm \frac{(13+21)}{4}} \cdot 2\pi \sqrt{-1} = 17 e^{\pm 2\pi \sqrt{-1}} \quad (29)$$

$$e^{\pm \frac{1}{2}} \cdot 2\pi \sqrt{-1} + e^{\pm \frac{(13+17)}{4}} \cdot 2\pi \sqrt{-1} = 18 e^{\pm 2\pi \sqrt{-1}} \quad (30)$$

By taking the differential, I obtain

$$\pm \left( \frac{1}{2} \cdot 2\pi \sqrt{-1} \right) e^{\pm \frac{1}{2}} \cdot 2\pi \sqrt{-1} \pm \left( \frac{17+21}{4} \cdot 2\pi \sqrt{-1} \right) e^{\pm \frac{(17+21)}{4}} \cdot 2\pi \sqrt{-1} = \pm 2\pi \sqrt{-1} \times 16 e^{\pm 2\pi \sqrt{-1}} =$$

the area of the 16th ring  $\dots\dots\dots (31)$

$$\pm \left( \frac{1}{2} \cdot 2\pi \sqrt{-1} \right) e^{\pm \frac{1}{2}} \cdot 2\pi \sqrt{-1} \pm \left( \frac{13+21}{4} \cdot 2\pi \sqrt{-1} \right) e^{\pm \frac{(13+21)}{4}} \cdot 2\pi \sqrt{-1} = \pm 2\pi \sqrt{-1} \times 17 e^{\pm 2\pi \sqrt{-1}} =$$

area of the 17th ring  $\dots\dots\dots (32)$

$$\pm \left( \frac{1}{2} \cdot 2\pi \sqrt{-1} \right) e^{\pm \frac{1}{2}} \cdot 2\pi \sqrt{-1} \pm \left( \frac{13+17}{4} \cdot 2\pi \sqrt{-1} \right) e^{\pm \frac{(13+17)}{4}} \cdot 2\pi \sqrt{-1} = \pm 2\pi \sqrt{-1} \times 18 e^{\pm 2\pi \sqrt{-1}} =$$

area of the 18th ring  $\dots\dots\dots (33)$

The experimental quantities of equation (31) are reducible to

$$\left\{ \begin{array}{l} e^{\pm 13\pi \sqrt{-1}} = -1 \\ e^{\pm 19\pi \sqrt{-1}} = -1 \end{array} \right\} \text{(Vid. equation 13)} \left\{ \dots\dots\dots (34) \right.$$

$$\left\{ \begin{array}{l} e^{\pm 17\pi \sqrt{-1}} = -1 \\ e^{\pm 17\pi \sqrt{-1}} = -1 \end{array} \right\} \dots\dots\dots (35)$$

$$\left\{ \begin{array}{l} e^{\pm 17\pi \sqrt{-1}} = -1 \\ e^{\pm 17\pi \sqrt{-1}} = -1 \end{array} \right\} \dots\dots\dots (36)$$

$$\left\{ \begin{array}{l} e^{\pm 21\pi \sqrt{-1}} = -1 \\ e^{\pm 15\pi \sqrt{-1}} = -1 \end{array} \right\} \dots\dots\dots (37)$$

The equation (33) becomes

$$e^{\pm 21\pi \sqrt{-1}} = -1 \dots\dots\dots (38)$$

$$e^{\pm 15\pi \sqrt{-1}} = -1 \dots\dots\dots (39)$$

\* This kind of differential is the true and strict meaning of Lemma II, Sect. II, Book II, of Newton's Principia. (Momentum Genitæ, &c.) The manner in which Newton has demonstrated this lemma entirely refutes every possible objection.

By substituting these values of the exponential in the equations (31), (32), (33), they take the following forms:—

$$\mp \frac{1}{4} \cdot 2\pi \sqrt{-1} \mp \frac{1}{2} \cdot 2\pi \sqrt{-1} = \pm 2\pi \sqrt{-1} \times 16 \dots (40)$$

$$\mp \frac{1}{4} \cdot 2\pi \sqrt{-1} \mp \frac{1}{4} \cdot 2\pi \sqrt{-1} = \pm 2\pi \sqrt{-1} \times 17 \dots (41)$$

$$\mp \frac{1}{2} \cdot 2\pi \sqrt{-1} \mp \frac{1}{4} \cdot 2\pi \sqrt{-1} = \pm 2\pi \sqrt{-1} \times 18 \dots (42)$$

Now  $2\pi$  expresses the circumference, of which one half is on the positive side, and the other half on the negative side: in respect to the same diameter,  $-2\pi$  expresses the same things taken in contrary directions; consequently  $\pm 2\pi$  and  $\mp 2\pi$  are two different expressions of the same circumference, when the diameters are the same; they then represent the same rings; and the last three equations give by reduction the following:—

$$\pm 2\pi \sqrt{-1} \left( \frac{26 \times 38}{4} \right) = \pm 2\pi \sqrt{-1} \times \frac{1}{4} = \pm 2\pi \sqrt{-1} \times 16 \dots (43)$$

$$\pm 2\pi \sqrt{-1} \left( \frac{34 \times 34}{4} \right) = \pm 2\pi \sqrt{-1} \times \frac{1}{2} = \pm 2\pi \sqrt{-1} \times 17 \dots (44)$$

$$\pm 2\pi \sqrt{-1} \left( \frac{42 \times 30}{4} \right) = \pm 2\pi \sqrt{-1} \times \frac{1}{4} = \pm 2\pi \sqrt{-1} \times 18 \dots (45)$$

These last equations are evidently identical, and the problem proposed is now completely resolved by the roots (25), (26), (27), which, expressed arithmetically, are

$$a = \frac{1}{4} = 3\frac{1}{4} \dots (46)$$

$$b = \frac{1}{2} = 4\frac{1}{2} \dots (47)$$

$$c = \frac{1}{4} = 5\frac{1}{4} \dots (48)$$

*Unity being the area of any circle*, which is an essential remark; for if unity were an abstract number, this solution would be absurd.

It is only true when unity is expressed by  $e^{\pm 2\pi \sqrt{-1}}$  (= the area of any ring or central circle.)

#### *Remark.*

Wallis, who devoted much time and attention to the proposed problem, resolved the equations (1), (2), (3), by approximation. (Wallis's Algebra, Chap. 62.) His roots are,

$$a = 2,525,513,986,744,158 \dots (49)$$

$$b = 2,969,152,768,619,848 \dots (50)$$

$$c = 3,240,580,681,617,174 \dots (51)$$

Comparing these roots with the roots (46), (47), (48), it is easily perceived that they have not the least relation to each other. In order to prove how nearly the roots (49), (50), (51), verify the

equations (1), (2), (3), Wallis has substituted them in the place of  $a, b, c$ , by which he obtained the following result :—

$$16 = a^2 + b c = 16,000,000,000,000,000 \dots\dots(52)$$

$$17 = b^2 + a c = 17,000,000,000,000,000 \dots\dots(53)$$

$$18 = c^2 + a b = 17,999,999,999,999,997 \dots\dots(54)$$

However near Wallis's approximation may be, the equations which he obtained are not for that reason the less absurd, nor are they the less

*Equations, whose two members are not equal and never can become so.*

In order that the two members should be equal, it is necessary that the inequality of the units compensate the inequality of the numbers, as in the equation  $2l = 40s$ ; this compensation takes place if each member and its unit are in an inverse ratio to each other. This is the case in my solution, where *the height of each ring, multiplied by the mean circumference between the two extreme circumferences, give for the product the constant area of the central circle*, as I thus demonstrate, for we have for each ring the following proportion :

- ++ *The sum of the radii of the extreme circumferences of the rings*
- : *To the tangent drawn to the smallest of these circumferences*
- :: *This tangent, (which is = the radius of the central circle,)*
- : *The difference of the radii of the extreme circumferences of the rings. (This difference being the height of the ring, 185.)*

From whence we have the following theorem.

*In a series of concentric rings, each of whose areas are equal to the central circle. The rectangle formed by the sum of the radii of the extreme circumferences of any one of the rings and its height, is equal to the square of the central circles. (56.)* Now the area of each ring is equal the area of a trapezium, which has for its base the height of the ring, and for the mean height half the sum of the extreme circumferences. But this half is equal to a mean circumference between the two extreme circumferences. We can then transform theorem (56) into the following.

*In a series of concentric rings whose areas are equal to the area of the central circle, the rectangle formed of the height of each ring and the circumference, which is a mean between the two extreme circumferences, is equal the area of the central circle, which is the proposition I had to demonstrate.*

#### General Corollary.

The preceding resolution of equations (1), (2), (3), gives a complete solution of Gauss's problem, viz. "*To divide a circumference into 17 equal parts.*" The dotted isosceles triangle  $\bigcirc \begin{smallmatrix} 1 & 1 & 1 \\ 17 & 16 & 17 \end{smallmatrix}$  (fig. 1)

whose summit is the centre  $\bigcirc$ , has for its base the continued line  $\begin{smallmatrix} 1 & 1 & 1 \\ 17 & 16 & 17 \end{smallmatrix}$ ; it is this base whose extremities are  $\begin{smallmatrix} 1 & 1 \\ 17 & 17 \end{smallmatrix}$ , which divides the

circumference into 17 parts. The arc  $\overset{17}{1} \overset{17}{1} \overset{17}{1}$  is one of these parts ;

the continued line  $\overset{17}{1} \overset{16}{1} \overset{17}{1}$  is equal to the central diameter  $1 \overset{0}{\circ} \overset{0}{1}$ .

Now this central diameter is a unit ; for the central circle and its diameter are respectively units, which are heterogeneous with each other. All the parallel lines,  $\overset{1}{1} \overset{0}{\circ} \overset{1}{1}$ ,  $\overset{1}{1} \overset{1}{1} \overset{1}{1}$ , &c. are equal to this

last unit. These lines are the greatest which can be drawn between the two extreme circumferences of each ring, and they may be considered as the diagonals of those rings, in the same manner as

the diameter  $1 \overset{0}{\circ} \overset{0}{1}$  can be considered as the diagonal of the central

circle. We have, then, between the diagonal of the central circle and the diagonal of the rings, the same analogy as between the central circle itself and these rings ; the central circle and the rings are both superficial units, and *maxima* ; the diagonals of the central circle and of the rings are both *linear* units, and also *maxima*. This geometrical construction of the problem, as presented by fig. 1, is the only one which can be right ; because all the straight lines of this figure are either *maxima* or *minima*, in such a manner that if they be *maxima* as lines, they are *minima* as being opposite to an angle or an arch which is a *minimum* ; and if they be *minima* as lines, they are *maxima*, as being opposite to an angle or an arch which is a *maximum*, as I shall demonstrate at length in another paper. These lines, then, cannot be greater or less than they are. M. le Gendre, at the end of his Geometry, has given an algebraical solution of this problem, but he has not given the geometrical construction of his formulæ : that construction was in fact impossible, without destroying the law of continuity.

### ARTICLE III.

*Some Experiments on pure Nickel, its Magnetic Quality, and its Deportment when united to other Bodies.\** By W. A. Lampadius.

1. AFTER having in 1796 discovered a method of obtaining pure malleable nickel by means of an oxygen gas fire, either from Freiberg *Bleispeise*, or from the common regulus of nickel obtained from copper nickel by the usual process, I occasionally made a number of accurate experiments on many of the properties of this metal, which had been hitherto examined only in a cursory manner. 124 grains of *speise* gave me 43 grains of nickel, and 123 grains of copper nickel ore gave me 63 grains of the pure metal.

#### 2. Magnetic Power of Nickel.

The magnetometer described in the preceding paper gave the

\* Translated from Schweigger's *Journal für Chemie und Physik*, x. 174. 1814.

magnetic energy of nickel = 35, and that of iron = 55. The magnetic energy of cobalt was likewise tried, and found = 25; but as this metal was not quite pure, this experiment, as well as the magnetism of an alloy of cobalt and nickel, will be hereafter repeated.

### 3. *Alloy of Nickel and Platinum.*

This, as well as all the subsequent alloys, was made upon charcoal kept intensely hot by a stream of oxygen gas, according to the method described in my *Manual for the Analysis of Minerals*. A grain of each of the two bodies, nickel and platinum, was put upon the charcoal. After they had been softened by the application of the heat for about half a minute, both bodies incorporated together in a very striking manner. They formed an alloy possessing nearly the fusibility of copper, although nickel by itself is nearly as infusible as platinum. The alloy was completely malleable, acquired a fine polish, and had a light yellowish-white colour, not unlike that of sterling silver. Its magnetic energy was still 35.

### 4. *Gold and Nickel (equal parts).*

Both metals very readily melt into one round button; pretty hard, harder than the preceding alloy; externally malleable; capable of a fine polish; colour yellowish-white, a little darker than the preceding alloy. The magnetism continued = 35.

### 5. *Silver and Nickel (equal parts).*

When I attempted to alloy these two metals, I made the following observations. The silver melted in two seconds, and the nickel remained for some time unmelted upon the silver. In about a minute the silver, it is true, took up the nickel, but did not dissolve it. When the heat was continued some time longer, the two metals appeared to unite; but at that instant the silver burnt away with a blue flame, and left the malleable nickel behind it; but about one half of the nickel likewise was burnt.

### 6. *Copper and Nickel (equal parts).*

Both metals melted together in four seconds. The alloy was brittle and granular; the colour reddish-white; and the fracture porous. It exhibited no trace of magnetism.

### 7. *Nickel and Iron.*

Iron and nickel easily melted together into a round bead. The nickel was first melted, and the iron added to it, to prevent the last metal from being burnt by the heat. By continuing the heat, the greater part of the iron separated from the nickel in the state of a black oxide, still attracted by the magnet. By weighing the alloy I found that it consisted of ten parts of nickel and four parts of iron, or the iron amounted to rather less than one-third of the alloy. This alloy was moderately hard, quite malleable, and had the colour of steel. Its magnetism was = 35.

### 8. *Phosphorus and Nickel.*

The bead of nickel was heated red-hot, and then a small piece of

phosphorus placed in contact with it. They melted together in a few seconds. 34 parts of nickel thus treated increased in weight five parts; so that 100 nickel had combined with 15 phosphorus. The button externally was tin-white, and had the metallic lustre. It was moderately hard, and very brittle. Its fracture was foliated and crystalline, partly dull, and partly with the metallic lustre. Its magnetism was gone.

#### 9. *Nickel and Sulphur*

Easily united together, when treated in the same way as the nickel and phosphorus had been. Externally the button was dull, swelled, and grey in colour. Its magnetism likewise was gone. 20 parts of nickel had taken up two parts of sulphur; so that 100 of the metal combine with 10. The mass was elastic, not very hard, the fracture uneven, and the colour yellowish-white, similar to that of native copper nickel ore.

10. From these experiments we learn,

- a. The readiness with which nickel and platinum unite together.
- b. The little affinity between silver and nickel, as the silver rather combines with oxygen and is dissipated, than remains united to the nickel.
- c. The singular effect of combining it with copper, in which we see two malleable metals produce a brittle alloy.
- d. The permanence of the magnetism of nickel when it is alloyed with gold and platinum.
- e. Its complete destruction when nickel is alloyed with copper.
- f. Its diminution when nickel is alloyed with iron.

Perhaps a farther prosecution of these experiments might have a tendency to throw some light upon magnetism. At present I lay aside all hypotheses, and satisfy myself with stating simple facts.

### ARTICLE IV.

#### *Magnetical Observations at Hackney Wick. By Col. Beaufoy.*

Latitude,  $51^{\circ} 32' 40.3''$  North. Longitude West in Time  $6^{\text{h}} \frac{23}{100}$ .

1814.

Month.	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
Nov. 18	h — /	— ' — "	1h 40' 24" 19' 59"		Not observed.	Not observed.
Ditto 19	8 35	24 16 12	1 45 24 21 22			
Ditto 20	8 45	24 15 40	1 35 24 20 05			
Ditto 21	8 40	24 17 47	1 35 24 20 22			
Ditto 22	8 45	24 18 15	1 35 24 21 17			
Ditto 23	8 40	24 17 22	1 40 24 20 48			
Ditto 25	— —	— —	1 35 24 26 10			
Ditto 26	8 45	24 17 04	1 45 24 20 04			
Ditto 27	8 45	24 16 03	1 45 24 20 33			
Ditto 28	8 40	24 17 41	1 30 24 21 29			
Ditto 29	8 45	24 17 34	1 25 24 19 30			
Ditto 30	8 45	24 17 43	— — — —			

1814.									
Mean of Observations in Nov.	Morning	at	8 <sup>h</sup> 41'	.....	Variation	24° 16'	20"	} West.	
	Noon	at	1 40	.....	Ditto	24 20	37		
	Evening	at	—	.....	Ditto	—	—		Not obs.
Ditto in Oct.	Morning	at	8 39	.....	Ditto	24 14	08	} West.	
	Noon	at	1 42	.....	Ditto	24 21	45		
	Evening	at	—	.....	Ditto	—	—		Not obs.
Ditto in Sept.	Morning	at	8 32	.....	Ditto	24 14	33	} West.	
	Noon	at	1 39	.....	Ditto	24 23	17		
	Evening	at	6 19	.....	Ditto	24 16	50		
Ditto in Aug.	Morning	at	8 30	.....	Ditto	24 14	13	} West.	
	Noon	at	1 39	.....	Ditto	24 23	48		
	Evening	at	6 57	.....	Ditto	24 16	31		
Ditto in July.	Morning	at	8 41	.....	Ditto	24 13	29	} West.	
	Noon	at	1 42	.....	Ditto	24 23	44		
	Evening	at	6 58	.....	Ditto	24 17	00		
Ditto in June.	Morning	at	8 44	.....	Ditto	24 13	10	} West.	
	Noon	at	1 30	.....	Ditto	24 22	48		
	Evening	at	6 52	.....	Ditto	24 16	29		
Ditto in May.	Morning	at	8 45	.....	Ditto	24 13	12	} West.	
	Noon	at	1 44	.....	Ditto	24 22	13		
	Evening	at	6 38	.....	Ditto	24 16	14		
Ditto in April.	Morning	at	8 45	.....	Ditto	24 12	53	} West.	
	Noon	at	1 48	.....	Ditto	24 23	53		
	Evening	at	6 29	.....	Ditto	24 15	30		
Ditto in March.	Morning	at	8 52	.....	Ditto	24 14	29	} West.	
	Noon	at	1 52	.....	Ditto	24 23	08		
	Evening	at	6 11	.....	Ditto	24 15	33		
Ditto in Feb.	Morning	at	8 47	.....	Ditto	24 14	50	} West.	
	Noon	at	1 52	.....	Ditto	24 20	58		
	Evening	at	—	.....	Ditto	—	—		Not obs.
Ditto in Jan.	Morning	at	8 52	.....	Ditto	24 15	05	} West.	
	Noon	at	1 53	.....	Ditto	24 19	03		
	Evening	at	—	.....	Ditto	—	—		Not obs.
1813.									
Ditto in Dec.	Morning	at	8 53	.....	Ditto	24 17	39	} West.	
	Noon	at	1 51	.....	Ditto	24 20	30		
	Evening	at	—	.....	Ditto	—	—		Not obs.
Ditto in Nov.	Morning	at	8 40	.....	Ditto	24 17	17	} West.	
	Noon	at	1 54	.....	Ditto	24 20	24		
	Evening	at	—	.....	Ditto	—	—		Not obs.
Ditto in Oct.	Morning	at	8 45	.....	Ditto	24 15	41	} West.	
	Noon	at	1 59	.....	Ditto	24 22	53		
	Evening	at	—	.....	Ditto	—	—		Not obs.
Ditto in Sept.	Morning	at	8 53	.....	Ditto	24 15	46	} West.	
	Noon	at	2 02	.....	Ditto	24 22	32		
	Evening	at	6 03	.....	Ditto	24 16	04		
Ditto in Aug.	Morning	at	8 44	.....	Ditto	24 15	55	} West.	
	Noon	at	2 02	.....	Ditto	24 23	32		
	Evening	at	7 05	.....	Ditto	24 16	08		
Ditto in July.	Morning	at	8 37	.....	Ditto	24 14	32	} West.	
	Noon	at	1 50	.....	Ditto	24 23	04		
	Evening	at	7 08	.....	Ditto	24 16	43		
Ditto in June.	Morning	at	8 30	.....	Ditto	24 12	55	} West.	
	Noon	at	1 33	.....	Ditto	24 22	17		
	Evening	at	7 04	.....	Ditto	24 16	04		
Ditto in May.	Morning	at	8 29	.....	Ditto	24 12	02	} West.	
	Noon	at	1 37	.....	Ditto	24 20	54		
	Evening	at	6 24	.....	Ditto	24 13	47		
Ditto in April.	Morning	at	8 31	.....	Ditto	24 09	18	} West.	
	Noon	at	0 59	.....	Ditto	24 21	12		
	Evening	at	5 46	.....	Ditto	24 15	25		

*Magnetical Observations continued.*

Month.	Morning Observ.			Noon Observ.			Evening Observ.	
	Hour.	Variation.		Hour.	Variation.		Hour.	Variation.
Dec.	1	8 <sup>b</sup> 45'	24° 18' 52"	—	—	—		
Ditto	2	8 45	24 16 53	1 30	24 20 04			
Ditto	3	8 45	24 17 04	1 00	24 21 07			
Ditto	4	—	—	1 40	24 20 01			
Ditto	5	8 45	24 17 28	—	—			
Ditto	6	8 40	24 17 18	1 45	24 21 18			
Ditto	7	8 45	24 17 10	1 35	24 20 30			
Ditto	8	8 45	24 17 36	—	—			
Ditto	9	8 35	24 17 19	1 25	24 19 33			
Ditto	10	8 35	24 18 00	—	—			
Ditto	11	—	—	1 20	24 19 59			
Ditto	12	8 45	24 18 12	1 35	24 18 55			
Ditto	13	8 45	24 20 20	1 30	24 20 48			
Ditto	14	8 50	24 20 14	1 40	24 21 44			
Ditto	15	8 45	24 19 40	—	—			
Ditto	16	8 55	24 18 14	1 55	24 19 40			
Ditto	17	8 50	24 19 46	1 30	24 21 19			

Not observed.

Not observed.

*Comparison of the Variations in the Years 1813 and 1814.*

		1813.	1814.	Difference.
April	Morning .....	24° 09' 18"	24° 12' 53"	+ 3' 35"
	Noon .....	24 21 12	24 23 53	+ 2 41
	Evening .....	24 15 25	24 15 30	+ 0 05
May	Morning .....	24 12 02	24 <del>12</del> 49	+ <del>0</del> 47 <i>13.17. +</i>
	Noon .....	24 20 54	24 22 13	+ 1 19
	Evening .....	24 13 47	24 16 14	+ 2 27
June	Morning .....	24 12 35	24 13 10	+ 0 35
	Noon .....	24 22 17	24 22 48	+ 0 31
	Evening .....	24 16 04	24 16 18	+ 0 44
July	Morning .....	24 14 32	24 13 29	- 1 03
	Noon .....	24 23 04	24 23 44	+ 0 40
	Evening .....	24 16 43	24 17 00	+ 0 17
Aug.	Morning .....	24 15 55	24 14 13	- 1 42
	Noon .....	24 23 32	24 23 48	+ 0 16
	Evening .....	24 16 08	24 16 31	+ 0 23
Sept.	Morning .....	24 15 46	24 14 33	- 1 16
	Noon .....	24 22 32	24 23 17	+ 0 45
	Evening .....	24 16 04	24 16 50	+ 0 46
Oct.	Morning .....	24 15 41	24 14 08	- 1 33
	Noon .....	24 22 53	24 21 45	- 1 08
	Evening .....	—	—	—
Nov.	Morning .....	24 17 17	24 16 20	- 0 57
	Noon .....	24 20 24	24 20 37	+ 0 13
	Evening .....	—	—	—

In taking the mean of the observations for the month of November, the variation on the 4th and 25th are rejected, as being unusually great, for which no cause can be assigned.

*Dec. 9.*—The needle vibrated at intervals 26' 20"; and in the evening the wind blew very hard and squally from the S.W. accompanied by rain.

Dec. 16.—The needle vibrated 15'; wind high and squally from the S.W., followed by rain.

Rain fallen { Between noon of the 1st Nov. } 2.207 inches.  
 { Between noon of the 1st Dec. }

## ARTICLE V.

*On the Conversion of Cast-Iron Pipes into a Substance bearing some resemblance to Plumbago.* By William Henry, M.D. F.R.S. &c.

I was lately requested by a gentleman who resides in the neighbourhood of Newcastle-upon-Tyne, to examine the nature of the change effected in a cast-iron pipe, placed in the shaft of a coal mine near that town. In sinking the shaft, it was necessary, as sometimes happens, to put down a *curl*, or cylinder, of cast-iron, in order to support a bed of quicksand; and into a suitable opening in this cylinder, the cast-iron pipe, three inches diameter, was bolted by means of a *flanche* at its extremity. Its use was to allow an exit to the water and gas, which issued from the stratum of quicksand.

The fragment of the pipe, with which I was furnished, was of a dark grey colour; its inner surface was smooth and black; and its outer surface had a thin ochrey incrustation. The usual fracture of cast-iron was exchanged for an earthy one, except near the centre of the mass, where somewhat of the usual texture of cast iron was still visible. It was soft enough to be easily scraped by a knife; and was readily broken by a slight blow of a hammer. Some parts of it left a black trace on writing paper, but destitute of the lustre which the traces of plumbago exhibit.

The specific gravity of the specimen was 2.008; and, after being soaked an hour or two in water, it became 2.155.

Twenty grains of the powdered substance were projected upon 200 grains of melted nitre. The combustion was very feeble, compared with that which happens to plumbago under similar circumstances. After washing off the nitre, there remained  $14\frac{1}{2}$  grains, which were almost entirely oxide of iron, consisting probably of 11.2 iron and 3.3 oxygen.

A small piece of the pipe, weighing  $6\frac{1}{2}$  grains, was passed into a glass tube containing diluted sulphuric acid over mercury. The acid acted very slowly; and, in eight days, when its effect seemed to be complete, only half a cubic inch of hydrogen gas was formed. This was only about  $\frac{1}{8}$ th of what would have been obtained from the same quantity of cast-iron.\* There remained a black and

\* A piece of plumbago from Borrowdale, kept eight days in dilute sulphuric acid, had evolved no gas.

bulky substance, which, when dried and projected on red-hot nitre, burnt rather more vividly than the original substance, but still afforded much oxide of iron.

It was clear, therefore, that a large share of the metallic part of the pipe had been removed; and that what remained was composed of iron, plumbago, and other impurities usually present in cast-iron. With the view of ascertaining the cause of the change, I next examined the water from the bed of quicksand, a bottle of which had been sent along with the fragment of pipe.

The water had a brackish taste, and was of the specific gravity 1008. It gave no traces of iron, either with triple prussiate of potash, or succinate of ammonia. I collected the gas from a wine pint of it; but it was lost by accident before being examined. By the usual methods of analysis, a wine pint gave 64 grains of dry salt, composed of

Muriate of soda .....	32
————— magnesia .....	16
————— lime .....	10
Sulphate of lime .....	4
Carbonate of lime .....	2

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64

It is most probably to the agency of the muriates of lime and magnesia, that we are to ascribe the removal of the metallic part of the pipe. I have often remarked the effect of solutions of these salts in discharging writing ink from the labels of bottles, to which they have been accidentally applied: and I was lately baffled in several attempts to restore the legibility of some of the M.S.S. of a most accomplished scholar (the late Mr. Tweddell of Trin. Coll. Cambridge) which had lain some time under sea water, abounding, as is well known, in muriate of magnesia. The texture of the paper was entire, but the iron basis of the ink, as well as the gallic acid, was entirely removed.

In that copious repository of valuable knowledge, Dr. Priestley's *Experiments and Observations on Air*,\* some facts are stated, that bear an analogy to the one which I have described. Cast-iron nails, he found, dissolved very slowly in diluted sulphuric acid; and left a large proportion of black matter, which had the original form of the nails. This experiment, he observes, explains what happens to cast-iron pipes in pits, the water of which is impregnated with vitriolic acid; for, in time, they become quite soft, or, as it is called, *rotten*, and may be cut with a knife.

In Cornwall, I am informed, cast-iron pipes are disused in many of the mines; but this is owing to the presence of sulphate of copper in the water, the corroding effects of which render it necessary to substitute pipes of brass or copper.

\* Abridged and methodized, vol. iii. part iii. sect. 2.

The following fact, which was lately observed at the printing-works of a friend of mine, belongs to a different class of phenomena ; and is perhaps to be accounted for by galvanic agency. In order to confine the heat in some cast-iron steam-pipes, they were placed in a trough or gutter made of bricks, into which powdered charcoal was tightly rammed. At a place contiguous to a joint, formed by bolting two flanches together, a leak had happened ; and when the iron pipe was taken up, it was found in the neighbourhood of the leaky part, to be perfectly soft and rotten. I was not able to obtain an opportunity of examining the nature of the change by any experiments on the altered iron.

*Manchester, Dec. 1814.*

## ARTICLE VI.

### *Proceedings of Philosophical Societies.*

#### ROYAL SOCIETY.

On Thursday the 24th of November, part of a paper by Dr. Brewster was read, on the depolarisation of light by mineral, vegetable, and animal bodies.

On Wednesday the 30th of November, the day of the anniversary election, the Copleyan medal was given to Mr. Ivory for his Mathematical Papers published in the Transactions. The following office-bearers were elected for the ensuing year.

**PRESIDENT.**—Right Hon. Sir Joseph Banks, Bart. K. B.

**SECRETARIES.**—William Hyde Wollaston, M. D.

Taylor Combe, Esq. M. A.

**TREASURER.**—Samuel Lysons, Esq.

#### OF THE OLD COUNCIL.

Right Hon. Sir Joseph Banks.

Sir Charles Blagden, Knt.

Samuel Goodenough, Lord Bishop of Carlisle, V. P.

Taylor Combe, Esq. Secretary.

Samuel Lysons, Esq. Treasurer, V. P.

George, Earl of Morton, K. T. V. P.

Thomas Murdoch, Esq.

John Pond, Esq. Astr. Royal.

William Charles Wells, M. D.

William Hyde Wollaston, M. D. Secretary.

Thomas Young, M. D. Secretary for Correspondence.

#### OF THE NEW COUNCIL.

Mr. William Allen.

William Blake, Esq.

Rev. Charles Burney, LL. D.  
Charles William, Earl of Charleville.  
Davies Giddy, Esq. M. P.  
Sir Everard Home, Bart.  
James Horsburgh, Esq.  
Alexander Marcet, M. D.  
Thomas, Earl of Selkirk.  
Henry Warburton, Esq.

The number of deaths since the last anniversary election has been 16; the number of new members admitted, 23. The number of ordinary members on the election list amounts to 580, that of foreign members to 43, making a total of 623 members.

On Thursday Dec. 8, Dr. Brewster's paper on the depolarisation of light by different bodies was continued. He gave a long list of animal, vegetable, and mineral bodies, that have the property of depolarising light; together with a list of bodies that have no effect in producing this depolarisation. He then gave his theory of this depolarisation. Bodies capable of depolarising light may be divided into seven classes. 1. Those that have a neutral axis and produce a double image. With respect to these bodies the theory is evident. 2. Those that have a neutral axis but produce only a single image, as the human hair. In these bodies he supposes that two images are really produced, but that they coincide with each other.

On Thursday Dec. 15, Dr. Brewster's paper was concluded. He continued his enumeration of the different classes of depolarising bodies as follows:

3. Those that have no depolarising axis but depolarise light in every direction, as gum arabic. These he conceives to be composed of layers placed one over the other, each of which has a depolarising axis; and as these axes are placed in every direction, the body acquires the property of depolarising in every direction. 4. Those bodies that have an approximation to a neutral axis, as gold-beater's skin. 5. Those that have an approximation to a depolarising axis. 6. Those that allow the light nearly to vanish, but not quite, at every alternate sector, as oil of mace. 7. Those that allow it to vanish entirely at every alternate sector, as calcareous spar, when the light passes through the shorter axis.

At the same meeting part of a paper by Benjamin Travers, Esq. was read, On the Mechanism by which the Eye adjusts itself to different Distances. The paper was drawn up with uncommon clearness, and even elegance. It consisted of three parts: 1. A sketch of the different hypotheses to which this mechanism has been ascribed, with the author's reasons for not admitting them. These are the muscles of the eye, the ciliary processes, the supposed muscularity of the lens itself. 2. An account of the anatomy of the eye, as far as it is requisite for the purposes of the paper. 3. His reasons for believing that external pressure is applied to the lens, and that this pressure is sufficient to alter its shape.

On Thursday 22d Dec. Mr. Travers's paper was continued. He gave his reasons for considering the iris as muscular, and that by means of it the pupil is adjusted to different distances.

The Society adjourned over two Thursdays, on account of the approaching holidays.

#### LINNÆAN SOCIETY.

On Tuesday, December 6th, a paper by James Johnson, Esq. was read, giving an account of some fossil bones found in the cliff near Lyme, Dorsetshire. This cliff abounds in belemnites, nautili, and the remains of other sea animals. The bones in question have been supposed to belong to the crocodile; but Mr. Johnson gave his reasons for considering that opinion as ill founded. He thinks they constitute the bones of a new and unknown species of amphibious animal. He is of opinion that the animals whose remains are found here lived and died upon the spot.

A communication from Mr. Sowerby was also read, On the Spiral Tubes of some Fossil Shells belonging to the genus *Anomia* of Linnæus and *Terrebratula* of Lamark. These shells occur in limestone, and he thinks the spiral tubes will furnish characters sufficient to constitute them a new genus.

M. Vieillot's paper begun at the last meeting was continued.

On Tuesday December the 20th M. Vieillot's paper was continued.

Specimens of flax and hemp were exhibited to the Society, prepared by machinery invented by Mr. Lee, without water steeping or dew retting. The specimens were beautiful. The advantages of the plan are that the produce is greater and better; and the green part of the plant is preserved, which furnishes very good food for cattle, and is an excellent manure. The seed also is preserved.

#### WERNERIAN SOCIETY.

At the Meeting of this Society, on Saturday the 12th of November, there was read a paper by the Rev. Dr. Fleming of Flisk, On the Fossil Species of *Orthocera* found in Britain. The Doctor has paid much attention to petrifications; and he here described no fewer than ten species of orthocera, all of them found in strata belonging to the independent coal formation in the county of Linlithgow. Two of these orthoceratites have already been figured by the late Rev. Mr. Ure, in his *History of Rutherglen and Kilbride*; the rest were now for the first time described; and the descriptions were illustrated by beautiful and correct drawings. The secretary presented two communications from Mr. Scoresby, junior, of Whitby, of great interest to the meteorologist; being journals kept during voyages to Davis's Straits, in the years 1813 and 1814, in which Mr. Scoresby notes the latitude and longitude; the temperature, generally giving the mean of three daily observations; the direction and strength of the winds; the aqueous and other meteors;

the modifications of the clouds according to Mr. Luke Howard's plan; and the different animals seen, the appearance of the ice, and other incidental circumstances.

At the same meeting there was read a letter from Mr. William Nicol, Lecturer on the Philosophy of Natural History, addressed to Professor Jameson, giving an account of a bed of flexible float limestone, observed by him on the coast of Durham county, near Tynemouth castle. Fine specimens were exhibited. This limestone is very distinctly of a lamellar structure. When a thin bar of it is thoroughly wetted, it is very flexible; when evaporation has proceeded a certain length, it becomes rather rigid; but when the water is completely exhaled, it regains its flexibility, nearly to the same extent as when saturated with water.

There was also read a short account of the different strata cut through in sinking the pit to the coal at Brora in Sutherlandshire, communicated by Mr. Dempster of Dunichen.

## ARTICLE VII.

### SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

#### I. *Lectures.*

Mr. SINGER will commence his Lectures on Electricity and Electro-Chemistry, at the Russell Institution, on Monday the 16th of January, at 8 o'clock in the evening. The Lectures will be continued every succeeding Monday at the same hour. A syllabus may be obtained of the secretary, at the institution, Great Cornam-street, Russell-square; or of Mr. Singer, N° 3, Princes-street, Cavendish-square.

Dr. Clarke and Mr. Clarke will begin their next Course of Lectures on Midwifery and the diseases of women and children, on Monday, January 23d. The Lectures are read every morning, from a quarter past ten to a quarter past eleven, for the convenience of students attending the hospitals. For particulars apply to Dr. Clarke, New Burlington-street, or to Mr. Clarke, 10, Saville Row, Burlington Gardens.

*Medical School of St. Thomas's and Guy's Hospitals.*—The Spring Course of Lectures at these adjoining Hospitals will commence the beginning of February, viz.:

*At St. Thomas's.*—Anatomy and the Operations of Surgery; by Mr. Astley Cooper and Mr. Henry Cline.—Principles and Practice of Surgery; by Mr. Astley Cooper.

*At Guy's.*—Practice of Medicine; by Dr. Babington and Dr. Curry.—Chemistry; by Dr. Babington, Dr. Marcet, and Mr. Allen.—Experimental Philosophy; by Mr. Allen.—Theory of Medicine, and Materia Medica; by Dr. Curry and Dr. Cholmeley.—

Midwifery, and Diseases of Women and Children; by Dr. Haughton.—Physiology, or Laws of the Animal Economy; by Dr. Haughton.—Structure and Diseases of the Teeth; by Mr. Fox.

N. B. These several Lectures are so arranged, that no two of them interfere in the hours of attendance; and the whole is calculated to form a complete Course of Medical and Chirurgical Instruction. Terms and other particulars may be learnt at the respective Hospitals.

## II. Important Chirurgical Operation.

A new method of operating for the cure of Popliteal Aneurism has been employed in Dublin with the most complete success; which seems to open new and important views with respect to the treatment of diseased and wounded arteries in general.

The operation was performed by Mr. Crampton, Surgeon General, at the King's Military Infirmary, near Dublin, on the 15th of October. The following particulars respecting the operation are extracted from the Hospital Journal. "The femoral artery was laid bare at the usual place, by an incision three inches in length and *compressed*, (but not tied circularly,) by a narrow tape, so as completely to obstruct the current of the blood *with the least possible disturbance to the artery, or injury to its coats*. The ligature was by a peculiar contrivance applied in such a manner as to enable the operator to tighten or to relax it at pleasure, without interfering with the wound. In two hours and a quarter the ligature was gently relaxed, but not completely loosened; no pulsation in the ham. In twenty-four hours the artery was relieved from all compression; but as a measure of precaution, the ligature was left in the wound. In forty-eight hours the ligature was withdrawn, and the wound was united by adhesive plaster."

The patient was examined on the fifth day by several professional gentlemen; his health was not in the least degree deranged. The tumor, which had decreased by one half, was without pulsation, and nearly incompressible. The temperature of each foot was 84°.

On the 14th day the wound was nearly healed, and the man went about the ward on crutches.

On the 18th day the wound was healed, and the tumor which could be seen only in the extended position of the limb, was incompressible, and altogether free from pain.

The advantage of Mr. Crampton's operation consists in its imitating in the most favourable way the process of nature, in the spontaneous cure of aneurism.

1st. By interrupting the course of the blood through the ruptured artery, the fluid contents of the aneurismal sack are allowed to coagulate, and the circulation is thrown upon the collateral branches.

2d. The subsequent obliteration of the artery is effected by a natural process, which protects the patient from the long train of sufferings, and of dangers necessarily attendant upon the permanent

contraction of a great artery, and its separation from the ligature by the process of sloughing or ulceration.

The details of this important case will no doubt be speedily communicated to the profession, through some of the usual channels of scientific intelligence.

### III. *Air Volcano.*

The Europeans who are not seasoned to the climate are accustomed to leave Carthagena (in South America) during the heats of summer, and to spend that season at Turbaco, an Indian inland village, delightfully situated on the top of a hill. Mr. de Humboldt and his fellow traveller M. Bonpland, spent the month of April, 1801. in this village. During their herbalizations, they were informed by their Indian guides, that there existed a country not far off, in the midst of a forest of palms, to which the name of *volcanitos* (*little volcanoes*) was given; that its surface had been formerly covered with fire; that a priest had extinguished the combustion with holy water; and that now it emitted nothing but water. On going to the spot, our travellers found in the middle of a vast plain, eighteen or twenty small cones, about 24 feet in height. These cones are formed of a blackish grey clay, and have an opening at their summits filled with water. On approaching these small craters, a hollow but very distinct sound is heard at intervals, 15 or 18 seconds previous to the disengagement of a great quantity of air. The force with which this air rises above the surface of the water, may lead us to suppose that it undergoes a great pressure in the bowels of the earth. Five explosions generally take place in two minutes, and this phenomenon is often attended with a muddy ejection. According to the Indians, the forms of the cones undergo no visible change in a great number of years; but the ascending force of the gas, and the frequency of the explosions, appear to vary according to the seasons. Mr. de Humboldt examined this gas, and found that it was azote mixed with less than one thousandth of its bulk of oxygen gas. Here is a phenomenon well worth the attention both of geologists and chemists. Whence comes this azotic gas? How is it produced? Does there exist in this place an immense magazine of ammonium, which by means of water is converted into azote? We are far indeed from being able to philosophize in the present state of our knowledge, respecting the changes that take place under the surface of the earth.

### IV. *Mathematical Problem.*

(To Dr. Thomson.)

SIR,

Plymouth, Nov. 21, 1814.

I beg, through the medium of your Journal, to present to the attention of the mathematical part of your readers, the following

Theorem, originally proposed by the celebrated John Bernouilli, and which I have extracted from Bossut's *Calcul. Integral*, tom. i. p. 484.

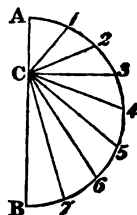
The learned author, from whose invaluable work I have taken it, has not given the investigation; and I therefore solicit the favour of your inserting it, in hopes that some of your correspondents may be able to furnish a solution to it.

I am, Sir, your humble Servant,

GEORGE HALVEY.

*Theorem.*

"Let  $A 4 B$  be the semi-circumference of a circle, of which  $AB$ , the diameter, is equal to the two semi-axes,  $AC$ ,  $BC$ , of a given ellipse. Divide the semi-circumference,  $A 4 B$ , into a number, 2, 4, 8, 16, &c., of equal parts; draw straight lines from the point  $C$  to all the points of division: take the arithmetical mean of all the straight lines drawn to the points of division denoted by odd numbers, and also the arithmetical mean of the sum of all the lines drawn to the points of the even numbers, and the half of  $AB$ ; these two arithmetic means shall be the radii of two circles, the circumference of one of which shall be less than that of the ellipse, and the circumference of the other greater."



Required the demonstration?

*V. Burning Gas in the Appenines.*

Sir Humphry Davy lately discovered, in the Appenines, a jet of gas burning with great brilliancy, and forming a column of flame six feet high. The gas was pure carbureted hydrogen. This is a fact of considerable interest in a geological point of view. The Appenines consist chiefly of limestone, and belong, unless I am misinformed, to the floetz formation. It would be of importance to know, whether any coal exists in the neighbourhood of this flaming jet of gas, or whether it proceeds from a great depth under the surface. This gas is formed in great abundance at the bottom of dirty stagnant water during the summer time. But that fact throws no apparent light on the formation of the gas in the mountains of the Appenines.

*VI. Weather in Iceland during the Winter 1813—14.*

Some of my meteorological friends are anxious to know through the medium of the *Annals of Philosophy*, whether there was any thing peculiarly severe in the last winter in Iceland. If any of my readers happen to be acquainted with the matter of fact, it would be considered as an obligation if they would communicate what they know.

VII. *Chromate of Iron.*

Chromate of iron occurs in the Bare Hills near Baltimore, in North America, in great abundance. These hills appear to be serpentine, so that the situation of this mineral is the same in America as in France. The chromate of iron occurs in these hills in three states: 1. In compact masses of considerable size, mixed with talc, steatite, &c. In this state, which is I presume the same nearly with the French chromate, it has some resemblance to brown blende. 2. In grains from the size of a mustard seed to that of a pepper corn, imbedded in steatite. 3. Crystallized in regular octahedrons. The colour of these crystals is black, inclining to steel grey. Surface smooth. Lustre vitreous. Fracture conchoidal. Opaque, except some specimens which transmit a blood red light. Infusible before the blow-pipe, without addition. Magnetic in a considerable degree. Mr. Hayden, from whom this account is taken, believes that many specimens of supposed octahedral iron ore are really chromate of iron.\*

VIII. *Magnetic Pyrites.*

Stromeyer has lately subjected this mineral to a new analysis, and as he was at uncommon pains to attain the mineral in a state of purity, it is probable that the result which he obtained is a close approximation to the truth. Mr. Hatchett published the first analysis of this mineral, and made us acquainted with its true nature. It was analysed by Proust, and experiments on the artificial combinations of iron and sulphur were made by Vauquelin. The following is the result which they obtained:—

	Iron.	
Hatchett .....	100	+ 57·48 sulphur.
Proust. ....	100	+ 60
Vauquelin . ....	100	+ 28·22
Stromeyer. ....	100	+ 67·08

If my number for the weight of an atom of iron be correct, and if magnetic pyrites be composed of one atom iron and two atoms sulphur, its constituents should be

Iron.	
100	+ 55·99 sulphur.

This circumstance throws some doubt upon the accuracy of Stromeyer's analysis. He informs us, that magnetic pyrites usually contains a mixture of metallic iron and of cubic pyrites. If this be the case, the chance of error in such an analysis was considerably increased.

IX. *Whitestone.*

Among some minerals which I lately received from Freyberg, there is a specimen of the true whitestone of Werner. This rock

\* Bruce's American Journal, No. iv. p. 243.

I find it is composed of three different minerals; namely, *felspar*, *quartz*, and *garnet*. The *felspar* and *quartz* are both white, and constitute the principal part of the rock. The *garnet* is scattered throughout the rock in small red grains and crystals. From this it appears that our notions respecting this rock in this country have been hitherto incorrect. I have never seen any rock resembling it in Great Britain, except in cabinets; and in no one was I able to learn whence the specimen had come. We see from *Humboldt's* analysis, given in a preceding number of the *Annals*, that it is of no real utility, as the constituents so found must depend upon the accidental proportions of the three minerals constituting the *whitstone*.

#### X. Height of the Peak of Teneriffe.

Of all the measurements of this mountain, there seem to be only three upon which we can rely. Borda measured it trigonometrically in 1776; Lamanon barometrically in 1785; and Cordier barometrically in 1803. Their results are as follows:—

Borda .....	1905 French toises.
Lamanon .....	1902
Cordier .....	1920

Mean 1909

This gives us the height of the peak above the level of the sea, 12,207 English feet.

#### XI. Dip of the Magnetic Needle, and Intensity of the Magnetic Forces in the North Atlantic in 1799.

N. Latitude.	W. Longitude from Greenwich.	Magnetical Dip.	Number of oscillations in ten min.	Remarks.
38° 52'	18° 42'	68° 18'	242	Good observation.
37 26	18 52	67° 8'	242	Almost perfect calm.
34 30	19 15	65° 70'	234	Perfect calm.
31 46	19 24	64° 71'	237	Doubtful, especially the intensity.
28 28	20 53	62° 41'	238	Good.
24 53	23 18	60° 84'	239	Very good.
21 29	28 2	58° 18'	237	Good.
19 54	31 5	57° 27'	236	Good.
14 15	50 23	50° 67'	239	Good.
13 2	55 35	45° 60'	234	Dip good, intensity doubtful.
11 1	57 11	42° 34'	237	Good.
10 46	63 14	42° 25'	239	Good.

Humboldt's Personal Narrative, ii. 115.

#### XII. Postman of the Province of Jaen de Bracamoros.

In order to render the communication between the coasts of the Pacific Ocean, and the province of Jaen de Bracamoros, situated on the east of the Andes, more easy, the postman of Peru

descends swimming for two days, first the river Guaneabamba, or Chamaya, and afterwards that of Amazons, from Pomahuaca and Ingatambo to Tomependa. He wraps the few letters of which he is the bearer every month, sometimes in a handkerchief, sometimes in a kind of drawers called guayuco, which he winds as a turban round his head. This turban contains also the great knife with which every Indian is armed, rather to cut his way through the forests, than as a weapon of defence.

The Chamaya river is not navigable, on account of a great number of small cascades. Its fall, according to Humboldt, from the ford of Pucara to its mouth in the river of the Amazons, below the village of Choras, a distance of only 18 leagues, is 1778 English feet. The postman in order to fatigue himself less in descending the river, supports himself on a log of bombax or ochroma, trees of very light wood. When a ledge of rocks intersects the bed of the river, he lands above the cascade, crosses the forest, and re-enters the water when he sees no further danger. He has no need of taking provisions with him, for he finds a welcome in a great number of huts, surrounded with plantain trees, and situated along the banks of the river, between las Huertas de Pucara, Cavico, Sonanga, and Tomependa. These rivers are not infested with crocodiles; the savage hordes therefore almost all travel like the Peruvian postman.—*Humboldt's Researches*, p. 254.

### XIII. Floetz Trap Rocks.

It appears from the observations of Professor Jameson that there exist three floetz trap formations: 1. That in old red sandstone. 2. That in the independent coal. 3. The newest floetz trap, which is considered as newer than the other two. It constitutes the floetz trap formation described by Werner. The floetz trap mountains of Scotland belong chiefly to the two first formations.

### XIV. Granite in Scotland.

Two granite mountains have been lately discovered in Roxburghshire, immediately on the borders of England. They appear to be situated in the midst of the transition rocks of that country.

### XV. Hoffman.

Hoffman, well known as the author of the best German work on mineralogy, is just dead. Unfortunately he has left his work unfinished.

### XVI. Steffens.

Dr. Steffens, one of the most eloquent teachers of natural history on the Continent, is a professor at Breslaw, the capital of Silesia. When Silesia was invaded by the French armies, he called together his class, pointed out to them in glowing colours the disgrace of quietly witnessing the pollution of their country by an enemy, and the honour to be obtained by joining in the glorious

struggle for liberty. His eloquence was successful. The whole class joined the army of Blucher; and Steffens, as adjutant, with his pupils, shared the glory of the campaign of 1813. He has returned with his pupils to Breslaw, has re-opened his class, and is preparing the continuation of his excellent Manual of Oryctognosy

#### XVII. *Transition Limestone.*

It appears from the late observations of Mohs, that the great ranges of limestone in Carinthia, Stiria, &c. considered formerly as belonging to the first floetz or Alpine limestone, are in fact transition rocks. This is an important fact.

#### XVIII. *Porphyry.*

Porphyry is now described by the German geognosts as occurring not only in the primitive, but likewise in the transition and floetz formations.

#### XIX. *Interior of New Holland.*

In the month of December, 1813, Governor Mc. Quarrie sent Mr. Evans to explore the interior of New Holland, by crossing the blue mountains, and travelling in a westerly direction. He took his departure from Emu island, which lies nearly west from Port Jackson, in the Nepean river. The Nepean river runs into the Hawkesbury river, and the colony has extended nearly to its banks. Mr. Evans was absent about three months. He crossed the blue mountains, and travelled nearly due west rather more than 100 miles, through a flat and fertile country. He came to a river running west, to which he gave the name of Mc. Quarrie river. Want of room prevents us at present from entering further into detail. But we shall take an opportunity of resuming the subject in a future number.

### ARTICLE VIII.

#### *New Patents.*

EDWARD CHARLES HOWARD, Nottingham-place, Middlesex; for certain means of separating insoluble substances from fluids in which the same are suspended. Aug. 4, 1814.

TOBIAS MICHEL, Upper Thames-street, London; for a machine for raising water with less power than has hitherto been experienced for the impelling of machinery and other purposes. Aug. 4, 1814.

MICHAEL LARKIN, Blackwall, Middlesex; for improvements in windlasses for ships and other vessels. Aug. 16, 1814.

HENRY WILLIAM VANDERKLEF, No. 253, High Holborn, London; for constructing a walk staff to contain a pistol, powder, ball, and telescope, pen, ink, paper, pencil, knife, and drawing utensils. Aug. 17, 1814.

## ARTICLE IX.

## METEOROLOGICAL TABLE.

1814.	Wind.	BAROMETER.			THERMOMETER.			Evap.	Rain.	
		Max.	Min.	Med.	Max.	Min.	Med.			
11th Mo.										
Nov.	4 N E	29.83	29.74	29.785	44	33	38.5	—	—	
	5 N E	29.80	29.71	29.755	42	32	37.0	—	.45	
	6 N	29.84	29.77	29.805	47	30	38.5	—		
	7 S W	29.77	29.32	29.545	49	31	40.0	—	1	
	8 S W	29.37	29.23	29.300	46	32	39.0	—	9	
	9 N W	29.89	29.37	29.680	42	29	35.5	—		
	10 N W	30.28	29.89	30.085	40	26	33.0	—		
	11 S W	30.28	29.78	30.030	47	31	39.0	.18		
	12 S W	29.85	29.78	29.815	52	38	45.0	—	8	
	13 N W	29.86	29.80	29.830	49	39	44.0	—	—	
	14 S W	29.85	29.75	29.800	53	43	48.0	—	—	
	15 S W	29.78	29.28	29.530	54	45	49.5	—	—	
	16 W	30.00	29.28	29.640	49	32	40.5	—	.18	
	17 S W	30.00	29.82	29.910	53	35	44.0	—	—	
	18 S W	29.82	29.37	29.595	52	42	47.0	—	.17	
	19 N W	29.48	29.37	29.425	47	28	37.5	.22	—	
	20 N E	29.57	29.48	29.525	43	34	38.5	—	.20	
	21 N	29.69	29.57	29.630	38	20	29.0	—	—	
	22 N W	29.67	29.65	29.660	36	19	27.5	—	—	
	23 N W	29.81	29.67	29.740	35	26	30.5	—	—	
	24 Var.	29.81	29.48	29.645	46	35	40.5	—	.16	
	25 S	29.48	29.18	29.330	50	42	46.0	—	.32	
	26 W	29.59	29.18	29.385	50	32	41.0	.5	7	
	27 Var.	29.68	29.30	29.490	44	35	39.5	—	.68	
	28 S W	29.64	29.44	29.540	45	36	40.5	—	.27	
	29 W	29.44	29.15	29.295	48	34	41.0	—	—	
	30 N	29.45	29.12	29.285	44	32	38.0	—	—	
12th Mo.										
Dec.	1 N W	29.73	29.45	29.590	43	30	36.5	—	—	
	2 W	29.86	29.73	29.795	38	30	34.0	—	3	
	3 E	29.86	29.50	29.680	38	28	33.0	.6	32	
		30.28	29.12	29.635	54	19	39.05	0.51	3.03	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

## REMARKS.

*Eleventh Month.*—4. Misty morning; cloudy; snow at night. 5. Overcast, a.m.: wet afternoon and evening. 6. The drops of dew were frozen, but so clear as not to appear like hoar frost; a fine day, with large *Cirri*, and afterwards *Cirrostratus* and *Cumulus*: a streaked orange sky at sun-set. 7. Hoar frost, followed by cloudy sky: some rain after dark. 8. Clear morning: hoar frost: about four p. m. the sky became very dark, and there was a storm of rain, attended by a single loud explosion of electricity. It is remarkable that on the same day of the month last year, at the same hour, and in the same direction, (south-west,) we had a similar storm. 9. *Cirrus*, with the compound modifications: several shooting stars this evening: windy afterwards. 10. A steady breeze from the N.W.: *Cirrostratus* appeared over the Thames at sun-rise: ice a quarter of an inch thick, and permanent; a brilliant twilight; first lemon colour, surmounted by purple; then, by the blending of the two, a rich deep orange. 11. Hoar frost. I observe this often continues an hour longer on some tufts of *saxifraga cæspitosa* than any where else in my garden: the plant grows on close spongy masses of fibre, which are bad conductors of heat. 12. It was stormy last night; and is said to have thundered and lightened about three a. m. Wet this morning; but the day fine: twilight orange, delicately varied with dusky horizontal striæ. 13. Cloudy and raining at eight a. m., but fine afterwards, with a smart breeze. 14. Cloudy. 15. *Cirrostratus*, a.m.: then *Cirrus nimboformis*, depending and curling beneath: a turbid sky ensued, and wind in the night. 16. A wet stormy morning. 17. Much wind by night. 18. Small rain, a.m.: wet and stormy at night. 19. Hoar frost: a little rain at intervals. 20. Overcast. 21. Overcast. 22, 23. Hoar frost: misty. 24. The same: near sun-set a hollow wind from S.W., and rain in the night. 25. Wet day. 26. Some rain, a.m. 27. Clear morning, with evaporation: but before noon cloudiness was coming on from the W., and it rained steadily from sun-set till midnight. 28. Wet again in the night. 29. Large elevated *Cirri*, with *Cirrostratus*. 30. Wet.

*Twelfth Month.*—1. Brisk wind at N.: in the night a slight sprinkling of snow. 2. Wet morning.

## RESULTS.

Prevailing Winds, Westerly.

Barometer: Greatest height..... 30.28 inches;

Least..... 29.18 inches;

Mean of the period..... 29.635 inches.

Thermometer: Greatest height..... 49.00°;

Least..... 34.91°;

Mean of the period..... 39.07°.

Rain, 3.09 inches. Evaporation, 0.51 inch.

\* \* In the period last reported, the amounts of evaporation were put, by an oversight, in the column headed " Rain," and *vice versa*; which the reader is requested to correct.

# ANNALS

OF

## PHILOSOPHY.

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FEBRUARY, 1815.

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### ARTICLE I.

*An Essay on the Shapes, Dimensions, and Positions of the Spaces, in the Earth which are called Rents, and the Arrangement of the Matter in them.* By Mr. John B. Longmire.

(To Dr. Thomson.)

SIR,

I COMMENCED in the year 1812 to write an essay on rents, which I advertised in the beginning of the year 1813 as a work "the object of which was to prove, that metallic veins, dykes, slips, and all other rents, in the internal part of the earth, were formed when it was passing from a fluid to a solid state, and are owing to the unequal contraction of its matter; and that the phenomena of stratification, and formations, in some points of view, as well as the features of the earth at its surface, are effects of the same cause." This work I intended to publish in the beginning of the present year; but since advertising, I have extended it much beyond what I originally intended; and as I purpose to make considerable additions to it, perhaps a few years may elapse before I publish it; but as some of my friends and correspondents have requested its early publication, I have determined, with your permission, to give an abstract of it in the *Annals of Philosophy*. In doing so, I will commence with that part which relates to rents.

I am, Sir, with great respect,

Your very humble servant,

Frontbeck, near Kendal,  
Oct. 30, 1814.

JOHN B. LONGMIRE.

*Of Rents in General.*

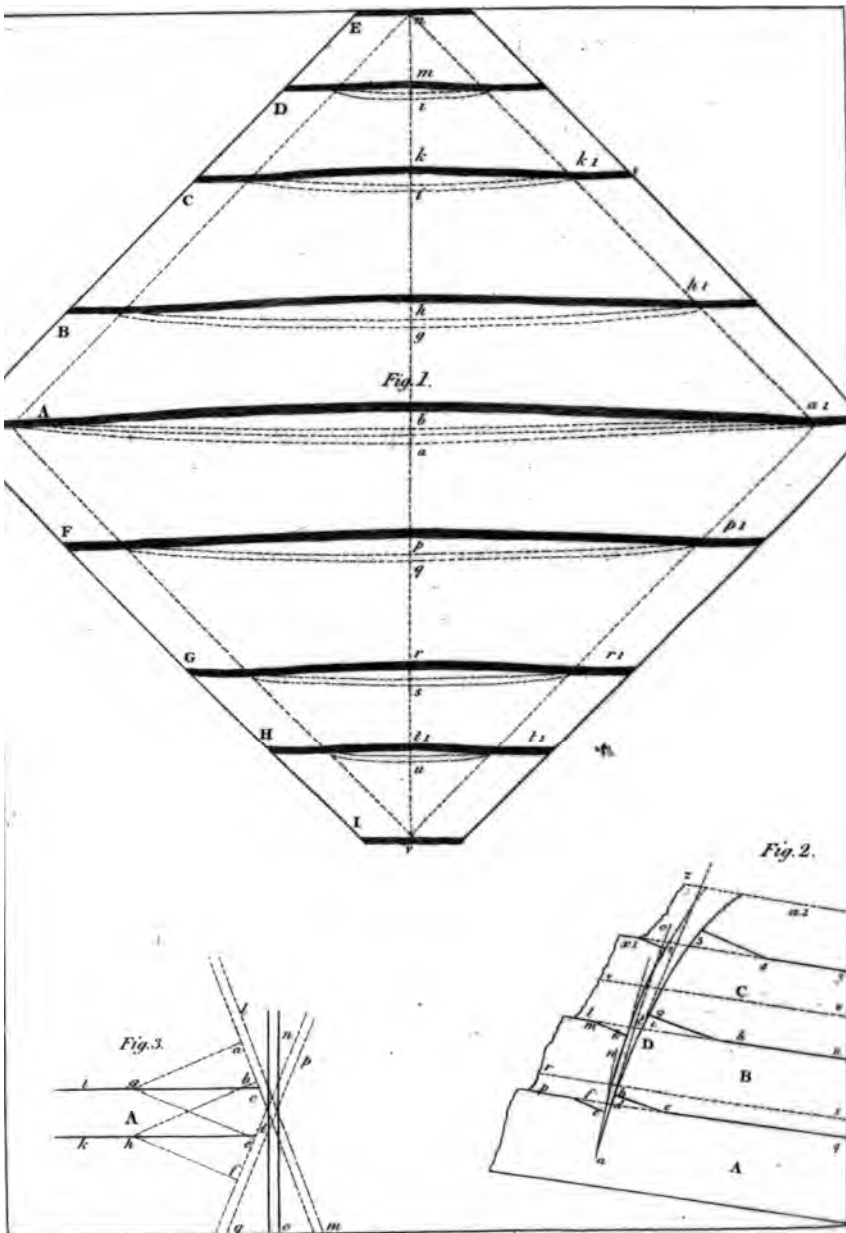
There are numerous spaces existing throughout the earth, many of which extend further from its surface towards its centre than man has hitherto penetrated. These spaces contain irregularly-shaped *tabular masses* of earthy and metallic matter, which have a position very different from that of the matter filling up the other parts of the earth. A close examination of all the phenomena presented by these spaces convinces the observer that they have been formed when the matter of the earth changed its state from fluidity to solidity, and are owing to its contraction unequally. The most of these spaces, or those which were formed the earliest, were filled by some of the matter which is contiguous to their sides being forced into them, from portions of it still nearly in a fluid state, by the weight of the matter incumbent on these portions; the rest of them, or those which were formed when every portion of the matter contiguous to their sides was so far advanced to its present state of solidity as to resist the incumbent weight, were filled with matter that entered them at the earth's surface, in either a fluid state, as the matter of green-stone, basalt, &c.; or a solid state, as gravel, sand, and clay. These spaces, therefore, are rents; and may be called, *according to their shapes*, the bended-tabular, the straight-tabular, the ovalar, and cylindrical, rents.

*Of the Bended-Tabular Rent.*

In treating of this rent I will, in the first part, describe its common names, its shape, dimensions, and position; the difference in the appearance and position of the strata, between where they are contiguous to it, and at given distances from it; and, lastly, the forming cause of the rent. In the second part, I will describe the arrangement of the matter in rents of this shape; dividing them into such as contain matter which proceeded from that on their sides, and such as have been filled from above.

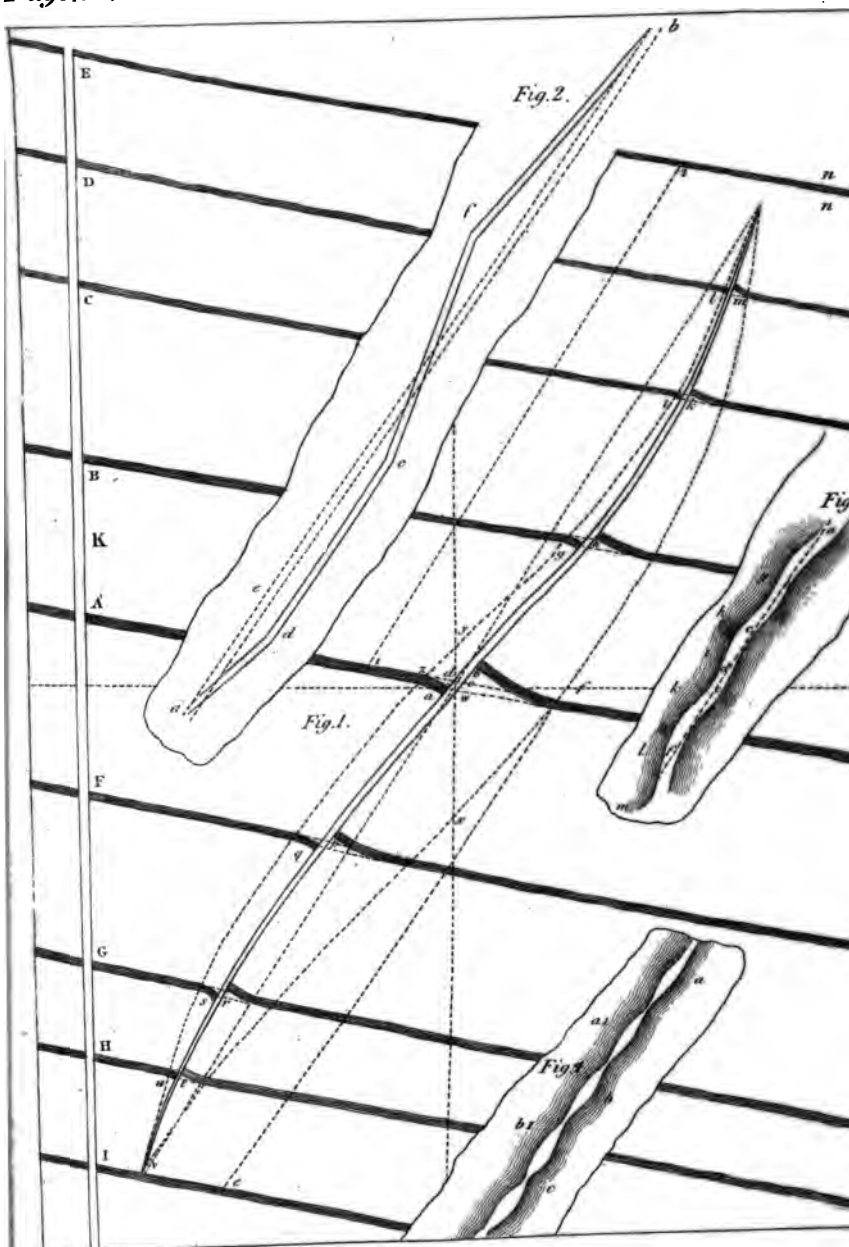
1. *The Names common to this Rent.*—Bended-tabular rents which contain only earthy tabular masses, and metallic and earthy crystals, are known by the appellations of slips, dykes, shifts, lodes, troubles, and faults. They are denominated slips by some geological writers, because the strata on one of their sides have slipped from those on the other, and fallen below them. They are known at Whitehaven by the names of dykes, because they divide the seams, or bands of coal, as they are there called, into *fields*. They also call them *up-throw* and *down-throw dykes*; up-throw dykes, when the strata are higher on the side opposite to that on which the observer is standing; and down-throw dykes, when they are lower on the former than on the latter side. They are called shifts in some parts of England and Scotland, as they are considered by the majority of miners in these parts to have shifted the strata on their sides. In Cornwall they are denominated cross lodes; and in some





M<sup>r</sup> LONGMIRE on RENTS (2)





M<sup>r</sup>. LONGMIRE on RENTS (1)

parts of England and Scotland they are called troubles and faults. Bended-tabular rents, containing earthy and metallic tabular masses and crystals, are generally called metallic veins; and in Cornwall, metallic lodes.

2. *The Shape of this Rent described.*—The shape of this rent is as follows:—Let fig. 1, Plate XXIX., represent a perpendicular view of its angular direction in a situation which is in the middle of its horizontal direction; let the empty spaces between the strata E, D, C, B, &c. be supposed to be filled with strata also, but which are not represented, to avoid confusion in the figure; and let the strata be continued uninterruptedly from the rent,  $n w v$ , to the pit, K. Then if a miner be excavating the stratum E, he may proceed a considerable distance in it, say beyond the point  $o$ , without meeting with a rent; but in excavating the stratum D, he may meet with one at  $l$ ; and if so, he will meet it in the strata C, B, A, F, G, H, at  $i, g, a, q, s, u$ , and in the stratum I, at  $v$ ; there it terminates, probably near the upper surface of this stratum. Let a straight line,  $n f$ , be drawn from the highest to the lowest extremity of the rent. This line passes through the rent at its centre,  $w$ , only, for the rent's upper half is a slightly curved line,  $n m k b w$ , which lies before the straight line; and the lower half is a similarly curved line,  $w p r t v$ , that lies above it. Again, let fig. 1, Plate XXX., represent this rent as a person sees it in approaching its upper side; and let the parts of fig. 1, Plate XXIX., be referred to by the same letters: then the parts of the strata shaded dark in the latter figure are on the under side,  $n, h, p, v$ , of the rent; and the parts represented by the pricked lines only, are on its upper side,  $n, g, q, v$ . The horizontal direction of the rent is greatest opposite its centre, or opposite the stratum A; there let it terminate at A and  $a l$ : then if this distance be bisected, and a line,  $n v$ , drawn at right angles to the points A,  $a l$ , it will bisect the angular direction of the rent. The horizontal direction of the rent decreases in length upwards and downwards from this stratum, with such a ratio, that if four straight lines be drawn, two upwards till they meet in the line,  $n v$ , at  $w$ , each meeting it at an angle of  $45^\circ$ , and two downwards, till they meet the same line at  $v$  under angles of  $45^\circ$  also, these lines will enclose a square figure; whose position is such, that two of its opposite angles, A and  $a l$ , are situated both on the same horizontal line, and the other two,  $n$  and  $v$ , both on the same perpendicular line. There is a general increase in the width of this rent in every direction, from the lines that join together its sides to its centre. This, therefore, is a square bounded bended-tabular rent; whose width is greatest at its centre, and decreases to the lines that join together its sides.

Such is the general shape of every bended-tabular rent; but particular parts of the angular directions of these rents deviate more or less from the curved line just described, and their horizontal directions deviate also from straight lines, while the widths of these

and other parts vary much from that general increase which begins at their lines of termination and ends at their centres.

Both directions of this rent are crookedly and curvilinearly bent. I will describe two of the principal bends. Let the dotted lines, *a, b*, fig. 2, Plate XXIX., represent the angular direction of a part of this rent, which being a small part of the whole, reduced to so small a magnitude, when compared with its real size, the curve peculiar to this direction cannot be observed. Suppose the rent's angular direction in the first part make an angle, *c d a*, of about ten degrees with its general direction, *a b*; it may then be parallel to this direction a greater distance, *d c*; then incline to, and cross it, making with it as small an angle as before, during a greater or less distance, *c f*; it then inclines to and re-crosses it, making only a small angle with it, for a considerable distance, *f b*. These bends are common to the horizontal, as well as the angular, direction of this rent, and they may be thus accounted for: The parts which differ in appearance, consistence, &c. from the characteristic parts of the strata, originally laid in these crooked directions; and as these parts caused the rents in a manner to be afterwards described, the directions of these rents must necessarily follow those of such parts. Both directions of these rents are curvilinearly bended, generally as fig. 3, Plate XXIX., represents.

The variations in the width are mostly similar to those described as follows: The width of this rent will often decrease opposite a few strata till the sides are nearly in contact; it will then increase opposite a few more strata, till it be as great or greater than before; it will then decrease and increase alternately, with different ratios, during which its sides will occasionally meet, and remain in contact for variable distances. The widths of both directions of a rent are subject to these variations; and either of them will be well represented by fig. 4, Plate XXIX., which in this instance is a part of the angular direction.

3. *The Dimensions of Bended-Tabular Rents.*—Bended-tabular rents exist of all dimensions, from a few feet to several hundred yards in both directions; but their widths are generally from two to four feet, and seldom exceed four yards, although there are a few instances in Cornwall of their swelling out to ten yards. The following may be considered large rents: Two or three rents have been traced in, and beyond, the Whitehaven coal-mines about 3000 yards in their horizontal directions; and one called Saltom North Dyke is known to be more than 550 yards in its angular direction; but how much more cannot be now ascertained: and Professor Jameson mentions two near Freyberg, in Germany; one of which is more than four miles, and the other more than ten miles, in this direction. Now the extent of the largest mine, whether one of those which are used for the abstraction of ores out of rents, or useful minerals from the strata themselves, is far from being equal to the large of these rents; of course, the observations of miners

cannot have been so numerous, nor so perfect in their nature, as to describe with sufficient accuracy the general laws, much less the peculiarities of the laws, respecting the figures of the larger rents, and other necessary particulars; but many mines, probably, are sufficiently large to enable skilful miners to determine the particulars necessary to describe all phenomena peculiar to the smaller rents, and to show that the larger correspond with the smaller in every particular known of the latter rents. This is particularly the case with the Earl of Lonsdale's coal-mines at Whitehaven,\* in which I made the many new observations respecting the shapes and positions of this rent, and the peculiar arrangement of the strata contiguous to them, which will appear in these essays.

4. *The Positions of Bended-Tabular Rents.*—The most of these rents have angular positions; but some have a position which is angular in one part and perpendicular in another; others, one that is angular in one part and horizontal in another; and others, one which is angular in one part, perpendicular in another, and horizontal in a third. The positions of these rents depend on those of the strata which form their sides; if, as Williams says, “the strata lie in a horizontal position, the fissure of the slip,” or the position of a rent, “will lie nearly perpendicular; but if the strata hang with any considerable degree of declivity, the fissure of the slip will be found in a slanting direction, as all these sections cut transversely, or right across the strata, or nearly so.” This law arises from that which follows: The surfaces of separation of the strata contiguous to the sides of this rent are at right angles, or nearly so, to the upper and under surfaces of these strata. Therefore when the stratum A, fig. 3, Plate XXX., is horizontal, as when it is represented by the letters *i g c d h k*, its small surface, or surface of separation, of which *c d* is the perpendicular direction, and which is at right angles to the stratum's upper and under surfaces, will give a perpendicular position to the rent, *n o*, of which it forms a part of one side. On the same principle, when the stratum A, is bent upwards till it acquire the position represented by the letters *i, g, a, b, h, k*, it will give an angular position to that part of a rent, (the part *l m*), where it forms a part of one of its sides. If this stratum be bent downwards till it be represented by the letters *i, g, e, f, h, k*, the side of the rent, *p q*, of which it forms a part, will be angular also; but in this instance the angular direction

\* In the Whitehaven coal-mines three strata of coal are worked: the first, the thickness of which varies from four to six feet, is called the Bannock Band, and appears at the earth's surface at the highest part of the mine, but is more than 90 fathoms below it at the lowest part: the second stratum, whose thickness varies from six to twelve feet, is called the Main Band, and is situated at a distance below the highest band, that, if taken in different places, will vary from 11 to more than 20 fathoms: the third, the Six Quarters Band, is from four to six feet in thickness; and is situated in some places 30, in others 40, fathoms, below the Main Band; and in others any distances between these extremes. The surface area, under which the first two named bands are excavated, is not less than from ten to twelve square miles. This is an area of mine, not to be equalled, I believe, by that of any other proprietor.

points to the reverse side of a perpendicular line,  $no$ , to that to which the angular direction in the second instance points; and if the stratum be bent till it be perpendicular, the side of the rent where its surface of separation is situated will be horizontal.

5. *The Difference in the Position and Appearance of the Strata between where they are contiguous to, and at given Distances from, these Rents.*—The difference in the position of the strata is such, that on the under side of all rents of this shape they are bent upwards toward these rents; and on their upper sides, downwards toward them. This is the reason why miners never meet with two parts of one stratum *opposite each other* on reverse sides of a rent of this shape, but one part always a variable distance above the other. The part of a stratum is very little bent downwards, to what it is bent upwards, towards this rent. Both parts of a stratum are bent: the greatest distance opposite the middle part of a rent; and the least, near its highest and lowest extremities; and they are also the least bent near the horizontal extremities, and the most at the centre of this rent. These positions will be better understood by having reference to a diagram. The part,  $bf$ , of the stratum A, fig. 1, Plate XXIX. which is opposite the centre of the rent,  $nwv$ , is bent upwards; the parts  $h, k, m$ , of the strata B, C, D, are also bent upwards; but the further they are individually situated from the middle stratum the less they are bent. The same rules hold good with the parts of the strata  $p, r, t$ . Again, the part  $za$  of the stratum, A, on the upper side of the rent, is bent downwards a very little, but more than any of the strata, either above or below it, as the ratio of the strata's bending decreases upwards, and downwards from the stratum A, to the strata E and I, which are not bent near this rent. Again, the surfaces of separation of the stratum A, are the greatest distance asunder at  $a b$ , fig. 1, Plate XXX., or opposite the centre of the rent; and the least, near its horizontal extremities, A,  $a l$ . The same rule applies to all the strata opposite this rent, either above or below the stratum A.

The appearance and hardness of that part of every stratum which is close to this rent are different to the part of the same stratum that is situated more remotely from it. These differences are owing to corresponding differences in their proportions of elementary matter; for on examining the strata we find one part of them has sometimes one class\* of elementary matter, and often one denomination, or more than one, of the class which the other parts want; and that the proportions of the different denominations of the classes peculiar to both parts are different in one to what it is in another part of a stratum. This arrangement of the elementary matter of a stratum is found in different parts of all strata, but is most observable near rents. These differences have been the most

\* The classes of the earth's elementary matter are generally divided into four—earths, metals, inflammables, and salts. The different denominations of these classes may be silice, alumina, &c.; iron, copper, lead, &c.; or bitumen, carbon, &c.

accurately examined in common coal. Sometimes we find that part of a stratum of coal which is contiguous to this rent contains only carbon and small portions of iron, and earthy matter; while the parts more remote contain carbon, *bitumen*, and the other ingredients peculiar to common coal. Again: near this rent the coal often contains ten per cent. of earthy matter; but this is not the case with it further from the rent: and coal sometimes has its portion of iron increased to five or six per cent., but wants its bitumen; then its appearance and consistence approaches very near to those of graphite, or black lead.\* The differences in the appearance of one part of every stratum, when compared with that of another, is very various; and the nature of these differences in different strata is as various; but in this particular they all agree with one another, that the parts near the rents which differ from the characteristic parts of the strata have the least degree of hardness, and would be the least able to resist any force that may have acted against the attraction of cohesion. All the strata are not wholly different near this rent to what they are at given distances from it. We pass through a rent in many places near its centre, sides, or higher or lower extremities, without perceiving an alteration in the strata on either side of it; but after much examination of them, I have satisfied myself that there is the following general law respecting them: the parts which are different to the rest on the under side of the rent are generally in the greatest abundance opposite its lower half, but these parts are opposite the higher half on the upper side of this rent. In fig. 1, Plate XXIX., the lines *b f*, *f e*, and *e v*, will represent the extent of the differing parts opposite the lower half, and the lines *a 1*, *12*, and *2 n*, the extent of these parts opposite the upper half of this rent.

6. *The forming Cause of this Rent deduced from the foregoing Facts.*—When we consider that the positions of the strata above and below the small bended-tabular rents, have not been altered by the force which altered the positions of those on their sides, and that it has altered the positions of these for small distances only, we must conclude, that had it acted either from above or below the rents, or from either of their sides, it could not have formed them, by producing such alterations in the positions of the strata adjoining them. We must, therefore, define the forming cause of this rent, one that has acted only in the altered parts of the strata. This cause *could* be no other than the matter's contraction, unequally, in changing its state from fluidity to solidity.

During the change of the matter from fluidity to solidity, there would be a period of time when its consolidating process was so far

\* Many such differences in other strata have been described by mineralogists. The parts which differ very much from the rest they have supposed to be parts changed by some chemical agent; but I consider them as *original* differences, and the cause of all rents; because the strata, in the parts very different from the rest, would diminish in bulk, less or more in the former parts than in the latter, as is afterwards described.

advanced towards completion that if any separations took place in it the parts into which it was separated could continue so. At that time the attraction of cohesion in the middle parts of strata could not be so powerful as to draw the matter towards their centres as they diminished in bulk; they would, therefore, be gradually separated into masses of such dimensions that the attraction of cohesion could draw the matter of each towards its centre from every side as it continued to contract.

The *diminution of bulk*, or the *contraction* of the strata, is very evident from the rents or spaces between the numerous masses into which they are divided. The contraction of strata horizontally can be exactly ascertained. Thus if the length of the curved line,  $fb$ , be taken and laid in the direction  $fz$ , it will reach to  $c$ , and the curve line,  $za$ , similarly disposed will reach to  $d$ , so that there will be a distance,  $dc$ , between them, which is the *horizontal contraction* of the part,  $zf$ , of the stratum A. A distance between similar parts of all other strata on the sides of this rent is also observed; the length of which is more or less, according as its situation is more or less distant from the centre of the rent. And although we have no *direct* proof that the strata have contracted perpendicularly; because in this direction its weight has no tendency to counteract the force of cohesion, therefore no separation would take place; yet as the strata have contracted horizontally, we must infer their contraction perpendicularly, as it is impossible to conceive that they could contract in the former, and not in the latter direction. But, as will be afterwards shown, because the contraction horizontally is greater in one part than in another, it is equally probable that the contraction perpendicularly would be as unequal. This is precisely the case: for a stratum lies in different levels on opposite sides of all rents of this shape. Hence the strata on one have contracted a greater distance perpendicularly than on the other of their sides.

The formation of the rents of this shape by the contraction, unequally, of the matter contiguous to their sides is a consequence resulting from the arrangement of the strata near them; but the differences in the classes, and the proportions of each class of elementary matter of the parts of the strata near the sides of rents, when compared with those of these at given distances from them, gave rise to the unequal contraction of the strata; for the parts of strata which differ from others in the proportions of their usual classes of elementary matter must have contracted more or less than them; and those also which have a different class of matter, and different proportions of the classes common to both, must have contracted more or less also. Thus if the part of a stratum of coal, with its usual proportions of elementary matter, was joined to a part wanting a considerable quantity of bitumen, but had an extra portion of earthy matter to make up this deficiency; these two parts would not contract equally in passing from a fluid to a solid state, and of course they would separate where they joined each other.

Many instances might be brought forward to show the cause of the unequal contraction of every stratum ; but the one adduced is sufficient to establish the principle. Having made these premises on the nature of the unequal distribution of the elementary matter of strata, I will now show the mechanical part of the process by which it produced the peculiar arrangement of the strata near the bended-tabular rent ; and, in consequence of it, the formation of this rent.

Suppose the state of solidity of the matter such that the formation of rents had commenced, and that the rent, D, fig. 2, Plate XXX., was produced in a place where the matter's attraction of cohesion was least able to resist the separating force. Let the dotted lines,  $rs$ ,  $vw$ ,  $zal$ , be the upper sides of the strata A' B C, when the formation of the rent, D, commenced ; and the lines  $pfebcq$ ,  $tmkghu$ , and  $x1234y$ , those of these of the strata when they had acquired their present state of solidity. The formation of the rent first commenced in the stratum A ; and after the surfaces of separation were formed, the part  $bc$  contracted perpendicularly less, and the part  $fe$  a very little more, than did the stratum A, in general. In consequence, then, of the contraction, horizontally and perpendicularly, of this stratum, the rent  $bae$  was produced ; whose under side,  $bd$ , is at right angles to the part  $bc$ , and whose upper side,  $ae$ , is at right angles to the part  $fe$  ; and as the end,  $e$ , of the part  $fe$  is not so far below the line  $fc$  as the end,  $b$ , of the part  $bc$  is above it, the angular direction of the sides,  $ba$  and  $ed$ , of the rent, D, meet at the point,  $d$ , from a width,  $ed$ , at their upper extremities that is nearly equal to the horizontal contraction of the part  $fc$  of the stratum A. The arrangement of elementary matter in the stratum B being similar to its arrangement in the stratum A, a rent would take place in the former immediately above that in the latter stratum. Like the stratum A, the stratum B would contract less in one part,  $gh$ , than in another,  $mk$  ; but although its thickness be the same as that of the stratum B, the distance  $gi$  is twice the distance  $bd$  ; because, besides that it has contracted as much less in that part than in other parts, like the stratum A, the progress towards the present state of solidity of the stratum A was greater in the same period of time than that of the stratum B ; consequently the part  $cbgh$  of this stratum was raised as much above the line  $dc$  as the line  $bc$  is above the line  $fc$ , or as much as it contracted less in that part than in general. In such a manner were the parts  $t, r, p, b$ , of the strata H, G, F, A, fig. 1, Plate XXIX., raised above the other parts of the same strata. Again, the distance which the point  $k$  is below the line  $mh$ , fig. 2, Plate XXX., is twice that which the point  $e$  is below the line  $fc$  ; because the part  $mkef$  of the stratum B has contracted as much as the same part of the stratum A, and is as much let down below the other parts of the stratum B, by the unequal contraction of the stratum A, as the point  $e$  is below the line  $fc$ . In this manner were the parts  $u, s, q, a$ , of the strata H, G, F, A, fig. 1, Plate XXIX., allowed to

descend below the other parts of the same strata. But as the ratio of contraction of the parts  $h, k, m, n$ , of the strata B, C, D, E, is as much less than the general ratio of all the strata on the under side of this rent, as that of this of the strata below the stratum A is greater than it; so the elevation of the first stratum above the stratum A would not be so much as the elevation of this stratum by the difference of the ratio; nor would the third stratum above the stratum A be so much elevated as the second; nor the fourth, as the third; so that the elevations of the strata above the stratum A, as their distances from it increased, would be less; till at last no alteration in their positions could be observed. Again, as the ratio of contraction of the parts  $g, i, l$ , is as much greater than the general ratio as that of this of the strata below the stratum A is less than it; so the first stratum above this stratum would not be suffered to descend so much as it by the difference in the ratio; nor would the third stratum above the stratum A descend so much as the second; nor the fourth, so much as the third; so that at the distance above the stratum A which is equal to that between this stratum and the lowest extremity of the rent, no alteration in the general position of the strata would take place.

Upon this alteration in the position of the strata depend the *shape* and *position* of these rents. Let the line  $bo$ , fig. 2, Plate XXX., be a continuation of the angular direction of the separated surface of the stratum A on the under side of the rent D; then as the line  $gh$  makes an angle,  $gi$ , with the line  $hm$  that is greater than the angle  $bcd$ , which the line  $cb$  makes with the line  $cf$ , the last of which is parallel to the line  $mk$ : so an angle,  $og$ , is produced, which is the deviation of the stratum B's surface of separations' angular direction from that of the separated surface of the stratum A; and as the angle  $lmk$  is greater than the angle  $fed$ , so the angular direction,  $ek$ , of the stratum B on the upper side of the rent makes an angle,  $ken$ , with that of the stratum A, which, being less than the angle  $obg$ , the rent is wider at  $li$  than at  $de$ . Again, the angular directions of the stratum C's separated surfaces deviate as much from those of these of the stratum B as these directions of its surfaces of separation do from the same directions of the separated surfaces of the stratum A. Hence the angular direction  $abq$  3 is a curved line, the ratio of whose deviation from the line  $bo$  increases opposite every stratum above the stratum A; and the angular direction,  $ae$  2, of the upper side of the rent is a curved line also, which deviates from the line  $en$  with an increasing ratio, but one not so great as that of the under side of the rent; the width of this rent, therefore, increases upwards. In this manner the lower half of the rent,  $n w v$ , fig. 1, Plate XXIX., was formed; and in a similar manner, with the following exception, its upper half was formed. But as in the upper half, the ratio of contraction on the under side being greater, and on the upper side less, than the general ratio of contraction on similar parts of the lower half of the rent, the sectional lines which re-

present the angular direction of its upper half present a similarly disposed curve as its lower half, but it has a reverse direction; and the width of the upper half decreases from its maximum at the middle to a point at the highest extremity of the rent.

As the part of any one of the strata on the under side of this rent below the stratum A has raised that which lies upon it as much as it was itself raised by that on which it lies, with the additional distance of what it contracted less than that produced by the general ratio of contraction for a distance equal to its thickness; and as its correspondent part on the upper side of the rent has let the stratum immediately above it descend as much as its own descent below the general line of the stratum, with the additional distance of what it contracted more than the strata in general; it follows that the distances between the surfaces of separation of these strata are the respective measures of the *matter's unequal contraction* between them and the lowest extremity of the rent. Thus the distance,  $ab$ , between these surfaces of the stratum A is the measure of the unequal contraction of the matter between that stratum and the lowest extremity,  $v$ , of the rent. The distances  $p q$ ,  $r s$ ,  $t u$ , between separated surfaces of the strata F, G, H, are the measures of the matter's unequal contraction between these strata and the rent's lowest extremity respectively; and as much as the distance between the surfaces of separation of any one of these strata and the point  $v$  is less than the distance between the separated surfaces of the stratum A, so much is the distance of this stratum from the point  $v$  less than that of the stratum A. Again, as the part of any one of the strata which are situated above the stratum A on the under side of this rent has raised the stratum lying upon it as much as it was itself raised by that which lies below it, with the deduction of the distance of what its ratio of contraction is more than the general ratio; and as its correspondent part on the upper side of the rent has permitted the stratum immediately above it to descend a distance equal to that which itself descended below the general line of the stratum, with the deduction of what its ratio of contraction is less than the given ratio; it follows that the distance between the surfaces of separation of any one of these strata is the measure of the unequal contraction between that stratum and the bottom of the rent; and the difference of the distance between its surfaces of separation and that between the separated surfaces of the stratum A, is the measure of the decrease in the amount of the unequal contraction between that stratum and the stratum A: the first of which differences increases, and the second decreases as the distance of each from the stratum A increases, but both cease with the rent. The power, therefore, of the unequal perpendicular contraction of the matter to displace the strata commences at the rent's lowest extremity, accumulates in effect from that place to the middle, but from thence decreases upwards, and ceases with the rent.

As the lines,  $nfv$  and  $vzn$ , which represent the lines of distinction between the parts of the strata that have the regular posi-

tion, and those which are bent towards the rent, are widest in the middle, and meet at the highest and lowest points of the rent, so the distances from the rent to which the strata are bent are greatest opposite its centre, and decrease gradually to its highest and lowest extremities. Again, as the sphere of the horizontal contraction has extended as far as the strata are bent, and as the distance from the rent to where the contraction in this direction commenced, is greatest in the middle of the rent, and decreases upwards and downwards from this place; so the distance which the strata have contracted horizontally is greatest at the middle, and decreases gradually towards the highest and lowest extremities of the rent.

From what I have seen of the smaller rents of this shape, in general when the distance  $fw$  is four yards, the distance  $dc$  is two feet, the distance  $vb$  90 yards, and the distance  $ab$  three yards. From these data the contraction horizontally is one-sixth of the distance in which that contraction took place. How much the contraction is perpendicularly we have no direct means of ascertaining; but that it is as much in this as in the horizontal direction is extremely probable; and on this supposition the distance which the strata have contracted less on the under than on the upper side of this rent is one-fifth of their whole contraction in this direction.

The matter near this rent has contracted more horizontally in one part than in another. Thus its horizontal contraction is greater opposite the part  $b$ , fig. 3, Plate XXIX., than opposite the part  $c$ ; and more opposite the part  $d$  than the part  $e$ ; and so on. The inequality in the matter's contraction in this direction is from one-fifth to one-sixth of its whole contraction. This inequality gives the curvilinear bends to *both* sides of a rent, which necessarily produce inequalities in its width. But that arrangement of these inequalities represented by fig. 3 was formed by the inequalities of the matter's contraction taken perpendicularly as well as horizontally; because similar bends in the sides of this rent are never *wholly* opposite each other, but always situated lower on the upper side than on the under side; and this arrangement is owing to the circumstance before mentioned, that the parts of the strata on the former side, or the side  $gm$ , have contracted more perpendicularly or sunk lower than on the latter side, the side  $af$ , of the rent. When the hollow parts on the upper side of a rent have sunk directly opposite similar hollows on the rent's under side, the variation in the width before described and represented by fig. 4, Plate XXIX., is produced; but corresponding hollows are not opposite each other; for the hollow  $a$  was originally opposite the hollow  $a_1$ , &c. There are many other variations in the ratio of the strata's bending, and in the regularity of the widths and positions of bended-tabular rents, which depend upon certain modifications of the principles already laid down. Some of these variations I will afterwards describe in separate essays.

## ARTICLE II.

*Experiments to determine the Definite Proportions in which the Elements of Organic Nature are combined.* By Jacob Berzelius, M. D. F. R. S. Professor of Chemistry at Stockholm.

(Continued from p. 409, Vol. IV.)

III. *Analysis of the Ternary Oxides.*

1. *Analysis of citric acid.*—I shall begin with citric acid, because its composition is the most simple of all the ternary oxides. Ten parts of citrate of lead, strongly dried and decomposed by sulphuric acid, produced 8·9346 parts of sulphate of lead, equivalent to 6·582 of oxide of lead. Hence it follows that citrate of lead is composed of

Citric acid .....	34·18	.....	100
Oxide of lead .....	65·82	.....	190
			<hr/>
			100·00

These 190 parts of oxide of lead contain 13·588 parts of oxygen. In analyses made by combustion I found only 187 or 188 parts of oxide of lead combined with 100 acid. Citrate of lead retains humidity very strongly, and hence analysis makes the base always too small. As this analysis was one of the first that I made, I had not at that time found out the method of drying the substances in a sand-bath heated and exposed in a vacuum.

I made a great many experiments on the citric acid, because this acid and the tartaric served as a kind of exercise to prepare me for these experiments. A part of them was unsuccessful, because I had not at that time acquired the necessary practice in analysis. One among the successful experiments gave as the result of the analysis of 1 part of citrate of lead (which by combustion left for residue 64·956 per cent. of oxide of lead, and in which there was of course 35·044 per cent. of acid,) 0·1145 water, and 0·503 of carbonic acid in the state of gas; to this if we add 0·03 for the carbonic acid retained by the soda, we obtain 0·533 of carbonic acid. This shows us that citric acid contains per cent.:

Hydrogen .....	3·800
Carbon .....	41·369
Oxygen .....	54·831
	<hr/>
	100·000

We have seen that the capacity of saturation of this acid is 13·585; but  $13·585 \times 4 = 54·34$ . This shows us that citric acid ought to contain four times as much oxygen as the base by which it is neutralized. When we turn these numbers into

volumes, we find that they agree perfectly with the idea that citric acid is composed of equal volumes of its elements; that is to say, it ought to contain per cent.:

Hydrogen .....	3.634
Carbon .....	41.270
Oxygen .....	55.096
	<hr/>
	100.000

If this be so, citric acid has the simplest composition of any of the ternary oxides. Its capacity of saturation indicates that it ought to contain four volumes of oxygen, four volumes of carbon, and four of hydrogen. Must we consider it as a compound of 12 atoms or of three? Experiments on the combination of citric acid with water seem to prove that it contains only three atoms, an atom of each constituent.

When we compare the result of my former experiments on the quantity of water contained in citric acid, we find that they do not coincide with the result of the analysis of citrate of lead: 10 parts of citric acid in crystals dissolved in water, neutralized by ammonia, and mixed with nitrate of lead, as long as any precipitate fell, produced 23.756 of dry citrate of lead. In this citrate there is 8.3 of citric acid; that is to say, that 100 parts of acid contain 17 of water: or that 100 parts of acid combine with 20.5 of water, which contain 18.1 oxygen. But  $18.1 \times 3 = 54.3$ ; that is to say, that in crystallized citric acid the acid contains three times as much oxygen as the water.

I took five parts of citric acid reduced to a coarse powder, and exposed them in a glass capsule exactly weighed to a temperature between  $118^{\circ}$  and  $122^{\circ}$ . In 24 hours it lost 0.43 of its weight, and had assumed the form of a white light powder. In another experiment the loss was 0.424. No further loss was sustained by longer exposure to heat. This experiment shows that citric acid by efflorescing loses from 8.58 to 8.6 per cent. of its weight of water, which is obviously the half of the water of crystallization contained in the acid. Citric acid does not part with the rest of its water; for if we heat it to a certain temperature, it loses indeed weight, but at the same time it becomes brown, is decomposed, and leaves a brownish deliquescent mass, no longer possessing the properties of citric acid. In order to expel the water we must substitute another oxide in its place. From these experiments it follows, that in effloresced citric acid, or *citrate of water*, as it may be called, the acid contains six times as much oxygen as the water, and that in crystallized *citrate of water* the acid contains three times as much as the water.

This property of citric acid to allow itself to be saturated by water in another proportion than by the stronger oxides is peculiar; but it is easily explained by the simplicity of the composition

of citric acid, and cannot take place in other circumstances. The composition of citric acid may then be expressed by this formula,  $H + C + O$ .

My expectation of being able to illustrate the composition of this acid by the examination of a subcitrate of lead was disappointed, by the property which the neutral citrate has of dissolving in ammonia, and of forming a triple salt, from which the ammonia could not be driven off even in a vacuum.

Thenard and Gay-Lussac, in their experiments on citric acid, obtained as a result 6.33 hydrogen, 33.811 carbon, 59.859 oxygen. This result differs very much from mine. Their analysis of the citrate of lime employed by them shows that very little water of combination remained in the salt. Besides, a correction for the water of combination remaining in the salt would only increase the difference between our results. I am unable to divine the cause of this difference.

2. *Tartaric acid*.—100 parts of tartrate of lead analysed by combustion, left 62.5 of oxide of lead in one experiment, and 62.48 in another. This differs but little from my old experiments, in which I found 62.2 per cent. of oxide of lead in this salt. It is composed then of

Tartaric acid .....	37.5	.....	100
Oxide of lead .....	62.5	.....	167
			<hr/>
			100.0

But 167 of oxide of lead contain 11.94 of oxygen. In one of my old experiments I found that 100 of tartaric acid are neutralized by 70.4 of potash, in which there are 11.93 oxygen. I have shown likewise in these experiments, that tartaric acid must contain five times as much oxygen as the base by which it is saturated.

$1\frac{1}{2}$  of tartrate of lead, equivalent to 0.5 of tartaric acid, produced in different experiments from 0.161 to 0.162 of water, equivalent to from 3.79 to 3.807 per cent. of the weight of the acid of hydrogen. In the same experiments I obtained from 0.654 to 0.661 of carbonic acid, equivalent to from 35.36 to 35.98 per cent. of the weight of the acid of carbon. The acid is then composed of

Hydrogen .....	3.807
Carbon .....	35.980
Oxygen .....	60.213
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	100.000

But  $11.94 \times 5 = 59.7$ . If we examine the ratio of these numbers, we find that the hydrogen is to the oxygen :: 66 : 1000; that is to say, that there is a volume of each. The carbon constitutes  $\frac{1}{3}$  of a volume. But if the oxygen is in reality five volumes, it is combined with five volumes of hydrogen and four volumes of carbon. Supposing then tartaric acid a compound of  $5 H + 4 C + 5 O$ , calculation gives its composition as follows:—

Hydrogen .....	3.951
Carbon .....	36.167
Oxygen .....	59.882

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100.000

The true capacity of saturation of this acid is then 11.976, instead of 11.94 given by experiment. It follows from this, that in the neutral tartrates the carbon of the acid is combined with  $1\frac{1}{4}$  volume of oxygen, including the oxygen of the base. I have not been able to obtain a subtartrate of lead.

Let us now examine the result of the analysis of this acid made by Thenard and Gay-Lussac. They found it composed of hydrogen 6.629, carbon 24.05, oxygen 69.321; but they employed tartrate of lime, in which they supposed 77.577 of tartaric acid. That is to say, that they paid no attention to the water of combination of the citrate of lime. To find the quantity of this water I made the following experiments.

I dissolved neutral tartrate of potash in water, and precipitated it by muriate of lime; the neutrality was not altered. Hence it follows, that 100 tartaric acid saturate a quantity of lime containing 11.976 of oxygen; that is to say, equal to that in the potash separated from the tartaric acid. I burnt 100 parts of this tartrate of lime in a platinum crucible, till the lime was reduced to a caustic state. To be quite sure of getting rid of all the carbonic acid, I allowed the lime to unite with a little water, and exposed it again to a strong heat; its weight was not altered. It weighed 21.64 parts, and dissolved in diluted muriatic acid without the smallest disengagement of gas. 21.64 parts of lime require for saturation 50.55 of tartaric acid; hence it follows, that tartrate of lime is composed of

Tartaric acid .....	50.55
Lime .....	21.64
Water of combination .....	27.81

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100.00

But 21.64 of lime contain 6.094 of oxygen, and 27.81 of water, 24.54. Now  $6.094 \times 4 = 24.376$ ; that is to say, that the salt contains a quantity of water of combination, the oxygen in which is four times that in the lime. It follows from this, that what Thenard and Gay-Lussac considered as 100 tartaric acid, was in fact a mixture of 65.46 of acid with 34.54 of water. If we subtract this proportion of water from their result, we obtain tartaric acid composed of

Hydrogen .....	3.912
Carbon .....	36.888
Oxygen .....	59.200

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100.000

*which agrees perfectly with the result of my experiments.*

3. *Oxalic acid*.—My former analytical experiments on the oxalate of lead, indicate 25·2 of oxalic acid in 100 of oxalate of lead. The analysis was made by combustion, but without attending to the lead which was reduced and concealed in the oxide of lead obtained. Later experiments on four parts of oxalate of lead gave me a loss amounting to 0·9835; the oxide being dissolved in vinegar, 0·095 of metallic lead remained. This indicates 0·009 of oxygen, which must be subtracted from the loss in order to obtain the true quantity of acid; of course it amounted to 0·9816, or 24·54 per cent. of the weight of the oxalate. This result was constant in various experiments. The oxalate of lead then is composed of

Oxalic acid .....	24·54	.....	100
Oxide of lead .....	75·46	.....	307·5
<hr/>			
100·00			

But 307·5 of oxalate of lead contain 22·062 of oxygen. And since according to my old experiments on oxalic acid, it contains three times as much oxygen as the base, it follows that it ought to contain 66·186 per cent.

I mentioned before that the analyses which I made by means of the brown oxide of lead gave me too little oxygen and too much hydrogen and carbon. The result of one of these experiments on oxalic acid was 0·66 per cent of hydrogen, 34·34 of carbon, and 64·99 of oxygen.

In my repetitions by my new method, I burnt four parts of oxalate of lead with two parts of hyperoxymuriate of potash. The quantity of water produced weighed only 0·019; hence I suspected that this oxalate, which had been strongly heated, might have been partly decomposed. I therefore repeated the experiment with an oxalate that had been dried in a temperature below 212°. I obtained exactly the same result. I next suspected that I had taken too little hyperoxymuriate of potash, in consequence of which some empyreumatic oil might have been formed, though no trace of it appeared in the water. I therefore burnt two parts of oxalate of lead with two parts of hyperoxymuriate. I obtained 0·004 of water in the receiver, and 0·001 in the tube with the muriate of lime. So that the result was almost exactly the same, giving 0·02 instead of 0·019 of water from four of oxalate. These four parts contain 0·9816 of oxalic acid; hence it follows, that this acid contains only 0·23, or 0·24 per cent. of hydrogen.

The carbonic acid produced weighed from 1·138 to 1·140, which makes 32·16 per cent. of carbon. A little of the carbonic acid remained, combined with the soda as in other experiments; but this quantity could not be determined, because a portion of the oxide of lead was converted into minium, and gave a red colour to the burnt mass. The colour was the same when I endeavoured to

keep the whole tube red hot during the combustion of the oxalate. Another portion of the oxide was changed into submuriate, as I satisfied myself by digesting the oxide in nitric acid, after it had been well washed. Oxymuriatic acid was disengaged, while at the same time neutral nitrate and muriate of lead were formed.

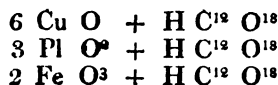
Though in consequence of this circumstance, it is impossible to determine the quantity of carbon with perfect precision, we know at least that it must contain more than 32.6, which would have been the result if all the oxide had been converted into minium, and less than 35 per cent., which would have been the result if no part of the oxide had been converted into minium. By attending to these two extremes, we find without difficulty, that the oxygen and carbon exist in such proportions in oxalic acid, that there is one volume of carbon for every  $1\frac{1}{2}$  volume of oxygen, or two volumes of carbon for every three volumes of oxygen.

To determine the composition of oxalic acid, we must then ascertain the relation between the volumes of its oxygen and hydrogen. This is difficult, because the quantity of hydrogen is so small, that analysis is not able to decide concerning it. I think, however, that we can find it from other circumstances. There can be no doubt that the composition of oxalic acid is such, that in the oxalates the hydrogen of the acid is combined with a certain number of entire volumes of the radicle of the base; but saline bases may contain one, two, or three volumes of oxygen, without this producing any change in the ratio of the oxygen of the acid with that in the base. When we reflect on this, we perceive that with the known ratio between the carbon and oxygen in oxalic acid, there are only three modes of combination in which the above stated circumstance, (that a volume of hydrogen in the acid is combined with a certain number of entire volumes of the radicle of the base,) can take place; namely,  $H + 2C + 3O$ , or  $H + 12C + 18O$ , or  $H + 24C + 36O$ . For the degrees above this are not probable. The difference between these three degrees is so great, that the quantity of hydrogen found by experiment must be sufficiently exact to determine which of them belongs to oxalic acid. But analysis has given much more hydrogen than is consistent with the third of these, but less than would be requisite for the first mode of combination. But all the circumstances of the analysis agree perfectly with the idea, that oxalic acid is composed of one volume hydrogen, 12 volumes carbon, and 18 volumes oxygen.\* This constitutes per cent. :

\* In my memoir On the cause of Chemical Proportions, *Annals of Philosophy*, December, 1813, I have stated by a mistake in the calculation that this acid contains 27 volumes of carbon. It is obvious that this error is of no consequence relative to the subject treated of in that memoir. I acknowledge however that at present I do not lay much stress on that argument against the corpuscular doctrine. For it is clear that the compound atoms of organic nature ought to have a mechanical structure as different from that of inorganic atoms as their composition is different.

Hydrogen .....	0.244
Carbon .....	33.222
Oxygen .....	66.534
	<hr/>
	100.000

It follows from these reflections, that in an oxalate whose base contains only a single volume of oxygen, the hydrogen of the acid is combined with six volumes of the radicle of the base. When on the other side the base contains two volumes of oxygen, the hydrogen of the acid is combined with three volumes of the radicle of the base. When the base contains three volumes of oxygen, then the hydrogen is combined with two volumes of radicle. This may be seen in the following formulas, representing the oxalate of copper, the oxalate of lead, and the peroxalate of iron.



Let us now examine the result of the analysis of this acid by Gay-Lussac and Thenard. They employed in it oxalate of lime dried in the temperature of boiling water. This oxalate in that state contains water of combination, the quantity of which determined both by the experiments of Mr. Vogel of Bayreuth, and by calculation, is such, that it contains a quantity of oxygen equal to that in the lime; that is, very nearly 12 per cent. Therefore the oxalic acid used in their experiments consisted of 80 pure acid + 20 water. If we take away these 20 of water, that is to say, 2.35 hydrogen, and 17.75 oxygen, from the result of the analysis of Gay-Lussac and Thenard, we have

	Result of Gay-Lussac and Thenard.	Do. corrected.
Hydrogen .....	2.745	0.493
Carbon .....	26.566	33.217
Oxygen .....	70.689	66.290
	<hr/>	<hr/>
	100.000	100.000

This result agrees so well with my experiments, that it serves to confirm what I have advanced above.\*

4. *Succinic acid*.—I purified succinic acid from the empyreu-

\* Dr. Thomson in the Philosophical Transactions for 1807, has given an analysis of oxalic acid made by dry distillation and the analysis of the products. He found it composed of

Hydrogen .....	4
Carbon .....	33
Oxygen .....	64
	<hr/>
	100

I am astonished that he could have come so near the truth notwithstanding the difficulties of the method which he followed.

matic matter with which it is usually impregnated, in the following manner. I sublimed it in a moderate heat, the product of the sublimation is succinic acid, contaminated with colourless oil of amber, which becomes coloured by exposure to the air. I combined it with carbonate of ammonia, adding a slight excess of the alkali. I then digested it with charcoal, prepared by exposing coagulated blood to a strong heat in a covered crucible. This charcoal absorbed the empyreumatic matters, and allowed the succinate of ammonia to crystallize of a white colour. From this succinate of ammonia, I prepared by the addition of nitrate of lead, with the precautions already stated, pure succinate of lead. 100 parts of this succinate analysed by combustion gave 69.1 of oxide of lead. Hence the salt is composed of

Succinic acid .....	30.9	.....	100.00
Oxide of Lead .....	69.1	.....	223.62
			<hr/>
			100.0

But 223.62 of oxide of lead contain 16 of oxygen.

Subsuccinate of lead obtained by digesting the neutral succinate in ammonia, dried in a vacuum at the temperature of boiling water, and analyzed by combustion, yielded 86.93 per cent. of oxide of lead. A great excess of ammonia poured upon this subsuccinate did not alter it; for it gave afterwards the same analytical result. The subsuccinate of lead then is composed of

Succinic acid .....	13.07	.....	100
Oxide of lead .....	86.93	.....	666
			<hr/>
			100.00

But  $223.64 \times 3 = 670.8$ , which differs but little from 666. Hence it is evident, that the succinic acid in this salt is combined with three times as much base as in the neutral succinate. Of course succinic acid must contain three volumes of oxygen.

1.294 of succinate of lead, equivalent to 0.4 of succinic acid, produced by combustion 0.1536 of water, equivalent to 4.512 per cent. of hydrogen, and 0.656 of carbonic acid in form of gas, to which adding the 0.044 remaining in the soda, the whole amounts to 0.7, equivalent to 47.6 per cent. of carbon. The acid therefore is composed of

Hydrogen .....	4.512
Carbon .....	47.600
Oxygen .....	47.888
	<hr/>
	100.000

But  $16 \times 3 = 48$ . Hence it follows, that the acid contains three volumes of oxygen. We find that the carbon and hydrogen amount each to four volumes; so that succinic acid is composed of  $4H + 4C + 3O$ , which makes in a hundred parts

Hydrogen .....	4.218
Carbon .....	47.859
Oxygen .....	47.923

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100

The true capacity of saturation of the acid then is 15.9743, instead of 16 given by experiment.

(*To be continued.*)

### ARTICLE III.

*A Memoir on Iodine.* By M. Gay-Lussac.\*

(Read to the French Institute on the 1st of August, 1814.)

I HAVE already, in the sittings of the 6th and 20th of December, 1813, read to the Class the result of my researches† on the substance discovered by M. Courtois, to which I proposed to give the name of *iode*, on account of the beautiful violet colour of its vapour. To these researches, the object of which was to determine the nature of *iodine*, and the situation in which it ought to be placed with respect to other bodies, I have added new experiments, which I have now the honour to communicate to the Class. If there be no occasion to apologize for the length of time which has elapsed since I read my first experiments, I must at least beg your indulgence for the details into which I am about to enter. I regret that my labours have lost part of their interest, by appearing after those of M. Vauquelin, with which I was unacquainted when I drew up my memoir. But it will be at least a satisfaction to me, if, in the subjects which we have treated in common, I have in general drawn the same conclusions as he has done.

#### *Properties of Iodine.*

Iodine when in a solid state is greyish black; but its vapour has a fine violet colour. Its odour is exactly that of chlorine, weakened. It is often in scales similar to those of micaceous iron ore, sometimes in rhomboidal plates, very large and very brilliant. I have obtained it in elongated octohedrons about a centimetre in length. Its fracture is lamellated, and it has a fatty lustre. It is very soft and friable, and may be rubbed to a fine powder. Its taste is very acrid, though it be but very little soluble in water. It gives a deep brown stain to the skin, but this colour is soon removed. Like chlorine it destroys vegetable colours; but with much less energy. Water dissolves about one seven thousandth part of its weight of iodine, and acquires an orange yellow colour. The specific gravity

\* Translated from the *Annals de Chimie*, vol. xci. p. 5. July 1814.

† Printed in the *Annals of Philosophy*, iii. 106.

of iodine at  $62\frac{1}{4}^{\circ}$  is 4.948. It melts at  $225^{\circ}$ , and is volatilized under the common pressure of the atmosphere, at the temperature of  $347^{\circ}$  or  $356^{\circ}$ . I ascertained this last temperature by putting iodine in excess in concentrated sulphuric acid, which has little action on it, and observing at what degree of heat its vapours passed up through the acid, I fixed the boiling point at  $347^{\circ}$  and  $356^{\circ}$ , because these two numbers are the result of two experiments made in circumstances a little different from each other.\* As iodine, when mixed with water, may be distilled over with that liquid, it was at first concluded that its boiling point was nearly the same with that of water; but this is a mistake. At the temperature of  $212^{\circ}$ , the vapour of iodine mixes with the vapour of water in considerable quantity, and is carried with it to the receiver where it condenses. Iodine may even be distilled at a much lower temperature. The same thing happens to the essential oils; though they boil only at about  $309^{\circ}$ , yet they may be distilled over at  $212^{\circ}$  when mixed with water. Iodine seems to be a non-conductor of electricity. When a small fragment of it is put into the galvanic circuit, the decomposition of water immediately stops.

Iodine is not combustible. It cannot even be combined directly with oxygen. I consider it as a simple substance, and place it, in consequence of the experiments which I have made, between sulphur and chlorine; because its affinities are stronger than the former and weaker than the latter of these bodies. Like them it forms two acids, one by combining with hydrogen, and another by combining with oxygen; and most of its combinations have considerable analogy with those formed by these two bodies. The compounds of chlorine, iodine, and sulphur with hydrogen, possessing the properties of acids formed by oxygen, ought to be comprehended in the same class, and under the same acid name. In order to distinguish them I propose to prefix to the specific name of the acid, which we consider the generic term, *hydro*; so that the acid combinations of chlorine, iodine, and sulphur with hydrogen, shall have the names of *hydrochloric acid*, *hydriodic acid*, and *hydrosulphuric acid*. The acid compounds of oxygen with the same bodies, may receive, according to the principles of the nomenclature adopted, the names of *chloric*, *iodic*, *sulphuric acids*. The terms *chlorurets* and *iodurets* may denote the compounds of chlorine and iodine with combustibles or oxides. Thus the *oxymuriate of lime* will be called *chloruret of lime*.

*On the Combination of Iodine with simple Substances, and in particular of Hydriodic Acid.*

Iodine combines with most combustibles; but I have only examined a few of these combinations.

\* These experiments are somewhat hazardous. Though I had put pieces of glass into the sulphuric acid, the iodine in the second experiment was converted all at once into vapour and drove out the hot acid which burnt my right hand and foot severely.

Phosphorus unites to iodine in different proportions, with the disengagement of heat, but of no light. One part of phosphorus and eight parts of iodine formed a compound of a red orange brown colour, fusible at about  $212^{\circ}$ , and volatile at a higher temperature. When brought in contact with water, phosphureted hydrogen gas is disengaged, flocks of phosphorus are precipitated, and the water which is colourless contains in solution phosphorous and hydriodic acids.

One part of phosphorus and 16 of iodine, produce a matter of a greyish black colour, crystallized, fusible at  $84^{\circ}$ . The hydriodic acid produced by bringing it in contact with water, is colourless, and no phosphureted hydrogen gas is disengaged.

One part of phosphorus and 24 of iodine produced a black substance, partially fusible at  $115^{\circ}$ . Water dissolves it, producing a strong heat, and the solution had a very deep brown colour, which was not removed by keeping it for some time in a gentle heat. The proportion of one phosphorus and 16 iodine, results from the ratio between phosphorus and iodine, on the supposition that the phosphorus is converted into phosphorous acid. One phosphorus and 24 iodine should have given me colourless hydriodic acid, on the supposition that the phosphorus was converted into phosphoric acid.\* I had indeed ascertained that phosphorous acid converted iodine into hydriodic acid; but the effect stops or becomes very slow, before all the phosphorous acid is destroyed. This is the reason, that with the proportion one phosphorus and 24 iodine, we obtain a deep coloured acid. It holds a considerable quantity of iodine in solution. We see likewise, that if there form only phosphorous acid, when the phosphorus is in excess, there is formed on the contrary phosphoric acid, whenever the iodine is more than 16 times greater than the phosphorus.

With 1 phosphorus and 4 iodine we obtain two compounds very different from each other. One of them has the same colour as that formed of one phosphorus + 8 iodine, and seems to be the same with it. It melts at  $217\frac{1}{2}^{\circ}$ , and when dissolved in water gives a colourless hydriodic acid, phosphureted hydrogen, and phosphorus, which precipitates in orange yellow flocks. The other compound is reddish brown, does not melt at  $212^{\circ}$ , nor at a considerably higher temperature. Water has no sensible action on it. Potash dissolves it with the disengagement of phosphureted hydrogen gas, and when chlorine is poured into the solution it shows only traces of iodine. When heated in the open air, it takes fire, and burns like phosphorus, emitting white vapours without any iodine. I condensed these vapours in a moistened glass jar, and I could not perceive any iodine among them. This red substance is always obtained when the phosphorus is in the proportion of one to four of iodine. I am induced to consider it as identical with that

\* I have supposed in determining these proportions, that 100 of phosphorus combine with 100 oxygen to form phosphorous acid, and with 150 to form phosphoric acid.

red matter which phosphorus so often furnishes, and which is at present considered an oxide. But I have not compared the two bodies, as I had no oxide of phosphorus in my possession. Very little oxygen seems to be necessary to convert phosphorus into this red matter; and as the phosphorus which I employed, though well wiped, was not quite free from humidity, the formation of this substance during my experiment does not present any difficulty. It is a subject of investigation which deserves to fix the attention of chemists.

In whatever proportions the ioduret of phosphorus has been made, it exhales, as soon as it is moistened, acid vapours, owing to the hydriodic acid formed by the decomposition of the water. To obtain this gas without any mixture of phosphureted hydrogen, it is necessary to employ an ioduret in which the phosphorus at most does not exceed a ninth part. The method which I employ is to put the ioduret into a small crucible, and to moisten it with a little water, or what is better, with water already impregnated with hydriodic acid. We may employ likewise a small bent tube. We begin by putting the iodine into this tube; then reversing it over mercury, we drive out the air which it contains, by introducing a glass rod which almost fills it. The phosphorus is then brought in contact with the iodine, by introducing it through the mercury. As soon as the contact takes place, the two substances combine, and we collect the gas which is disengaged, by putting the extremity of the tube under a glass jar standing in the mercury.\*

As soon as the hydriodic gas comes in contact with mercury, it is decomposed. The surface of the metal is covered with a greenish yellow substance, which is an ioduret of mercury; and if the contact is prolonged for a sufficient time, or if we agitate, the hydriodic gas is completely decomposed. The iodine combines completely with the metal, and there remains a volume of hydrogen gas, which is exactly one half of that of the hydriodic gas. I have made this gas pass upon zinc and potassium, and the result has always been hydrogen gas and an ioduret. From this analysis, and the phenomena which iodine exhibits with hydrosulphuric acid, (*sulphureted hydrogen*), and those of the ioduret of phosphorus

\* When distilling a mixture of hydriodic acid and an ioduret of phosphorus, (made without any determined proportions,) in order to obtain the hydriodic acid, towards the end of the operation cubic crystals were formed in the neck of the retort; they were white, and as transparent as wax. On hot coals they burnt like phosphorus. When put into water, they were instantly decomposed, with a copious discharge of subphosphureted hydrogen gas, and allowing flocks of phosphorus to precipitate. The water contained hydriodic acid. Concentrated sulphuric acid, into which these crystals were plunged, became brown, as happens when it decomposes hydriodic acid. But it soon became reddish yellow and milky, probably owing to the phosphorus which precipitated. I had too few of these crystals to subject them to more numerous trials and determine exactly their nature. I was led to consider them as composed of hydriodic acid and phosphorus, and attempted to obtain them by passing a current of hydriodic acid gas over phosphorus. I obtained the new compound, but its quantity did not correspond with that of the phosphorus and hydriodic acid. Further researches are necessary,

with water, there can remain no doubt about the nature of hydriodic gas.

This gas is colourless. Its odour is similar to that of hydrochloric gas, (*muriatic acid*.) Its taste is very acid. It contains half its bulk of hydrogen, and saturates its own bulk of ammoniacal gas. Chlorine instantly deprives it of its hydrogen. A beautiful violet vapour is produced, and hydrochloric gas is formed. I took its specific gravity by weighing it in a glass vessel the capacity of which I knew, weighing it in succession full of air and of gas.\* I found by this method in a first experiment, that it is 4.602 times heavier than common air; and in a second experiment, more exact than the first, that its specific gravity is 4.443. This specific gravity ought to be a little too great, because notwithstanding the precaution which I took to pass the gas through a glass tube cooled down to  $-4^{\circ}$ , slight traces of moisture were seen in the weighing bottle. These were more conspicuous in the first experiment than in the second. When we compare hydriodic gas with hydrochloric, we obtain a density differing but little from my last experiment, and which ought to be the true one.

I must put the reader in mind in the first place, that from the experiments of M. Thenard and myself, one volume of chlorine by combining with one volume of hydrogen produces exactly two volumes of hydrochloric gas; hence it follows, that the density of this last gas is the mean of that of chlorine and hydrogen. But since a volume of chlorine takes one volume of hydrogen, its ratio to oxygen in bulk will be that of two to one, and its ratio in weight may be immediately deduced from this. We do not know the density of the vapour of iodine; but from experiments to be stated below, I have found that the ratio of oxygen to iodine is 1 to 15.621. Now the density of a demivolume of oxygen being 0.55179,  $0.55179 \times 15.621 = 8.6195$  will represent the density of iodine under the volume taken for unity. If to this density we add that of hydrogen, 0.07321, and take half the sum, we have 4.4288 for the density of hydriodic gas; and it is composed by weight of 100 iodine and 0.849 hydrogen. The density of the vapour of iodine is the greatest of all those with which we are acquainted. It is 117.71 greater than that of hydrogen; and as it is according to the volume of their vapours that bodies combine, this explains why iodine does not take the hundredth of its weight of hydrogen to be converted into an acid. With a denser vapour, as would be the case with that of mercury, the proportion of hydrogen requisite would be still smaller, as in the combination of mercury

\* Let  $p$  be the weight of the vessel full of air;  $P$  its weight full of water: for a first approximation  $P - p$  will express the volume of water which it contains. If  $\delta$  represent the ratio between the density of air and that of water under a given pressure and temperature,  $P - p + (P - p)\delta$  will be a second expression of the capacity of the vessel more exact than the first, and which in ordinary cases is sufficient. Greater accuracy may be attained by the formula:—

$$P - p + (P - p)\delta + (P - p)\delta^2 + \&c.$$

with hydrogen and ammonia. We must not then reject the influence of a very small quantity of matter in a compound, unless it be demonstrated that the density of its vapour bears a considerable ratio to that of the other elements of the compound, or, which comes to the same thing, unless it be demonstrated that its capacity of saturation is small.

Hydriodic gas is partly decomposed at a red heat. The decomposition is complete if it be mixed with oxygen. Water is formed and iodine separated. I find on the other hand, that when iodine and vapour of water are passed through a red-hot porcelain tube, no decomposition takes place. This constitutes the great difference between iodine and chlorine; for this last substance takes hydrogen from oxygen. But the circumstance shows a nearer relation between iodine and sulphur, as oxygen takes hydrogen from both.

Hydriodic gas is very soluble in water. It gives it a great density, and renders it smoking when it is dissolved in great quantity. The most convenient way of obtaining liquid hydriodic acid is to dissolve ioduret of phosphorus in water. It is separated from phosphorous acid by heat. Hydriodic acid is prepared with still greater facility, by putting iodine in water and making a current of hydrosulphuric gas pass through the liquid. This gas gives out its hydrogen to the iodine, and allows its sulphur to precipitate. The liquid is heated to drive off the excess of hydrosulphuric acid. It is then filtered, or the liquid is decanted off the precipitated sulphur. By this method a colourless hydriodic acid is obtained. The acid prepared by these means is not very concentrated, unless care has been taken, in distilling that prepared by phosphorus, not to mix the last distilled portions with the first, which consist of little else than pure water. This acid, like sulphuric, may be concentrated by evaporation. While its temperature is lower than  $257^{\circ}$ , the liquid that distils over may be thrown away, because it contains very little acid. Above this point the acid itself begins to distil over, and the temperature remains constant at  $262\frac{1}{2}^{\circ}$ . The specific gravity of the acid is then 1.7, and it does not sensibly vary. This property of hydriodic acid to boil only at  $262\frac{1}{2}^{\circ}$ , renders it a powerful acid, and prevents it from being disengaged from its combinations by the volatile acids.

Hydriodic acid always becomes more or less coloured when distilled. It even becomes coloured at the ordinary temperature, provided it be in contact of the air. Oxygen is absorbed, water formed, and the iodine, instead of precipitating, dissolves in the acid, and gives it a brownish red colour, the deepness of which is proportional to the quantity of iodine held in solution. I have attempted, but in vain, to destroy this colour by boiling the acid; hence I conclude, that iodine has a great affinity for hydriodic acid: for if its volatility were not diminished by the combination, it ought to be disengaged with the vapour of the water. However I do not think that we ought to consider this coloured acid as a peculiar acid.

Concentrated sulphuric acid, nitric acid, and chlorine, immediately decompose hydriodic acid. They seize upon its hydrogen, and the iodine precipitates, or exhales in purple vapours. Chlorine is one of the most delicate re-agents to detect small quantities of hydriodic acid; but it must be added cautiously: for when an excess is employed, it dissolves the iodine before it has time to precipitate, or at least colour the liquid. Like hydrosulphuric acid, hydriodic acid is decomposed by solutions of peroxide of iron. When heated by the oxides which give chlorine with hydrochloric acid, iodine is evolved, together with a hydriodate or an ioduret. If for example, it be heated with the black oxide of manganese, we obtain iodine and hydriodate of manganese; but with the red oxide of lead, we obtain iodine and an ioduret. Hydriodic acid forms compounds with the different bases, which have a great deal of resemblance to the hydrosulphates and the hydrochlorates.

Let us recapitulate the principal characters of hydriodic acid. In the gaseous state it is speedily decomposed by mercury, which is converted into an ioduret of a greenish yellow colour. With chlorine it immediately produces a fine purple vapour of great intensity. In the liquid state it is speedily decomposed, and coloured when exposed to the air. Concentrated sulphuric acid, nitric acid, and chlorine, separate iodine from it. Sulphureted hydrogen does not alter it in the least. When poured into a solution of lead it forms a fine orange precipitate. In the solution of peroxide of mercury it forms a red precipitate, and with silver a white precipitate insoluble in ammonia. I thought proper to give the properties of hydriodic acid in this place, because this will render more intelligible the account which I am going to give of the combinations of iodine with other bodies.

Iodine forms with sulphur a weak compound of a greyish black colour, radiated like sulphuret of antimony. Iodine is separated from it when it is distilled with water.

Hydrogen, whether dry or moist, did not seem to me to have any action on iodine at the ordinary temperature; but if, as was done by M. Clement, in an experiment in which I was present, we expose a mixture of hydrogen and iodine to a red heat in a tube, they unite together, and hydriodic acid is produced, which gives a reddish brown colour to water. We found that 100 parts of iodine absorb 1.53 of hydrogen, in order to be converted into an acid. But this proportion is a great deal too great, as I found afterwards that hydriodic acid is composed of 100 iodine and 0.849 hydrogen.

Charcoal has no action upon iodine, either at a high or low temperature. Several metals on the contrary, as zinc, iron, tin, mercury, and potassium attack it with facility, even at a low temperature, provided they be in a divided state. Though these combinations take place readily, they produce but little heat, and but rarely any light. The compound of iodine and zinc, which I call *ioduret of zinc*, is white. It melts readily, and is sublimed in the state of fine acicular four-sided prisms. It is very

soluble in water, and rapidly deliquesces in the air. It dissolves in water without the evolution of any gas. The solution is slightly acid, and does not crystallize. The alkalis precipitate from it the white oxide of zinc, concentrated sulphuric acid disengages hydriodic acid and iodine, because sulphurous acid is produced. We may conceive that water dissolves the ioduret of zinc without undergoing decomposition; but as the slightest force would afterwards decompose it, and besides, as the solution has exactly the same characters as the hydriodate of zinc obtained by combining the oxide of zinc with hydriodic acid, we have the same motives for admitting that the water is decomposed during the solution of the ioduret, as for admitting that it is formed when hydriodic acid dissolves the oxide. We may, however, adopt either supposition. For the sake of greater simplicity I shall adopt the latter, in order to determine the ratio of iodine to oxygen and hydrogen.

When iodine and zinc are made to act on each other under water in vessels hermetically sealed, on the application of a slight heat the water assumes a deep reddish brown colour, because as soon as hydriodic acid is produced it dissolves iodine in abundance. But by degrees, the zinc which I suppose in excess, combines with the whole iodine, and the solution becomes colourless like water. In three experiments, which differed little from each other, and of which I have taken the mean, I found that 100 iodine combined with 26.225 zinc. But 26.225 zinc combine with 6.402 oxygen, which saturate 0.849 hydrogen; consequently, the ratio of oxygen to iodine is 6.402 to 100, or 10 to 156.21, and the ratio of hydrogen to iodine is 0.849 to 100, or 1.3268 to 156.21. Thus if we represent the number for oxygen with Dr. Wollaston by 10, the number for iodine will be 156.21; the ratio which I assigned in my first experiments, as well as that of Davy, was very inaccurate.

Iron is acted upon by iodine in the same way as zinc. The ioduret of iron is brown, and fusible at a red heat. It dissolves in water, and the colour of the solution is a light green, like that of the chloruret of iron.

Iodine and potassium combine with a great deal of heat, and with the disengagement of a light which appears violet through the vapour of iodine. The compound melts, and sublimes at a red heat. On cooling it assumes a pearly and crystalline appearance. Its solution in water is perfectly neutral. It is easy to determine the proportion of these iodurets from those of ioduret of zinc. If we attend to this circumstance, that the quantities of iodine which combine with each metal, are proportional to the quantities of oxygen with which it combines; of course 100 potassium, which requires 20.425 oxygen to convert it into potash, combine with 319.06 of iodine.

The ioduret of tin is very fusible. When in powder, its colour is a dirty orange yellow, not unlike that of glass of antimony. When put into a considerable quantity of water it is completely

decomposed. Hydriodic acid is formed, which remains in solution in water, and the oxide of tin precipitates in white flocks. If the quantity of water be small, the acid being more concentrated, retains a portion of oxide of tin, and forms a silky orange-coloured salt, which may be almost entirely decomposed by water. Iodine and tin act very well on each other in water of the temperature of  $212^{\circ}$ . We may, by employing an excess of tin, obtain pure hydriodic acid, or at least an acid containing only traces of the metal. The tin must be in considerable quantity, because the oxide which precipitates on its surface diminishes much its action on iodine.

Antimony presents with iodine the same phenomena as tin ; so that we might employ either for the preparation of hydriodic acid, if we were not acquainted with preferable methods.

The iodurets of lead, copper, bismuth, silver, and mercury, are insoluble in water, while the iodurets of the very oxidable metals are soluble in that liquid. If we mix a hydriodate with the metallic solutions, all the metals which do not decompose water will give precipitates, while those which decompose that liquid will give none. This at least is the case with the metals of which I have spoken : and if this fact, which I consider as general, be not a sufficient proof of the existence of hydriodates, it at least renders their existence probable.

There are two iodurets of mercury : the one yellow, the other red ; both are fusible and volatile. The yellow, which corresponds to the protoxide of mercury, contains one-half less iodine than the red, which corresponds to the protoxide. In general there ought to be for each metal as many iodurets as there are degrees of oxidation.

All the iodurets are decomposed by concentrated sulphuric and nitric acids. The metal is converted into an oxide, and iodine is disengaged. They are likewise decomposed by oxygen at a red heat, if we except the iodurets of potassium, sodium, lead, and bismuth. Chlorine likewise separates iodine from all the iodurets ; but iodine, on the other hand, decomposes most of the sulphurets and phosphurets.

(To be continued.)

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#### ARTICLE IV.

*Some Observations on the Sap of the Vine.* By Dr. Prout.

ABOUT the middle of April last I was favoured by Mr. Astley Cooper with some sap which he had collected from a common white vine.\* The following are a few of its properties :—

It was slightly opaque, or rather had the whitish appearance of common river water. Taste sweetish, but not rough. No smell.

\* Mr. C. informs me that the vine, although it bled very profusely, seemed to produce a greater number of leaves than usual, but no grapes,

It did not affect litmus or turmeric papers in their natural states, nor the former when it was faintly reddened by acetic acid. Specific gravity not sensibly different from that of water.

1. *Potash*.—A solution of pure potash changed it to a beautiful reddish copper colour, and caused after some little time a flaky precipitate of the same colour, leaving the fluid nearly colourless and transparent. This precipitate was not redissolved by excess of potash even when heat was applied, but the application of heat changed it to a deepish brown colour. Acetic acid added in slight excess readily redissolved this precipitate.

2. *Ammonia*.—The same phenomena followed the use of this alkali as of potash above-mentioned. Acetic acid also as readily redissolved the precipitate occasioned by it as by potash.

3. *Muriatic Acid* produced no apparent change. After the addition of this acid, ammonia was added in excess, when precisely the same sort of precipitate as that above-mentioned was observed.

4. *Oxymuriatic Acid* produced no apparent change.

5. *Muriate of Barytes*.—No apparent change.

6. *Oxalate of Ammonia* produced a very sensible white precipitate.

7. *Prussiate of Potash* caused a very slight whitish precipitate.

8. *Hydro-sulphuret of Potash*.—A slight dark brown flaky precipitate.

9. *Nitrate of Silver*.—A slight flaky precipitate, which soon became of a purple hue.

10. *Oxymuriate of Mercury*.—No apparent change.

11. *Subacetate of Lead*.—A copious yellowish-white precipitate.

12. *Infusion of Galls*.—No apparent change.

13. *Gelatine*.—No apparent change.

Four hundred and sixty grains of the sap were evaporated in a glass capsule on a sand-bath. During the evaporation air bubbles collected on the sides and bottom of the vessel. The fluid became slightly opake, and towards the end of the operation brown flocculi precipitated. There was left only  $\frac{1}{2}$  gr. of solid matter (= .044 per cent.), about half of which was carbonate of lime, the rest a peculiar vegetable matter. This peculiar vegetable matter was not soluble in alcohol, and therefore did not agree in this respect with the ill-defined class of substances called extracts or extractives. Both it, however, as well as the lime, were evidently held in solution by some volatile acid or acids. One of these acids was doubtless the carbonic. There were also traces of the acetic acid, and likewise of an alkali (potash?), since the glass capsule on exposure to the air became sensibly moist. The quantities, however, of the last two were extremely minute.

Every thing connected with vegetable physiology is exceedingly obscure. The opinion,\* however, appears to be correct, which

\* Darwin's *Phytologia*, p. 28, &c. Mr. Knight, in *Philosophical Transactions*, 1805, p. 70, &c.

supposes that one use of the large quantity of watery sap which flows in plants in the spring is to dissolve the thick and otherwise inactive juices which had been deposited in nearly a solid state in the vessels during the winter, and thus adapt them to the further uses required by the economy of the plant. It appears also that this solution is not effected solely by the agency of the water, which alone would perhaps be insufficient, but by the assistance of some acid or saline agent which probably pre-existed in the plant itself, and only required the presence of water to render it effective. And certainly we cannot conceive menstrua better adapted for this purpose than the above acids, not only from their considerable solvent powers, but from the ease with which they may be got rid of, either by decomposition, exhalation, &c. when they have performed their office.

I know of no use of the large proportion of lime found in all saps; but I have some reason for believing that there is a much greater relation between this earth and the saccharine principle than has been commonly imagined.

The saps of different vegetables have been examined by Vauquelin, Chaptal, and others. There is some resemblance between the sap of the common elm as observed by Vauquelin, and the above. See *Ann. de Chim.* xxxi. 20; *Memoires de l'Institut National*, i. 288; also *Dr. Thomson's Chemistry*, vol. v.

## ARTICLE V.

*On the Use of the Cerebellum, on the Spinal Marrow, and on Respiration.* By Dr. John Cross.

(To Dr. Thomson)

SIR,

Glasgow, Nov. 18, 1814.

PLEASE to announce in your *Annals of Philosophy* the following discovery of the function of the cerebellum, and new theory of respiration.

From considering that the cerebrum, cerebellum, and face have in the gradual progress of animality originated and evolved together, and that organs in general have their sphere of action in their own vicinity, I was led about a year ago to conclude that as the cerebrum is the fountain of sensation and intellect, the cerebellum must be the organ which supplies with nervous energy the face and other parts of the head extrinsical of the brain, perhaps also the cerebrum itself. This opinion pressed more and more strongly on my mind until I was at length tempted to use the trephine upon living animals. Having cut out a circular piece of the occipital bone of a sheep, and laid bare the cerebellum, I applied pressure upon it with the handle of a scalpel; immediately the ears, eyes, mouth, tongue, in short the whole muscles about the head and face, became convulsed. On thrusting the handle of the scalpel

through the cerebellum upon the medulla oblongata, the muscles of the body were at the same time convulsed. On pressing upon the spinal marrow as it issues from the foramen magnum, the muscles of the body were convulsed, those of the head and face remaining quiescent. The experiment has been frequently, and several times in presence of my medical friends, repeated upon sheep, rabbits, and dogs, with invariable success. This discovery led to several minute dissections of the brain and spinal marrow in dogs, cats, cows, horses, &c.; in which I found that the spinal marrow is uniformly composed of four longitudinal divisions—two larger, anterior, which may be traced into the cerebrum, and two smaller, posterior, which may be traced into the cerebellum. I suppose that the two anterior portions are the organ of feeling; the two posterior, of motion. I also found that the spinal marrow does not terminate at the sacrum in a lash of nerves, but tapers to a sharp point, to the very extremity of which I could distinctly trace the division into four portions.

On tracing the various organs of the body from man downwards through the chain of living beings, I find that there are three great fundamental functions—respiration, nutrition, and propagation, common to all organized beings. In all vegetables and animals there are neither more nor less, in so far as the living machine is concerned, than these three functions, every other being subsidiary. From this view I was induced to think more highly of respiration than as an absorber of oxygen, or an expeller of carbon. That such a large complicated organ, and such a laborious and incessant function, should serve no other purpose but drawing off carbon from the blood, is neither consistent with analogy, nor with that Divine wisdom and power which uniformly effectuates the greatest ends by the smallest means. It appears to me that vitality has been originally diffused equally over the creation; that vegetable and animal life is just a less or greater accumulation of this vitality; and that the leaves, gills, and lungs, are the chemical laboratory in which vitality is continually secreted as fast as it is expended by the vital and animal functions.

As the facts and arguments in support of this theory, and of the preceding discovery, are forthwith to come before the public, in a work to be entitled, *Researches in Physiology and Physiognomy*, I will not at present encroach further on your patience.

I remain, Sir, your most obedient servant,

JOHN CROSS.

## ARTICLE VI.

*Observations on Tanning.* By Mr. Murray.

(To Dr. Thomson.)

SIR,

NOTICING in the *Annals of Philosophy*, for the 1st of September, *Queries respecting Tanning*, I presume the liberty of submitting to

you the results of experiments I have instituted on this branch of our manufactures.

Respecting the question whether *lime-water* or pure *river water* extracts the tannin from oak bark most promptly and in greater quantity, experiment enables me to decide on the *latter*.

That *lime* and *alkalies* ought to be discarded in the process of *tanning* (could we find a substitute for the former) will appear evident from immersing a piece of leather in either; the fluid will appear in a few minutes of a *blood-red colour*, and the surface of the leather *blackened*; a proof that the tannin has been absorbed by the *alkaline menstruum*, or *lime-water*.

Hence the justice of the remark, by all possible means *get rid of the lime* employed in depilation, *previous to immersion* in the *tannin coze*. *Lime* will always produce with tannin an abundant and difficultly soluble precipitate: and for the same reason *alkalies* must not be used in *raising the pelt*, as they, though producing a quick and powerful extract, prevent the union of the tannin with animal gelatine by virtue of superior affinity.

The excrement of the pigeon and hen were tried with respect to their superiority over that of other animals. By sulphuric acid, a powerful effervescence takes place; and as lime had been proved to exist by the previous application of fluat and oxalate of ammonia, the *carbonic acid* was inferred to be in combination, and not the phosphoric, as the precipitate by barytes did effervesce by muriatic acid.

*Nitro-muriate of tin* and *gelatine* threw down an abundant precipitate. Hence I presume that it is to this *peculiar* substance, and not to any of the salts which it contains, we are to attribute its action on the pelt. The gelatine must be rendered less pliant by the lime used in depilation. The substance in question having an affinity for lime, would necessarily render the fibre of the hide less rigid than before, by removing the cause.

I have long suspected that *handling* did more for the beauty and value of the leather than was suspected. I endeavoured to ascertain to what this effect was owing. For this purpose a piece of leather was suspended in a vessel filled in water over the shelf of a pneumatic cistern, while the water was displaced by a stream of oxygen. The leather was in consequence beautifully *bloomed*, and this effect penetrated quite through the piece.

*Diluted nitric acid* stains the leather of a *blood-red*.

*Sulphuric acid* acts in an *inferior* degree, and makes the leather hard.

Muriatic, oxymuriatic, citric, phosphoric, acetic, and fluoric acids, communicate each a bloom. Of these the *oxymuriatic* and *muriatic acids* are by far the *preferable*.

Oxymuriatic acid and wheaten flour communicate a very fine light bloom.

It was therefore of considerable moment to ascertain whether the effect was owing to the *fecula* or *gluten* of the flour employed.

Having separated these constituents, I found *fecula simply* to communicate a bloom. *Oxymuriatic acid* and *gluten* had a *very bad effect*. Hence a self-evident proposition becomes irresistible: that those nutritious grains containing less gluten are the most proper; thus the farenaceous seeds of oats, barley, rye, &c.

The oxymuriate of lime appears to communicate a fine bloom: and as this acid may be employed to extract the lime after the process of depilation, so the salt of oxymuriate of lime may be employed afterwards to improve the bloom, so requisite to those tanners who prepare for the London market.

Two very respectable tanners have informed me that on my suggestion muriatic acid and oatmeal, and barley flour, have been used by them with great success. The muriatic acid renders hides which are required to possess this property pleasantly flexible.

“ Les choses ne sont estimables qu'autant qu'elles sont utiles.”

I am, with much respect,

Your very humble servant,

Stowmarket, Oct. 3, 1814.

J. MURRAY.

P.S. One of the desiderata in tanning appears to be some good substitute for lime in the process of depilation. Would not this be effected by a stove heat?

## ARTICLE VII.

*On Galvanism as a Solvent of Urinary Calculi.*

By Mr. William Stark.

(To Dr. Thomson.)

SIR,

A METHOD of decomposing urinary calculi by the galvanic influence having been suggested by a Mr. Goring in the last number of your *Annals of Philosophy*, p. 361, I am induced to trouble you with some of the remarks which I made in a paper read at the Norwich Philosophical Society, Nov. 26, 1812, on that subject. I have no wish to take from Mr. Goring any merit that belongs to him in proposing a substitute for one of the most dangerous operations in surgery; but I must confess I do not see that Mr. Goring's suggestion promises much advantage, either with respect to the greater safety, or the alleviation of the sufferings, of patients. I would ask Mr. G. whether puncturing the bladder with a trochar, and introducing the appendages of a galvanic battery *through* the wound, is not attended with as much pain, and as *much danger*, as attend the common method of operating for the

stone? Mr. G. considers, very properly, his mode of operating to be a work of time; and that consideration surely is sufficient to reject his plan; for how is it possible to keep a wound open a sufficient time to enable the action of the galvanic influence to decompose a calculus, without the greatest danger to the patient?"

The plan I suggested to the Norwich Society was this:—"Let an instrument similar in shape to a common catheter, made with gum elastic, waxed silk, or any non-conducting substance, with two cavities, one for the wire from the positive end of the battery, the other for the wire from the negative end. This instrument with the wires to be introduced along the urethra into the bladder till it touches the stone, the wires having elasticity, and an inclination to bend outward, would facilitate the operation, as they would then be easier fixed, one on each side the calculus. When so fixed, which a surgeon accustomed to similar operations could easily determine, the patient might be left at rest, and the action of the battery suffered to proceed. It is not to be supposed that in all cases of calculous affection this plan is advisable, but in those where the bladder contains but one concretion: and from the best information I can procure, it seems that, on the average, six-sevenths of those afflicted have but one concretion, I have no doubt of its success. The patient immediately after, or towards the close, of the action of the battery, should be suffered to drink plentifully of diluents, that the disunited matter may be carried off by copious evacuations of urine."

When the above suggestions were made, I intended to have tried experiments to ascertain the time and intensity that certain calculi required for their decomposition, by first analyzing the different strata of them, and submitting them to the action of batteries of different powers; but the difficulty of procuring calculi, and the want of leisure, have at present frustrated my intention.

The objection which Mr. Goring makes to the introduction of any instrument up the urethra surely is not well founded, and certainly not liable to the risk, pain, and inconvenience, of the method he proposes. I dare say every person is acquainted with some one in the circle of his friends who is obliged to perform such an operation himself daily, at least it falls to my lot to know many who are obliged to do it, and without the least danger. However, so satisfied am I that the power of the galvanic influence is sufficient to accomplish the desired object, that were I so unfortunate as to be afflicted with the malady, I would cheerfully submit to a trial of it. Experiment must prove whether the plan which I suggested in 1812, Mr. Goring's, or any other, is best adapted for its accomplishment.

"*Palmar qui meruit ferat.*"

Yours,

WILLIAM STARK.

Norwich, Nov. 12, 1814.

## ARTICLE VIII.

*Answer to Mr. R. Phillips's Animadversions on Mr. Hume.*

(To Dr. Thomson.)

SIR,

YOUR Correspondent, Mr. R. Phillips, affords another example of human imperfection, that we are all "prone to complain" and often to publish too, without any reason or foundation.

Much of his valuable time, and more than nine-tenths of his letter to you, might have been spared, had he only looked into one of the volumes of your own System of Chemistry. There he would have found that, instead of dashing into a stream of words, studied expressions and polished sentences, you have in a single line said all that was necessary to correct where there was assuredly on my side no intentional plagiarism; for, speaking of the super-sulphate of barytes, you say in a note, "Mr. Hume has also mentioned it, but the fact was well known to chemists." To your decision I have long ago assented, and have also abandoned all claim to priority respecting the super-sulphate of strontian, after I found that Mr. Clayfield, of Bristol, had anticipated my observations.

Mr. R. Phillips is equally unfortunate in the two experiments quoted from M. Sage; he has drawn inferences diametrically opposite to those of all the chemists who have written upon the subject, especially those of France. In these experiments there is neither *water of solution* nor *water of crystallization*; there is no guide to direct future operators to avoid such errors as I shall presently quote; nor is there any room for asserting, that putting a quantity of *carbonate* of barytes into either of the two acids, although it be granted that in such a case it must be converted into a nitrate or muriate, is precisely the same as adding these salts or more particularly their solutions to the respective acids. There is nothing here to invalidate what I have said on the subject; nothing detailed by M. Sage to show the direct meaning and tendency of the instructions which I took the liberty to offer, and which may be thus epitomized:—*That such is the avidity of nitrous and muriatic acid for water, that they will attract even the whole of the water of solution from their respective barytic salts.*

When I published my paper upon this subject, it had been a common practice to ascertain the purity of nitrous acid, and even to purify it, by dropping into it a solution of nitrate of barytes; and this method was pursued by some very eminent professional men. I recollect one case in which a Gentleman, well known as a chemist and mineralogist, condemned some nitrous acid which had been sent to Sir John St. Aubyn, which, on my proving the error, was

not returned. This happened above 14 years ago, and consequently was subsequent to M. Sage's experiments. Indeed, from the letter to you, I have no doubt but Mr. R. Phillips himself has often committed the same blunder.

That M. Bouillon-Lagrange did not benefit by M. Sage's example as he might have done by mine, will be evident to Mr. R. Phillips by the following sentence from Manuel d'un Cours de Chimie: "Ce sel (nitrate de baryte) est utile pour reconnaître la présence de l'acide sulfurique. On peut s'en servir pour séparer cet acide, qui se trouve quelquefois dans l'acide nitrique, et qui empêche de l'employer dans des expériences exactes."

I shall select another case, out of a great many I could name: it is that of Dr. Swediaur, who, as well as M. Bouillon-Lagrange, must have been even personally acquainted with M. Sage, and could not be ignorant of the story of carbonate of barytes in the acids, which may now be called Mr. R. Phillips's fable. In order to divest nitrous acid of sulphuric acid, the Doctor prescribes the following plan. Let a solution of nitrate of barytes be added until there be no more precipitation, "*donec nihil amplius præcipitetur.*" I trust this experiment will be closely examined by Mr. R. Phillips, and that he will favour your readers with the result, the *nihil amplius*; and, for my own part, it is of no moment in this experiment whether the acid contain sulphuric acid or not, for I will venture to predict that the experiment will prove equally amusing to Mr. R. Phillips or to any other operator.

Discoveries and improvements in science invariably precede the dates of their being presented to the public, depending on the disposition, convenience, and pursuits, of their authors, and various other causes. In my own case these intervals have generally been of many years, and that particularly of employing silver as a test for arsenic, and asserting its superior efficacy, was not published until above 20 years after I had discovered it. It occurred to me while examining certain materials used in the preparation of carmine, and its utility was more distinctly evinced in subsequent trials, especially while analyzing a metallic ore belonging to the late Judge Buller. I might indeed appeal to many living witnesses that I am not so forward to tease the public with my writings as Mr. R. Phillips would insinuate. The Journals of the Royal Institution will bear testimony that I can prefer a *private* communication. I am unwilling to bring names forward on this occasion without permission of the parties; but I can recollect one case in point, and not unlike one of the experiments of M. Sage, which took place more than ten years before my remarks on barytes were published. Few philosophers have contributed more effectually to the Philosophical Transactions than the Gentleman to whom I now allude, and he probably has not forgotten the short conversation that passed in his library, at that time in the same street and very near to my residence, on our inspecting a vessel containing carbonate of barytes in powder mixed with nitrous acid, and in a state of apparent quiescence. I am persuaded that this Gentleman did

not then consider me at all ignorant of any thing respecting M. Sage's experiment, or that I did not know in 1792 what I published in 1802 respecting barytes.

The two physicians to whom Mr. R. Phillips alludes cannot be more respected by him than by me; their reputations stand very high, and most deservedly so; they need no panegyric from either of us, and can fight their own battle; therefore any allusion to them on the present occasion is both irrelevant and intrusive. Such an interposition is more befitting an hireling than one in pursuit of the truth, and there are various ways of engaging such characters; for even flattery, ambition, malevolence or jealousy, is often as effectual a stimulus as any thing of a pecuniary nature. Who Mr. R. Phillips is, whether the initial be meant for *Robert, Richard, Ralph,* or *Roger*, I have yet to learn. I am totally unconscious of having offended any Gentleman of the name of Phillips, much less any one of whom I can boast a personal knowledge.\*

I shall now expect to be told that I have also been forestalled respecting my test for arsenic; that the arseniate of silver, the *brick-red* coloured compound, had been prepared by others; that it had been formed before I was born, having been found in the laboratory of that busy old being, Dame Nature; that Henckel, Bergman, and others can bear testimony to the fact; and that M. Klaproth had frequently got hold of it, analyzed it, regenerated the same compound by means of nitrate of silver; but was so cruel and unlucky as to disregard the silver as a *test*, always preferring the *acetate of lead*, even to the end of the second volume of his valuable Analytical Researches; thus depriving Mr. R. Phillips of another theme for his peevish effusions.

Upon the whole, Sir, and from the last paragraph of the letter, I think your Correspondent writes more from principles of enmity and revenge than from a desire to improve science; and, as he seems to hold out a threat, I must be prepared to repel such attacks as I may now expect from one who is capable of treating me with so much malice and so little candour.

I remain, Sir, your obedient servant,

Long Acre, Dec. 12, 1814.

JOS. HUME.

## ARTICLE IX.

*Notices respecting the Old Silver Mine in Linlithgowshire.*

By John Fleming, D.D. F.R.S.E.

IN the centre of the county of Linlithgow there is a small mountain group, the most elevated portion of which is known by the

\* Mr. Richard Phillips of the Poultry has been an acquaintance of mine these dozen years, and I have always considered him as one of the acutest chemists in London. He is well known to the public by various important papers in the *Philosophical Magazine*, and by his strictures on Dr. Powell's translation of the *last edition of the London Pharmacopœia*, one of the acutest pieces of criticism the English language.—T.

name of the Hilderstone Hills. Cairn-paple, or Cairn-naple, the highest of these hills, rises 980 feet above the level of the sea, and commands an extensive prospect of the surrounding country. The summit is flat, and is composed of green-stone, in many places passing into basalt and wacke.

The base on each side consists of rocks having the same dip and direction, and belonging to the Independent Coal Formation. At the western base there are many valuable beds of black coal and sand-stone, and at the eastern base there are extensive strata of lime-stone. These all stretch to the north-east, and have a westerly dip. The strata of lime-stone form a bed upwards of 30 feet in thickness, and are covered with beds of sand-stone, slate-clay, and clay-iron-stone. In these the vein is situated which is stated to have produced at one time a considerable quantity of lead and silver.

The lime-stone is of a blackish-grey colour, of various degrees of intensity. Its lustre is in general glimmering, often glistening, and even shining, but seldom dull. The compact fracture which it exhibits is in general fine splintery, often conchoidal, and sometimes earthy. It rarely occurs with a small granularly foliated fracture.\* When the stone contains many petrifications of entrochites, the foliated fracture is often conspicuous. It is opaque, or very faintly translucent at the edges.

The lime-stone contains many irregular masses of flint, and the same mineral not unfrequently occurs in thin beds, thus occupying the same place in compact lime-stone which quartz is often observed to hold in granular lime-stone. The petrified remains of marine animals frequently present themselves in this rock. The teeth of fishes, particularly the *shark*, the spines and portions of the crust of *echini*, and fragments of the *trilobite* of Mr. Parkinson, are but rarely found. The remains of corals and shells are more abundant. The corals belong chiefly to the genera *fungia*, *millepora*, *eschara*, *orbitolites*, and *tubipora*. The shells are principally the remains of *acephalous mollusca*, some of which may be referred to the following established genera: *pinna*, *modiola*, *corbula*, *terebratula*, *gryphæa*, and *productus*. There are likewise a few shells belonging to genera in the cephalous order of mollusca. Thus there are species of the genera *turbo*, *melania*, *nautilus*, *ammonites*, and *orthocera*. In the month of May last I transmitted to the Wernerian Society a description of ten species of *orthoceratites* from the strata of this district, and chiefly from the bed of lime-stone above-mentioned. This establishes the fact of their occurrence in the Independent Coal Formation, and thus proves that they are not peculiar to the lime-stones of the transition period. This point had been ascertained in Scotland upwards of 20 years ago by the late

\* One specimen of granularly foliated lime-stone from this bed is of a greyish black colour, and is much impregnated with bituminous matter, a small portion of which pervades the whole bed.

Rev. David Ure, Minister of Uphall, in Linlithgowshire. This Gentleman, in his History of Rutherglen and East Kilbride, published at Glasgow in 1793, describes and figures two species of this genus which he found in strata belonging to the great coal-field of Lanarkshire.

Having stated these facts in illustration of the natural history of the strata which the metalliferous vein is said to have traversed, I shall now mention a few circumstances concerning the vein itself with which I have become acquainted. Sir Robert Sibbald, in his *Scotia Illustrata*, published in 1684, part first, page 31, gives the date of its discovery, and records the name of the discoverer: "In Lothiana Occidentali, ad tria milliaria a Limnucho Austrum versus, in monte qui Cairne-papel dicitur, tempore Jacobi Sexti primi Britanniae Monarchae, ab Alexandro Mund Carbonario inventa est Argentifodina, ubi purius argentum, idque majore proportionem, ex lapide rubro extractum fuit." The same author, in his History of the Sherifffdom of Linlithgow, 1710, p. 27, adds a little to his former description: "In *Hilderstone Hills* is the *silver mine*, which afforded much silver at the first working of it: a part of the melting-house is yet to be seen: and amongst the *adites* to the mine, the richest was that called God's Blessing. The spars are of different colours; some are white, and others of a red colour." Tradition says that this mine was abandoned in consequence of the roof of the workings falling down, and a great increase of water taking place. In hopes of overcoming these obstacles, the proprietor, the Earl of Hopetoun, some years ago made an attempt to re-open the mine. He brought some workmen from his mines at Lead Hills, and employed them in boring and sinking shafts in the neighbourhood of the old workings. But the information thus obtained was considered of little importance, and a stop was put to all further investigation.

The ruins of the old smelting-houses are still visible, and considerable heaps of rubbish surround the openings of the old shafts; but as no access to the mines can now be obtained, no precise information can be procured concerning the quantity or value of the ore, or the constitution and extent of the vein. In a lime-stone quarry about 200 yards to the east of the old workings the outgoing of a vein is distinctly seen, which traverses the strata in the direction of their dip, and is filled with soft clayey marl, and contains masses of impure lime-stone, together with lead glance and heavy spar. It runs towards the place where the former workings were carried on, but does not appear to have been a portion of the principal vein, as the old shafts have not been sunk in the direction of its line of bearing. Judging from such circumstances, the principal vein must have traversed the strata in the line of their stretch. To the south of the old workings there are indistinct appearances of a vein of green-stone running in a northerly direction; but the nature of this vein cannot be ascertained by inspection of the surface, which is

deeply covered with soil. The heaps of rubbish in the neighbourhood of the mine are now our surest guides, and in them the following minerals may be observed :—

1. *Heavy Spar*.—This appears to have occurred in great quantity in the vein, and may even be observed filling up some small rents in the adjacent rocks. It is usually of a reddish colour, sometimes white, and presents the following sub-species. *Heavy spar earth*, found as a coating to the cavities of the other sub-species. *Granular heavy spar* is very abundant, and in some specimens appears to pass into compact heavy spar. *Curved lamellar heavy spar* is in small quantity at the old workings, but occurs abundantly in the cross vein mentioned as opening into the lime-stone quarry. *Straight lamellar heavy spar* appears in greatest plenty, usually compact, sometimes crystallized in the form of a rectangular four-sided table, having all the terminal planes levelled. In these sub-species of heavy spar the ores of the following metals occur either imbedded or disseminated.

2. *Lead*.—Lead glance was the ore sought after, and yielded so considerable a proportion of *silver* as to bear the expense of extraction. All the pieces of this ore which I have seen are broad foliated.

3. *Nickel*.—Both the ores of this metal are to be found here, but in small quantity. The *copper nickel* is in roundish pieces, from the size of a pea to that of a pigeon's egg. The *nickel ochre* sometimes occurs as a coating to the preceding species, and likewise fills small cells in the heavy spar, where it appears to hold the rank of an original deposition.

4. *Cobalt*.—I have not observed any of the alloys or oxides of this metal; but the arseniate of cobalt, or *cobalt crust*, fills the cavities of the heavy spar, and is spread as a coating on its surface.

5. *Zinc*.—The only ore of this metal which is here observable is a small portion of *brown blende*. The same ore associated with lead glance. Iron pyrites, brown spar, and lime-spar, may be observed in many of the small veins which traverse the strata of lime-stone in this district.

Amidst the rubbish may also be observed masses of calcareous sand-stone and indurated clay. After heavy rains, when fresh portions of the rubbish have been exposed, the poultry which feed near the place are observed to sicken and die. Are we to consider the cobalt crust as the cause of the mischief?

The rarest ores enumerated above are those of nickel and cobalt. These have been found in other places of Scotland, but not situated in the same kind of rocks. Some time ago I observed among the manuscripts of the late Dr. Walker, Professor of Natural History in the University of Edinburgh, a short notice of his having between the years 1761 and 1764 found copper nickel and nickel ochre in the mines at Lead Hills and Wanlockhead. These mines are situated in transition rocks. Hence we must consider nickel as belonging not only to the primitive class of rocks, but to the transition, and even to a new member of the floetz class.

At the old silver mine at Alva, in Clackmananshire, cobalt crust occurs in the cavities of heavy spar associated with native silver. The vein is situated in trap rocks, which are included in the *old red sand-stone*.\* I have heard that the ores of cobalt occur in other parts of Scotland; but at present I am ignorant of the nature of the rocks in which they have been found.

I regret that these notices are so imperfect and unsatisfactory in those particulars which have a reference to the nature of the vein. But as they establish the existence of the ores of nickel and cobalt as inmates of the Independent Coal Formation, I have ventured to communicate them to your readers.

*Manse of Flisk, Fifeshire,  
Nov. 12, 1814.*

## ARTICLE X.

*An Address to those Chemists who wish to examine the Laws of Chemical Proportions, and the Theory of Chemistry in general.*  
By Jacob Berzelius, M. D. F.R.S. Professor of Chemistry in Stockholm.

MR. DALTON has published in the *Annals of Philosophy*, vol. iii. p. 174, Observations concerning my memoir On the Cause of Chemical Proportions. It has given me pain to think that the respectable Dalton has taken my ideas on the corpuscular theory as a criticism on his, between which he has pointed out the difference. I think I have expressed myself in that memoir with sufficient precision to make the reader sensible that I neither meant to give the opinions of Dalton, nor a correction of them. There is a very essential difference between the researches of Mr. Dalton and myself. Mr. Dalton has chosen the method of an inventor, by setting out from a first principle, from which he endeavours to deduce the experimental results. For my own part, I have been obliged to take the road of an ordinary man, collecting together a number of experiments, from which I have endeavoured to draw conclusions more and more general. I have endeavoured to mount from experiment towards the first principle; while Mr. Dalton descends from that principle to experiment. It is certainly a great homage to the speculations of Dalton if we meet each other on the road.

Among the numerous experiments which I have myself made relative to this subject, there are some which do not appear to agree

\* I have reason to conclude, from observations which I made this summer, that the country between the Lomonds on the south, and Stonehaven on the north, including the Ochils, and the hills in the neighbourhood of Perth, Dunder. Red-head, and Montrose, is composed of rocks belonging to the old red sand-stone. Upon the southern extremity of these rocks the great coal-field of the Forth rests, and occupies the situation of a newer deposition.

with the atomic theory so well as the rest; and which of course I have not been able to explain in a satisfactory manner. There are others whose existence is not a necessary consequence of the atomic theory of Mr. Dalton. These, in my opinion, prove that there is still something wanting in that theory, and which must be added to it in order to render it more complete. In my memoir *On the Cause of Chemical Proportions* I have endeavoured to draw the attention of the reader to these difficulties. Mr. Dalton has endeavoured to remove them, but with a levity which I did not expect from him, and which appears to me injurious to the science. He begins with pointing out the reasons why he cannot be of my opinion respecting the relative size and form of the atoms, &c. I must observe that at the bottom of every speculation in the exact sciences there always remains something which cannot be verified by experiment, and on which, of course, the imagination is at full liberty to indulge. The reveries of one man may be more ingenious, more interesting, and more probable, than those of another; but the science never gains any thing by disputes about subjects which are not susceptible of proof. I shall therefore pass over that part of the question in silence.

Mr. Dalton states that the electro-chemical polarity of the atoms makes no necessary part of the atomic theory, such as he maintains; nor did I ever mean to convey any such idea to the reader. For my own part, in considering a corpuscular theory of chemistry, I conceived that it should constitute the fundamental theory of the science; and instead of being occupied with a part of the phenomena, ought to embrace the whole. But when we treat of atoms in a chemical theory, we ought to endeavour to find out the cause of the affinity of these atoms. We ought to endeavour to combine researches respecting the cause why atoms combine with researches into the cause why they combine only in certain proportions. I do not consider the conjectures which I hazarded on the electro-chemical polarity of the atoms as of much importance. I scarcely consider them in any other light than as an ideal speculation deriving some little probability from what we know of the chemical effects of electricity. Yet the ideas on the relation of atoms to their electro-chemical properties, ought in my opinion to constitute an essential part of the corpuscular theory of chemistry, such as I view it; because I consider it as the duty of a man of science to endeavour to reach the first principle of the science, even though it should be actually impossible to attain it.

Mr. Dalton disapproves the idea which I announced, that we ought not to suppose an atom composed of  $2A + 2B$ ,  $2A + 3B$ , &c. He thinks that such combinations take place, though but seldom. He inclines even to the idea that olefiant gas may be composed of two atoms of carbon to two of hydrogen, placed in the form of a rhombus, those of hydrogen being at the extremities of the longest diameter. Is there a chemical fact which countenances such an idea of the construction of the atoms of olefiant

gas? If there be, the notion may be considered as an interesting conjecture; but if there be no such fact, the notion is a mere dream. There can be no doubt that if we give free liberty to our imagination in this manner, the science will degenerate into a mass of vain speculations, of no utility whatever, because founded on nothing but imagination. My idea, that in every inorganic combination one of the elements enters as unity, is founded on the circumstance that in all the inorganic bodies which I have analyzed, and I have analyzed a great number, I have found it to be so. Besides, if this were not the case, it is evident that all traces of chemical proportions would disappear in combinations which consist of various oxides, and could only be perceived in the most simple combinations, I have then founded my opinion on experience. Can Mr. Dalton produce an instance in which this opinion is obviously inaccurate?

Mr. Dalton then proceeds to remove the difficulties which I found attached to the corpuscular theory. I shall pass by the first, which he finds that I myself have obviated in a manner conformable to his opinion. The second difficulty consists in this—I have found compounds which are represented, for example, by  $A O^3 + 1\frac{1}{2} B O$ , which is contrary to the views of the atomic theory; while a combination of  $A O^3 + B O^3$  does not exist, although it be conformable to that theory. Mr. Dalton removes the first part of these difficulties, by saying, “the body B in such a case has in reality three atoms of oxygen for one of metal, and the union in question is  $2 A O^3 + B O^3$ .” I have given two examples of which Mr. Dalton has said nothing. These examples are the subarseniate of lead ( $As O^6 + 1\frac{1}{2} P O^3$ ), and the subsulphate of copper ( $S O^3 + 1\frac{1}{2} Cu O^3$ ). It is evident that neither of these examples admits the explanation by which Mr. Dalton has endeavoured to remove the difficulty; for neither the oxide of lead nor copper can contain three atoms of oxygen. Mr. Dalton adds merely, that he considers with Proust, minium as a compound of yellow oxide and brown oxide of lead; from which it seems to follow that he considers the subarseniate of lead as  $As O^6 + 3 P O$ . But what reason has Mr. Dalton to consider minium as composed in this manner? The necessity of it for his theory? But this necessity proves nothing as long as the theory itself is sub judice, which it probably will be for a long time to come. We may likewise explain the subsulphate of copper by supposing sulphuric acid composed of  $S + 6 O$ , or black oxide of copper  $Cu + O$ , and of course the protoxide  $2 Cu + O$ . But here lies the difficulty; for we have other reasons for considering the acid as  $S + 3 O$ , and the oxide  $Cu + 2 O$ . These difficulties cannot be removed by a stroke of the pen. Yet I am persuaded that both myself and Mr. Dalton will in time make use of these very difficulties to determine the true number of atoms in such and such compound bodies; but we must in the first place make a much greater number of experiments on these subjects than we possess at present; for it is not speculation alone, but experi-

ment guided and accompanied by speculation, that can give us new information. Let me be allowed here to give an example. Hydrate of iron is so composed that the oxygen of the oxide is twice that of the water; but both Mr. Dalton and myself give to this oxide three atoms of oxygen. This hydrate, then, is  $F O^3 + 1\frac{1}{2} H^2 O$ . I should certainly be disposed to explain this at once, by supposing that the oxide of iron contains six atoms of oxygen. But in order to see whether there be other proofs for such an idea, let us run over the combinations of the oxide of iron with other bodies. Let us examine, for example, the combinations of this oxide with acids which contain six atoms of oxygen, but which in their neutral saline combinations contain only three times as much oxygen as the base. If in these neutral combinations with these acids, the red oxide of iron does not constitute an exception to the general law, it ought likewise to contain six atoms of oxygen; for otherwise in the arseniate or chromate of iron there would be for each atom of iron half an atom of arsenic or chromium. Let us extend these researches still farther, and examine if the arseniates, chromates, &c. can be formed with oxides in which there is evidently no more than three atoms of oxygen, &c. The ultimate result of our researches will probably be that four and six atoms of oxygen, instead of three, are much more general than we have hitherto supposed; and that not only oxide of iron, but silica and various other oxides contain in fact six atoms of oxygen instead of three. I suppose that Mr. Dalton will agree with me that by such researches we may render much more complete the beautiful theory for which he feels himself so much interested, and for which we are in a great measure indebted to him.

I have considered it as a great difficulty attached to the atomic theory that it does not explain the laws concerning the combinations of the oxides with each other, namely, why the oxygen in the one is always a multiple by a whole number of that in the other. Mr. Dalton removes this difficulty by saying, "It is not the peculiar business of the atomic theory to explain it any more than to show why all the metallic oxides do not mutually combine with each other." But we must recollect that the principal circumstance in favour of the atomic theory is, that it gives a mechanical and very satisfactory cause why elementary atoms unite only in proportions which are multiples of each other. We have observed likewise that the compound atoms, that is to say, most of those which contain oxygen, combine likewise in a *multiplex ratio*, provided we attend only to the oxygen which they contain. It is clear that this must be owing to a cause similar to that which occasions similar proportions between elementary atoms; but the atomic theory throws no light on the subject. This must be admitted as something still wanting in the theory; for when a theory only explains one half of the phenomena which result from the principle which regulates these phenomena, it is surely incomplete. I differ, then, from Mr. Dalton, and must continue to consider the atomic theory

as imperfect, and as clogged with difficulties, till it give us satisfactory explanations of all the phenomena relative to the chemical proportions. I do not think that we are very far from this explanation, but this is not the place to dilate upon the subject.

Mr. Dalton continues to observe, that the difficulty presented by the new oxide of iron of Gay-Lussac has been employed by me in a manner "particularly unfortunate." Had not Mr. Dalton thought it of importance to the science to express his opinion of the first pages of my memoir, before he was acquainted with its general tenour and termination, he might have seen how I myself removed the difficulty in question, both in the memoir itself, and by my analysis of the magnetic iron ores, where I have shown that the numbers given by Gay-Lussac are inaccurate. (*Försäk til et System för Mineralogien*. Stockholm, 1814. p. 97.\*) As to the expression which Mr. Dalton has employed, "particularly unfortunate," I should have been glad to be deceived respecting it, by too little knowledge of the exact meaning of the phrase.

Mr. Dalton finishes his observations by endeavouring to remove the difficulties which the composition of organic bodies presents, in which the number of elementary atoms is often very great. I had instanced the atom of oxalic acid as an example of the composition of organic bodies. The method employed by Mr. Dalton to remove that difficulty deserves attention. "Were it," says he, "a matter of necessity, an anatomist might conceive one atom of hydrogen surrounded by nine of carbon, and the compound globule to have 18 of carbonic oxide adhering to it. But this would be an atom truly formidable in every sense of the word, as the least friction must be supposed capable of producing a violent explosion of such a mass of elasticity. I cannot, however, doubt that Dr. Berzelius having resumed the consideration, will very soon discover and acknowledge that his analysis is incorrect. In the mean time, I shall give my reasons for believing it to be so."

The tone of confidence with which Mr. Dalton finds me in the wrong in this passage has surprised me a little, and so much the more, that "any chemist is competent to satisfy himself on this head without appealing to authorities." As to the organic atoms, and the difficulties which they present, it would be too long to discuss them here. I therefore refer the reader to a set of experiments on the subject, which I am at present publishing in the *Annals of Philosophy*. As to my experiments on oxalic acid and oxalate of lead, I acknowledge that on repeating them I have found slight inaccuracies; but none of these have been in favour of Mr. Dalton's opinion. I have found that the oxalate of lead had given me rather too little oxide of lead, and oxalic acid rather too much hydrogen. Mr. Dalton has candidly stated his manner of analyzing the oxalate in question, and the result which he obtained. I shall likewise state mine. I

\* An English translation of this interesting work has been published, to which the reader is referred. T.

prepared oxalate of lead by pouring a solution of pure oxalic acid into a solution of pure and neutral nitrate of lead. I did not employ acetate of lead, because that salt combines in part with all the substances which precipitate from it, as I have shown in my experiments on carbonate of lead, and more particularly in my Essay on Organic Combinations. Oxalate of lead contains no water of combination; hence it is easily dried, and requires only to be heated a few degrees above the temperature of the atmosphere. I burnt it in a capsule of thin glass, of which I had exactly determined the weight. I heated it in the flame of a spirit lamp, taking care to apply the first heat near the edge of the glass, and advancing gradually to the centre. The oxalate takes fire and burns quietly. When the whole is burnt I allow it to cool. I now weigh the glass with the oxide which it contains. This oxide is mixed with a small quantity of metallic lead, reduced by the charcoal of the acid. I dissolve it in distilled vinegar, wash the residual lead, dry it, and weigh it. To the weight of the oxide found, I add the quantity of oxygen requisite to convert the metallic lead into oxide. By this method of experimenting, oxalate of lead gave me 75.46 per cent. of oxide of lead. I would not recommend to the reader to repeat this experiment in metallic vessels, as of platinum or iron. When platinum vessels are used, I find that nine times out of ten the reduced lead unites with the platinum and spoils it. As to iron, every body knows that it increases in weight in the fire. As to the water which I found in effloresced oxalic acid, and to the difference between the result of the analyses of oxalate of lime, and of my analysis of oxalate of lead, Mr. Dalton will give me leave to refer him to the experiments of those, who in their analysis of oxalate of lime, have not neglected the water of combination contained in that salt.

When I endeavoured to draw the attention of chemists to the difficulties in the atomic theory, it was not my intention to refute that hypothesis. I wanted to lay open all the difficulties of that hypothesis, that nothing might escape our attention calculated to throw light on the subject. I wished the experiments to verify the theory; and I should have considered it as absurd, if I had taken the opposite road. I placed beside the corpuscular theory, a theory of volumes; because that theory is in some measure connected with facts which may be verified. To those who think that the theory of volumes may be fatal to the corpuscular theory, I would observe, that both are absolutely the same thing; but that the theory of volumes has this immediate advantage over the other, that it may be more easily verified. Let us suppose for a moment, that the theory of volumes were absolutely demonstrated. We would then ask, what is the difference between a solid and a gaseous body? The answer would restore to the corpuscular theory its rights. It would be demonstrated by that of volumes. The only difference between the two theories consists in the words *atom* and *volume*, that is to say, in the state of aggregation of the element

Chemical proportions begin to be more generally studied; but chemists are not agreed about the laws which regulate them. One party waits the opinion of those whom they consider as authorities; and these last appear to suspend their judgment, because the opinion requires to be verified by experiments, the number and difficulty of which is revolting to their minds. Let me be permitted to point out here, what in my opinion ought to be the principal object of their examination.

Chemical proportions depend upon two cardinal points: 1. The proportions in which the elementary atoms unite. 2. The proportions in which the compound atoms combine. The first of these is a necessary consequence of the atomic theory; and as the number of combinations which it includes is very limited, almost the whole of them have been examined, without finding a single exception to the law. Hence this point is very generally admitted. The case is very different with the second point. It includes an almost infinite number of compounds, varying in their elements as to the number of compound atoms which they contain. I have examined a great number of these substances, and as they are for the most part composed of oxides, I paid particular attention to the oxygen which they contain. The laws which I considered myself as entitled to establish from these experiments are well known.

The first cardinal point being established we must endeavour to establish or refute the second. If the accuracy of my experiments be admitted, I think their number is such that the second point also may be considered as verified. If this accuracy be not admitted, it is obviously necessary to repeat my experiments, and produce other analogous ones. My experiments have informed me how very difficult and even painful this kind of labour is; but it is absolutely necessary. I know of only two chemists who have hitherto occupied themselves with this examination, namely, Mr. Vogel, of Bayreuth; and Dr. Thomson, of London. The first undertook the analysis of a set of double salts, mostly containing water of crystallization; that is to say, composed of three or four oxides. The results which he obtained corresponded with the law. Dr. Thomson has in a theoretic dissertation gone over a great number of saline combinations. He inclines to admit the law.

This law is of much more importance than may be supposed at first. It deserves therefore the most careful and impartial examination. Were it not for this law, no combination composed of several oxides could be calculated, nor any analysis verified in a decisive manner for the theory. For it is clear, that if there be combinations expressible, for example, by  $2A O^3 + 4B O^4 + 7C O + 5H O$ ; or if nature were to allow us to change the numbers in the formulas in any manner whatever, in such a case, all idea of determinate proportions would disappear, in proportion as these combinations became more complicated.

It is likewise by means of the law concerning the combination

of oxides with each other, that we can hope to penetrate into the secrets of organic composition. When that law is demonstrated, the theory of chemical proportions may be considered as established.

It is absolutely necessary, that every person who repeats an analysis in order to ascertain its accuracy, should be acquainted with all the requisites to make an exact analysis, in the same degree as the author of the work which he proposes to examine. Otherwise, he takes upon him to judge his master. Accurate chemical analysis is half an art, half a science. He who does not unite both in one, will never be in a condition to make an exact analysis. We must always chuse that method in which the precision of result depends the least on the dexterity of the operator. We must likewise be able, from long experience, to avoid all the circumstances by which the result would be rendered inaccurate, that neither from forgetfulness, carelessness, or ignorance, we may neglect to observe and remove them. These circumstances are generally of such a nature that chemists usually pay no attention to them; yet they are of considerable importance when our object is to obtain the utmost possible accuracy. The most skilful chemist will continue long an apprentice in the art of analysis, if he has not been in the habit, from the commencement of his studies, to attend to these circumstances. Long experience is even often necessary before we discover their existence, unless we have the advantage of being informed by others.

When we have to judge between two different analytical methods, or between two different chemists, we must be well acquainted with the methods employed; and both the method and the reputation of the author ought to enter for something in our judgment. If, for example, we have to compare two experiments on the composition of a metallic oxide, one of which was made by dissolving the metal in nitric acid in a phial, from which the acid was then evaporated, and the residue exposed to a strong heat: if the other was made by precipitating the solution in nitric acid by means of an alkali, filtrating, &c., it is presumable, that supposing the same dexterity in the operators, the first experiment is more accurate than the second, because it has been less exposed to the influence of circumstances. It is necessary, however, that he who judges of two results, should know the circumstances that may render the one or the other inexact. We must know, for example, that the first experiment can scarcely have any other error, than giving too great a quantity of oxygen to the oxide. This may be either owing to the application of too little heat, or (which is more usual) to the glass not having resisted the action of the nitric acid during the evaporation, so that a little nitre is formed, the acid of which increases the weight of the oxygen that we think we have found. (This circumstance long deceived me in my first experiments before I perceived it.) The other method of operating can have no other error than indicating too little oxygen, if ammonia has been em-

ployed to precipitate the oxide. But if a fixed alkali has been employed, the result may be too great, as well as too small; for if the precipitate has not been well washed, there will remain in it nitrate of potash or of soda. Or if too much alkali has been employed in precipitating, a portion of that alkali for the most part combines with the oxide, and cannot afterwards be removed by washing. But I should never have done, were I to point out here all the circumstances requisite to make an accurate analysis, and to judge of its accuracy.

It is obvious, that the theory of chemical proportions is intimately connected with the general theory of chemistry; but it is necessary, that those who labour either to examine it, to verify it, to extend, or apply it, should have their eyes constantly fixed upon the whole of chemistry. They must not adopt any theoretical explanation, till they have seen that it is not in contradiction with any other part of the theory, which we have reason to consider as well founded. This is a circumstance which chemists often neglect, though it is of the highest importance; and without attending to it, we cannot expect to make any valuable improvement in the theory of chemistry. Long custom is necessary, and a very extensive acquaintance indeed with chemistry, to have, so to speak, the whole science before our eyes, in order to be able to judge, without long reflection, whether an ingenious explanation of some phenomenon be inconsistent or not with some other part of the theory. It is from not attending to this circumstance, that chemists of the greatest distinction sometimes adopt ideas respecting some points, which are inconsistent with other opinions equally adopted by them.

I shall here give an example of this. Dr. Thomson, whose merits as a skilful chemist do not stand in need of any panegyric from me, has examined, (*Annals of Philosophy*, iii. 139,) the laws relating to the combination of oxides with each other; an examination for which chemists are much obliged to him. He then says, "the more I have examined this law, the more correct in general does it appear." In the same volume, page 106, speaking of *iodine*, this philosopher expresses himself as follows: "How much these new discoveries must alter the presently received chemical theory, and how they serve to confirm Davy's opinion respecting muriatic acid, is too obvious to escape attention." But had the author at this moment called to his mind the constitution of the submuriates with water of combination, he would have found that the laws concerning the combination of oxides, and the theory of Davy respecting muriatic acid, are absolutely irreconcilable; and that either the one or the other of these must be abandoned as erroneous. If, at this time, the external resemblance of crystallized iodine to the oxide of manganese crystallized had recalled the last of these to his mind, he would perhaps have found, that it is not more difficult to conceive how the first disengages an excess of oxygen to combine with the bases, than

how the last disengages an excess of oxygen to combine with the acids.\*

But I have allowed myself perhaps to be carried too far by ideas which have spontaneously followed each other in my mind. I hope, however, that I have drawn the attention of the reader to circumstances which may be of some importance in the examination of chemical proportions, as well as in the theory of chemistry in general.

## ARTICLE XI.

### Magnetical Observations at Hackney Wick. By Col. Beaufoy.

Latitude,  $51^{\circ} 32' 40.3''$  North. Longitude West in Time  $6^{\text{h}} \frac{13}{100}$ .

1814.

Month.	Morning Observ.			Noon Observ.			Evening Observ.	
	Hour.	Variation.		Hour.	Variation.		Hour.	Variation.
Dec. 18	8 <sup>h</sup> 50'	24° 17'	27"	1 <sup>h</sup> 25'	24° 21'	31"	Not observed.	Not observed.
Ditto 19	8 45	24 23	37	1 45	24 24	36		
Ditto 20	8 30	24 21	50	1 30	24 21	52		
Ditto 21	8 45	24 19	17	1 45	24 21	03		
Ditto 22	8 45	24 17	28	—	—	—		
Ditto 23	8 45	24 16	29	1 20	24 21	33		
Ditto 24	8 45	24 15	26	—	—	—		
Ditto 25	8 45	24 17	00	1 20	24 22	07		
Ditto 26	—	—	—	1 20	24 20	48		
Ditto 27	8 45	24 16	28	1 25	24 20	05		
Ditto 28	—	—	—	1 25	24 19	55		
Ditto 29	8 45	24 17	13	1 35	24 19	35		
Ditto 30	—	—	—	1 35	24 20	28		
Ditto 31	—	—	—	1 30	24 19	45		

1814.

Mean of Observations in Dec.	Morning	at 8 <sup>h</sup> 44'.....	Variation 24° 18' 02"	} West.
	Noon	at 1 30.....	Ditto 24 20 36	
	Evening	at —.....	Ditto — — —	
Ditto in Nov.	Morning	at 8 41.....	Ditto 24 16 20	} West.
	Noon	at 1 40.....	Ditto 24 30 37	
	Evening	at —.....	Ditto — — —	

\* I must make a remark here. It has already struck me more than once, that Dr. Berzelius and myself reason from different principles; for he has frequently pointed out facts which he said were inconsistent with the theory of muriatic acid by Davy, when I myself could not perceive any inconsistency in them. Even in the present example, I feel myself in the same predicament. I am unable to see any inconsistency between the facts established respecting the submuriates, and the opinions I entertain respecting chlorine. But if Dr. Berzelius will state this inconsistency in plain terms, so that I can see what he means, I shall examine it, and if I find it to be as he says, I shall be very ready to embrace his opinion.—T.

Mean of	Morning	at	8 <sup>h</sup> 39'	.....	Variation	24° 14'	08''	
Observations.	Noon	at	1 42	.....	Ditto	24 21	45	West.
in Oct.	Evening	at	— —	.....	Ditto	— —	—	Not obs.
	Morning	at	8 32	.....	Ditto	24 14	33	
Ditto in Sept.	Noon	at	1 39	.....	Ditto	24 23	17	West.
	Evening	at	6 19	.....	Ditto	24 16	50	
	Morning	at	8 30	.....	Ditto	24 14	13	
Ditto in Aug.	Noon	at	1 39	.....	Ditto	24 23	48	West.
	Evening	at	6 57	.....	Ditto	24 16	31	
	Morning	at	8 41	.....	Ditto	24 13	29	
Ditto in July.	Noon	at	1 42	.....	Ditto	24 23	44	West.
	Evening	at	6 58	.....	Ditto	24 17	00	
	Morning	at	8 44	.....	Ditto	24 13	10	
Ditto in June.	Noon	at	1 30	.....	Ditto	24 22	48	West.
	Evening	at	6 52	.....	Ditto	24 16	29	
	Morning	at	8 45	.....	Ditto	24 13	12	
Ditto in May.	Noon	at	1 44	.....	Ditto	24 22	13	West.
	Evening	at	6 38	.....	Ditto	24 16	14	
	Morning	at	8 45	.....	Ditto	24 12	53	
Ditto in April.	Noon	at	1 48	.....	Ditto	24 23	53	West.
	Evening	at	6 29	.....	Ditto	24 15	30	
	Morning	at	8 52	.....	Ditto	24 14	29	
Ditto in March.	Noon	at	1 52	.....	Ditto	24 23	08	West.
	Evening	at	6 11	.....	Ditto	24 15	33	
	Morning	at	8 47	.....	Ditto	24 14	50	
Ditto in Feb.	Noon	at	1 52	.....	Ditto	24 20	58	West.
	Evening	at	— —	.....	Ditto	— —	—	Not obs.
	Morning	at	8 52	.....	Ditto	24 15	03	
Ditto in Jan.	Noon	at	1 53	.....	Ditto	24 19	03	West.
	Evening	at	— —	.....	Ditto	— —	—	Not obs.
1813.	Morning	at	8 53	.....	Ditto	24 17	39	
Ditto in Dec.	Noon	at	1 51	.....	Ditto	24 20	30	West.
	Evening	at	— —	.....	Ditto	— —	—	Not obs.
	Morning	at	8 40	.....	Ditto	24 17	17	
Ditto in Nov.	Noon	at	1 54	.....	Ditto	24 20	24	West.
	Evening	at	— —	.....	Ditto	— —	—	Not obs.
	Morning	at	8 45	.....	Ditto	24 15	41	
Ditto in Oct.	Noon	at	1 59	.....	Ditto	24 22	53	West.
	Evening	at	— —	.....	Ditto	— —	—	Not obs.
	Morning	at	8 53	.....	Ditto	24 15	46	
Ditto in Sept.	Noon	at	2 02	.....	Ditto	24 22	32	West.
	Evening	at	6 03	.....	Ditto	24 16	04	
	Morning	at	8 44	.....	Ditto	24 15	55	
Ditto in Aug.	Noon	at	2 02	.....	Ditto	24 23	32	West.
	Evening	at	7 05	.....	Ditto	24 16	08	
	Morning	at	8 37	.....	Ditto	24 14	32	
Ditto in July.	Noon	at	1 50	.....	Ditto	24 23	04	West.
	Evening	at	7 08	.....	Ditto	24 16	43	
	Morning	at	8 30	.....	Ditto	24 12	55	
Ditto in June.	Noon	at	1 33	.....	Ditto	24 22	17	West.
	Evening	at	7 04	.....	Ditto	24 16	04	
	Morning	at	8 22	.....	Ditto	24 12	02	
Ditto in May.	Noon	at	1 37	.....	Ditto	24 20	54	West.
	Evening	at	6 46	.....	Ditto	24 13	47	
	Morning	at	8 31	.....	Ditto	24 09	18	
Ditto in April.	Noon	at	0 59	.....	Ditto	24 21	12	West.
	Evening	at	5 46	.....	Ditto	24 15	25	

*Magnetical Observations continued.*

1815.

Month.	Morning Observ.		Noon Observ.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
Jan.	1	8 <sup>h</sup> 50'	24° 17' 01"	1 <sup>h</sup> 35'	24° 20' 04"	
Ditto	2	8 50	24 17 50	—	—	
Ditto	3	8 50	24 17 24	1 35	24 19 46	
Ditto	4	8 50	24 19 20	1 55	24 20 32	
Ditto	5	8 50	24 18 00	1 35	24 20 50	
Ditto	6	8 45	24 17 02	—	—	
Ditto	7	8 40	24 15 48	1 40	24 19 19	
Ditto	8	8 45	24 16 53	1 40	24 20 43	
Ditto	9	8 40	24 16 50	1 35	24 19 23	
Ditto	10	8 40	24 16 35	1 45	24 17 46	
Ditto	11	8 45	24 14 22	1 35	24 19 00	
Ditto	12	8 45	24 15 58	—	—	
Ditto	13	8 50	24 17 04	1 35	24 19 30	
Ditto	14	8 45	24 18 08	1 20	24 20 37	
Ditto	15	8 45	24 16 30	1 25	24 18 26	
Ditto	16	8 40	24 17 02	1 35	24 19 41	
Ditto	17	8 45	24 16 42	1 50	24 20 01	

Not observed.

Not observed.

*Comparison of the Variations in the Years 1813 and 1814.*

		1813.	1814.	Difference.
April	Morning .....	24° 09' 18"	24° 12' 53"	+ 3' 35"
	Noon .....	24 21 <del>00</del> 2	24 23 53	+ 2 41
	Evening .....	24 15 25	24 15 30	+ 0 05
May	Morning .....	24 12 02	24 <del>12</del> 10	+ <del>0</del> 47
	Noon .....	24 20 54	24 23 13	+ 1 19
	Evening .....	24 13 47	24 16 14	+ 2 27
June	Morning .....	24 12 35	24 13 10	+ 0 35
	Noon .....	24 22 17	24 22 48	+ 0 31
	Evening .....	24 16 04	24 16 <del>10</del> 20	+ 0 <del>44</del> 25
July	Morning .....	24 14 32	24 13 29	- 1 03
	Noon .....	24 23 04	24 23 44	+ 0 40
	Evening .....	24 16 43	24 17 00	+ 0 17
Aug.	Morning .....	24 15 55	24 14 13	- 1 42
	Noon .....	24 23 32	24 23 48	+ 0 16
	Evening .....	24 16 08	24 16 31	+ 0 23
Sept.	Morning .....	24 15 46	24 14 33	- 1 13
	Noon .....	24 22 32	24 23 17	+ 0 45
	Evening .....	24 16 04	24 16 30	+ 0 46
Oct.	Morning .....	24 15 41	24 14 08	- 1 33
	Noon .....	24 22 53	24 21 45	- 1 08
	Evening .....	—	—	—
Nov.	Morning .....	24 17 17	24 16 20	- 0 57
	Noon .....	24 20 24	24 20 37	+ 0 13
	Evening .....	—	—	—
Dec.	Morning .....	24 17 39	24 18 02	+ 0 23
	Noon .....	24 20 30	24 20 36	+ 0 06
	Evening .....	—	—	—

Dec. 19.—In taking the mean of the observations for the month

of December, the variation on the 19th is rejected, on account of its being unusually great.

Jan 10, 1815.—The needles vibrated between three and four minutes; and in the night the wind blew very hard, with rain, from the west.

Jan. 11.—At the morning observation the needles vibrated three and four minutes. The wind still blew very hard, and continued during the day. At noon the vibration of the needles was seven minutes.

Rain fallen { Between noon of the 1st Dec. } 2.590 inches.  
 { Between noon of the 1st Jan. }

## ARTICLE XII.

### ANALYSES OF BOOKS.

*Memoirs of the Wernerian Natural History Society, Vol. II.*  
 Part I. For the Years 1811, 1812, 1813. Edinburgh, 1814.

THE Wernerian Natural History Society was founded in Edinburgh, in 1808, for the purpose of cultivating all the different branches of natural history; though, from the name given to it, we are led to suppose that geognosy occupied the chief place, at least in the opinion of the original founders; as that name is derived from Werner, the celebrated founder of that important science. We had no opportunity of noticing the first volume of *Memoirs* published by this Society, as it made its appearance before the commencement of the *Annals of Philosophy*. The present volume contains 20 papers, which we shall notice in their order.

1. *Outlines of the Mineralogy of the Ochil Hills*. By Charles Mackenzie, Esq.—This paper having been already published in the *Annals of Philosophy*, vol. iii. p. 116, we shall satisfy ourselves with a very short account of it here. The Ochils are a beautiful chain of hills, lying chiefly in Perthshire in Scotland, and dividing Strathern from the river district of the Forth. These mountains are all clothed with verdure, and many of them cultivated to the very top; hence the rocks of which they consist are seldom exposed, and the task of ascertaining their structure is attended with almost insurmountable difficulties. It is not surprising, therefore, that Mr. Mackenzie was unable to determine exactly the relative position of the different rocks, and consequently the class of formations to which they belong. For my own part, I consider the Ochils as very like the Pentlands in their structure, and think that both consist of a series of rocks, not exactly similar to any that Werner has described as constituting either the floetz or the floetz trap; though, the lowest beds excepted, they seem more connected with the latter than the former. I suspect that a

careful study of these two chains of mountains will occasion some alterations in the present Wernerian arrangement of floetz trap rocks. The rocks of which the Ochils are composed are the following :—

1. Red sand-stone.
2. Amygdaloid.—Basis clay-stone.
3. Grey sand-stone.
4. Lime-stone.—Very uncommon.
5. Slate-clay.—Also in small quantity.
6. Clay-stone.—Abundant.
7. Tuff.
8. Basaltic clink-stone.—Peculiar to the Ochils.
9. Green-stone.
10. Clay-stone porphyry.—Common.
11. Felspar porphyry.
12. Compact felspar.—Common.

The veins which occur in these mountains contain calcareous spar, steatite, sulphate of barytes, iron, cobalt, silver, copper, lead.

Were those geologists, who consider all green-stone, basalt, and porphyry slate, as lava emitted from volcanoes, to examine the structure and relations of the Pentlands and Ochils, it would probably induce them to modify their opinions.

2. *A Geological Account of the Southern District of Stirling-shire, commonly called the Campsie Hills; with a few Remarks relative to the two prevailing Theories as to Geology, and some Examples given illustrative of these Remarks.* By Lieutenant-Colonel Imrie, F.R.S. Edin. — This district constitutes a part of the very extensive independent coal formation, which occupies so great a portion of the southern part of Scotland, extending from the German Sea at St. Andrews to the western parts of Air-shire. The district described by Col. Imrie consists of ranges of hills running north and south, and separated by narrow valleys. He found that these hills are composed of beds which have the following position, beginning with the lowest bed,

1. Shale.
2. Dark bluish grey lime-stone with entrochi.
3. Slate-clay.
4. Sand-stone.
5. Slate-clay mixed with glance coal,
6. Dark bluish grey lime-stone,
7. Slate-clay.
8. Sand-stone.
9. Slate-clay.
10. Sand-stone.
11. A very thick bed of green-stone.

All these beds evidently belong to the independent coal formation, except the uppermost bed of green-stone, which from its

position, and its being confined to the top of the hills, probably belongs to the newest floetz trap.

Col. Imrie accounts for this disappearance of the green-stone, by currents of water which formerly flowed over this district from east to west; and he points out traces of these currents, in scratches still remaining on the surface of the green-stone, and in the particular configuration of the hills. This is the ingenious speculation of Sir James Hall, to which he has given plausibility by his usual industry and address. But it is a very hazardous speculation, the sources of mistake being almost innumerable, and the means of correcting error very scanty. The speculation seems to have originated with Saussure and Dolomieu, who, however, were never able to make much of it.

Col. Imrie has entered into some discussions respecting the igneous or aqueous origin of green-stone. He has shown, by an example from the neighbourhood of Carthage, that columns may be formed in substances deposited from water; and by an example from the Lipari Islands, that they may be formed likewise from lava. Obsidian and pumice he considers as of volcanic origin. I should like much to know, whether the composition of the obsidian found in Hungary be the same with that found in Iceland, Lipari, and the Peak of Teneriffe. It is very likely that they would be found to differ; and that the dispute about the aqueous or igneous origin of this mineral, has originated from giving the same name to two different species.

3. *Chemical Analysis of a Specimen of Magnetic Iron Ore from Greenland.* By Thomas Thomson, M. D. F.R.S. L. and E.—The analysis of the ores of iron constitutes one of the most difficult parts of mineralogical chemistry. The present example, compared with the Swedish ores lately analyzed by Berzelius, is sufficient to show, that more than one species of magnetic iron ore exists. Berzelius's ores were combinations of black and red oxides of iron. My ore contained titanium, and from the quantity of red oxide of iron obtained, (126.5 from 100 of the ore,) it is obvious that part of the iron must have been in the metallic state. This is further confirmed by the effervescence of the ore with muriatic acid. The speculation, that the ore consists of an unknown protoxide of iron, is supported by too slight analogies to be admitted.

4. *Description of a Sword Fish found in the Frith of Forth, in June, 1811.* By William Elford Leach, Esq. F.L.S. W.S. &c.—To this species, Dr. Leach gives the name of *Xiphias Rondeletii*, because he thinks it was first discovered and described by Rondeletius. The *Xiphias Gladius* is likewise probably found in the Frith.

5. *Some Observations on the Genus Squalus of Linnaeus, with Descriptions and outline Figures of two British Species.* By William Elford Leach, Esq. F.L.S. W.S. &c.—Dr. Leach divides the family of the squalinidae into four genera, as follows:—

\* *Pinna anali*.

1. *Pinna caudalis lunata* ..... *Squalus*.
2. *Pinna caudalis irregularis* ..... *Galeus*.

\* *Pinna anali nulla*.

3. *Os ante oculos situm* ..... *Squatina*.
4. *Os pone oculos situm* ..... *Acanthias*.

The two species which he describes, are, 1. The *galeus mustelus*, the *squalus mustelus* of Linnæus. The specimen examined was caught in the Frith of Forth. 2. The *squalus selanonus*, from a dried specimen in the Museum of the University of Edinburgh, caught in Lochfyne, and described by the late Dr. Walker.

6. *An Essay on Sponges, with Descriptions of all the Species that have been discovered on the Coast of Great Britain*. By George Montagu, Esq. F.L.S. and M.W.S.—This is a very curious and elaborate paper; but from its nature, scarcely susceptible of useful abridgment. Mr. Montagu considers sponges as animals destitute of all motion, and possessed of organs of digestion similar to those of plants; in short, they are plants as to structure and properties, but they are of an animal nature; because, when distilled, they yield the same products with other animal bodies. He gives a description of no fewer than 39 species of British sponges. Only 14 species were previously known, so that he has nearly tripled the list. I wish he had been at the trouble to make his title grammatical. He has made it *Spongia Britannica*, instead of *Spongiæ Britannicæ*, which is the true title.

7. *Mineralogical Description of Tinto*. By Dr. Macknight.—Tinto is a mountain in Lanarkshire, about 2300 feet above the level of the sea. According to Dr. Macknight, it is composed of floetz rocks, which, probably, rest upon grey-wacke, of which the neighbouring hills are composed. The lowest bed is a conglomerate, having a basis of clay, with a dark greyish colour, and somewhat resembling an intimately mixed green-stone. The fragments it contains are water-worn masses of transition rocks, as grey-wacke, grey-wacke-slate, iron-clay, and common flinty-slate, with veins of quartz. There are also nodules of quartz, mica, felspar, splintery horn-stone and felspar, passing into conchoidal horn-stone. Over this conglomerate, which constitutes the base of the mountain, masses of clay-stone, green-stone, and green-stone passing into clink-stone and porphyry-slate, successively appear, till we arrive at the summit of the mountain, which consists of compact felspar, and felspar porphyry. These different rocks probably constitute so many beds, but the point was not ascertained.

8. *Short Account of the Rocks which occur in the Neighbourhood of Dundee*. By the Rev. John Fleming, Flisk.—These rocks are all floetz. A porphyry with base of compact felspar alternates with sand-stone, west from Dundee; and the sand-stone fills up several hollows in the porphyry. Further east, rocks of green-stone and clay-stone appear, and the green-stone seems to pass into clink-stone.

9. *Observations on the Mineralogy of the Neighbourhood of St. Andrew's, in Fife.* By the Rev. John Fleming, Flisk.—That part of Fife, which is on the north side of the Eden, consists of floetz formations; but the southern part of the county is composed of the independent coal formation. St. Andrew's lies at the north-east corner of the independent coal formation. The rocks there are of two kinds: 1. Rocks belonging to the independent coal formation. 2. Rocks belonging to the newest floetz trap. The first are sandstone, coal, slate-clay, and clay-iron-stone. Of the second, only two rocks are described, namely, basalt and trap tuff.

10, 11. *Meteorological Observations on a Greenland Voyage, in the Ship Resolution, of Whitby, in 1811 and 1812.* By William Scoresby, Jun. M.W.S.—These observations are very curious and valuable, though not susceptible of abridgment. The ship went nearly as far north as latitude  $79^{\circ}$ , or within  $11^{\circ}$  of the pole. The thermometer even in June was seldom much above the freezing point, and sometimes below it. In these climates no summer seems to exist.

12. *Analysis of Pearl Spar.* By William Hisinger, Esq., of Stockholm. This paper is printed in French, which I think rather injurious to the Memoirs of the Wernerian Society. It would have been better to have translated it. The result of the analysis is as follows:—

Lime .....	27.97
Magnesia .....	21.14
Oxide of iron .....	3.40
Oxide of manganese .....	1.50
Carbonic acid .....	44.60
Loss. ....	1.39

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100.00

13. *Outline of the Mineralogy of the Pentland Hills.* By Professor Jameson.—The Pentlands are a well known and beautiful chain of mountains, situated immediately on the south side of Edinburgh. They stretch from north-east to south west, passing through the counties of Mid-Lothian and Peebles. The chain is upwards of 20 miles in length; but the portion described in this paper is confined to the north-east part of the chain, and constitutes a length of about eight miles. The rocks of which this portion are composed are of two kinds, namely, transition and floetz. The transition rocks lie lowest, and seem to constitute the basis of the Pentlands. Three transition rocks occur, namely, clay-slate, grey-wacke, and green-stone. The two first alternate. Mr. Jameson conceives that the grey-wacke is an original deposit, that it never was in the state of loose sand, afterwards cemented together by some unknown process; but was originally deposited from a liquid in the state in which we find it. This opinion, I conceive, is very likely to be correct. The rock itself bears scarcely any marks of being composed of fragments that had been subjected to

trituration in water; but I do not think that Mr. Jameson has been quite so fortunate in his name as in the idea itself. All rocks, which are still in the original state in which they were deposited, he calls *chemical deposits*; while he distinguishes those which have been in the state of sand, or fragments, and afterwards cemented together, by the name of *mechanical deposits*. Now a chemical combination means substances united together by chemical affinity; and one of the characteristics of this kind of combination is, that the constituents enter always in the same proportion. I am afraid, that according to this definition, very few rocks would be entitled to the name of chemical deposits, while some of those that Mr. Jameson would distinguish by the name of mechanical, (as shell lime-stone,) would be in reality entitled to the name of chemical. Some other phrase, (*immediate deposits*, for example,) might easily be chosen to denote the idea, which then, in my opinion, would be unexceptionable.

The floetz rocks, which constitute by far the greatest portion of the Pentlands, are conglomerate, sand-stone, clink-stone, clink-stone porphyry, amygdaloidal clink-stone porphyry, green-stone, compact felspar, clay-stone, clay-stone tuff. and porphyry.

The conglomerate rock is composed of portions of quartz, grey-wacke, grey-wacke slate, porphyry, felspar, flinty-slate, common jasper, horn-stone, and mica. Two varieties of it occur, a hard and a soft. It lies low in the hills, and Mr. Jameson supposes that it alternates with grey-wacke. The sand-stone covers, and probably alternates with the conglomerate. Mr. Jameson considers it as an immediate deposite. The clink-stone seems to be over and alternate with the sand-stone. It often contains crystals of felspar, and is then called clink-stone porphyry, or porphyry slate. It passes into compact felspar. The green-stone is an uncommon rock in the Pentlands; but the compact felspar occurs in great abundance. It passes into clay-stone, which is a very copious rock, and often constitutes the summit of the different mountains. The porphyry, which is merely clay-stone containing crystals of felspar, often covers it.

14. *On conglomerated or brecciated Rocks.* By Professor Jameson. In this paper, Mr. Jameson endeavours to show, that many rocks hitherto supposed to be composed of fragments of older rocks cemented together, are in fact immediate, or, as he calls them, *chemical*, deposits from a liquid. The idea certainly deserves to be carefully examined. I have little doubt that, to a certain extent at least, his notions will be found correct; but it is difficult to reason on such a subject in a satisfactory manner, because the evidence can only be derived from an ocular inspection of the rocks in question. Conglomerated rocks occur in primitive, transition, and floetz formations. To the first belong conglomerated gneiss, conglomerated mica-slate, conglomerated granite, and conglomerated lime-stone. All of these Mr. Jameson considers as immediate deposits; because the supposed fragments pass insensibly

into the basis of the rock. The transition conglomerated rocks are grey-wacke, sand-stone, and lime-stone. He considers all these as immediate deposits, for a similar reason with the primitive conglomerates. The floetz conglomerates are sand-stone conglomerate, sand-stone, and trap tuff. The first and last he considers as always immediate deposits; the sand-stone as frequently so.

15. *On Porphyry*. By Professor Jameson.—Werner admits into his system of geognosy five kinds of porphyry; namely, clay-porphyry, horn-stone-porphyry, pitch-stone-porphyry, obsidian-porphyry, and pearl-stone-porphyry; all of which are primitive rocks. The object of this paper is to show, that there exist likewise transition and floetz porphyries. Transition porphyry has a basis of clay-stone or felspar. It contains crystals of felspar, grains of quartz, scales of mica, and crystals of hornblende. It occurs abundantly in Dumfriesshire, and between New Galloway and Dumfries, where it alternates with grey-wacke, grey-wacke-slate, and transition green-stone. *Floetz porphyry* has likewise clay-stone or felspar for its basis, and contains crystals of common felspar, glassy felspar, and quartz. It occurs in the islands of Arran, Rousay, and Skye, and in the Ochils and Pentland hills.

16. *Mineralogical Observations and Speculations*. By Professor Jameson.—These observations and speculations relate to three particulars: 1. Stratification. This first speculation is rather of a bold nature, as Mr. Jameson endeavours to explain in what manner the crust of the earth was originally formed. His notion seems to be, that the whole earth is a crystal, that the tabular masses or strata are the folia of this crystal. He even affirms, that these strata meet under determinate angles, and are to be considered as the same with the *cleavage* of common crystals. I am sorry that in this very uncommon and novel view of stratification, I cannot agree with the opinion entertained by my very ingenious friend. The laws of crystallization have been studied with sufficient care and success to enable us to decide, without hesitation, that the earth cannot be a crystal, and that its strata are not the same with the folia of crystals. The figure of crystals is owing to the figure of their ultimate particles, and the particular way in which they unite. Without chemical affinity and chemical combination we can have no crystal. The different plates of a crystal are arranged by the same law, and they are attached to each other by chemical affinity. But what chemical affinity can unite the different strata of the earth with each other? Where is the chemical affinity between lime-stone, slate-clay, coal, and sand-stone, for example; or between sand-stone, green-stone, slate-clay, or basalt? Yet what is more common than such alternations. To say that the strata form every where regular angles with each other, as happens with the plates of a crystal, is saying what has never been proved or even rendered probable. The last part of the speculation, that *different strata are often of simultaneous formation*, is, to a certain extent at least, more probable than the former part; though I think

that a more careful and rigid examination, than Mr. Jameson has bestowed on it, would be necessary in order to establish it, or even to render it probable. 2. On veins. Mr. Jameson considers veins as of two kinds. Some have been open rents afterwards filled up. Some have never been rents, but have been formed at the time the strata were deposited, or soon after, by a kind of crystallization in the rock. These he calls cotemporaneous veins, and he conceives them to be much more numerous than has hitherto been supposed. This opinion, which is very ingenious, is ably supported, and is, I think, very likely to be true. 3. On coal. Coal has been hitherto usually considered as vegetable matter, more or less changed in its nature; but Mr. Jameson believes that glance coal, and black coal, are original chemical deposites, as little connected with vegetable remains as the shells in lime-stone are with the lime-stone. His reasons are, that they occur in primitive rocks, as gneis, mica-slate, clay-slate, and appear to be of cotemporaneous formation with these rocks. Brown coal, on the contrary, he considers as of vegetable origin.

17. *Observations on the Natural History of the Colymbus Immer.* By Dr. Arthur Edmondstone.—This is a large bird which frequents the Zetland Islands in winter, but disappears in summer. It lives on the water, and seldom lands. It is the opinion of the Zetlanders that it cannot fly, and that it hatches its eggs under its wings. Dr. Edmondstone had an opportunity of refuting the first of these notions; for one, which he wounded, after diving, took to wing and flew a hundred yards, when exhausted by loss of blood it fell and was taken. It no doubt migrates to Greenland, or the North Cape, during the breeding season.

18. *Contributions to the British Fauna.* By the Rev. John Fleming, F.R.S.E.—Dr. Fleming gives a description of the following animals hitherto unknown, or nearly so, in Great Britain. 1. *Sorex fodiens*, or water shrew. 2. *Pleuronectes punctatus*. 3. *Lepas fascicularis*. 4. *Hirudo verrucosa*. 5. *Echinus miliaris*. 6. *Lucernaria fascicularis*. 7. *Caryophyllia cyathus*. 8. *Fungia turbinata*. 9. *Flustra ellisii*. Curious information is given respecting these animals, which I am obliged to omit here, as it is scarcely susceptible of abridgment.

19. *Description and Analysis of a new Species of Lead Ore from India.* By Thomas Thomson, M.D. F.R.S., &c.—The specimen examined was brought by Dr. Heyne from Madras, where it is sold in the shops as a medicine. It approaches most nearly to galena in its appearance, though its characters are a good deal different from those of that mineral. I found it composed of

Lead .....	50.059
Copper .....	32.500
Iron .....	1.370
Sulphur .....	11.328
Loss .....	4.743

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100.000

The loss is too great; but I was not supplied with a sufficient quantity of the ore to repeat the analysis. From my mode of proceeding, I think it likely that the whole, or nearly the whole, of the loss was sulphur. In that case the ore would be a compound of

Sulphuret of lead .....	57·269
————— copper .....	40·850
————— iron .....	2·190
	<hr/> 100·309

Or, supposing the sulphuret of iron accidentally present, it is composed of an integrant particle of sulphuret of lead and two integrant particles of sulphuret of copper.

20. *Notice concerning the Structure of the Cells in the Combs of Bees and Wasps.* By Dr. Barclay.—It appears, from this communication, that the partitions between different cells in the combs of bees and wasps are all double; or in other words, that each cell is a distinct, separate, and in some measure an independent structure, agglutinated only to the neighbouring cells: and that when the agglutinating substance is destroyed, each cell may be entirely separated from the rest.

### ARTICLE XIII.

#### *Proceedings of Philosophical Societies.*

##### ROYAL SOCIETY.

On Thursday, the 12th of January, the remainder of Mr. Travers's paper on the way in which the eye accommodates itself to the sight of objects at different distances, was read. He considers the iris as muscular, and as connected with a kind of inner iris, or ring, which by its contraction increases the convexity of the lens. The eye he conceives is only acted upon by the stimulus of light. Hence he believes that the contraction of the iris is regulated entirely by the retina. The eye, in his opinion, is fitted naturally for viewing distant objects. When near objects are viewed the iris contracts, in consequence of which the inner ring acts upon the lens, and increases its convexity. The sensation of fatigue he supposes owing to the over-action of the external muscles of the eye; and not to any fatigue in the iris itself.

On Thursday, the 19th of January, a paper by Dr. Storer was read, giving an account of a well dug in Bridlington harbour, Yorkshire, within high water-mark. The bottom of the harbour is a bed of clay; through this they bored to the rock below; a tinned copper pipe was then put into the circular cavity, and the whole properly secured. The cavity was soon filled with pure water. When the tide rises to within about 50 inches of the mouth of this well, the fresh water begins to flow over, and the quantity flowing

increases as the tide rises, and the flow continues till the tide sinks more than 50 inches below the mouth of the well. During storms, the water flows in waves, similar to the waves of the sea. Mr. Milne accounts for the flowing of this singular well in this way: the whole bay, he conceives, has a clay bottom. The water between the rock and this clay can flow out nowhere except at the termination of the clay, which is under the sea. As the tide rises, the obstruction to this mode of escape of the water will increase. Hence less will make its way below the clay, and of course it will rise and flow out at the top of the well.

At the same meeting a paper by Dr. Brewster was read, on the effect of pressure on transparent animal bodies, in causing them to polarize light. His first experiment was with a film of calf's-foot jelly. At first it produced no effect upon light; but as it became more and more firm it depolarized the light, at first at the edges, and at last throughout. The same thing was the case with a film of izinglass. When these films were subjected to pressure they depolarized light at first, and exhibited those complimentary colours which are peculiar to crystallized bodies.

#### LINNÆAN SOCIETY.

On Tuesday, the 17th of January, a paper by the Rev. Patrick Keith, on the epidermis of plants, was read. He gave an historical account of the different opinions entertained by vegetable physiologists respecting the epidermis. He himself considers it as composed of fibres rather than cells, an opinion adopted by some other persons. He gave an account of the epidermis of saffron, in which he found a peculiar structure. He observed pores likewise on the veins of leaves, where hitherto it has been supposed that they do not exist.

#### GEOLOGICAL SOCIETY.

In consequence of the great distance of the Editor from the place of meeting of the Geological Society, it has not been in his power for some time past to continue the regular details of the interesting papers read at their meetings. He now resumes these details from the time that they were interrupted.

March 4, 1814, a memoir, entitled, Observations on the Hill of Kinnoul, by Dr. Macculloch, V. Pr. G. S. was read.

The hill of Kinnoul, in the vicinity of the town of Perth, rises about 600 feet above the plain of the Tay. It is nearly a mile in length, and exhibits many abrupt faces in a state of constant ruin and depredation. It consists principally of floetz trap, sometimes in the state of black basalt, sometimes of a brownish red colour, but retaining the other characters of basalt; sometimes rendered porphyritic by the presence of numerous minute crystals of opaque felspar; sometimes porphyritic and amygdaloidal; and sometimes simply amygdaloidal, with a base of basalt, often passing into wacke. The concretions of the amygdaloid are green earth or chlorite in small grains, and varying in structure from compact to

completely scaly; calcareous spar, either single or mixed, with quartz or agate; quartz either common or amethystine; and agate and coloured and zoned chalcodony: sometimes the agate is partly stalactitical and partly zoned from green heliotrope without the red spots by which it is usually marked which also occurs in veins.

On the top of the great mass of trap is a bed of conglomerate, consisting of trap pebbles imbedded in a cement of the same nature. The most remarkable circumstance, however, in the hill of Kin-noul, is the variety of interesting junctions which it presents of the trap with grey-wacke-slate. These two rocks are intermixed and involved with the other in various ways, and the slate adjacent to the planes of contact is inflated and spongy, exhibiting a structure very analogous to that of the burnt micaceous schistus which is sometimes found in the walls of the vitrified forts, except that in the former the cavities are filled with calcareous spar.

The peculiarities in the structure of some of the agates, and in the trap and grey-wacke at the places of their junction, are in the opinion of the author of this paper, not to be accounted for by the exclusive agency either of fusion or of consolidation from aqueous solution.

A letter from Mr. Henry Sports, of Salisbury, on the formation of flint was also read. Certain of the flint nodules exhibit unquestionable marks of animal organization; and from this fact Mr. S. supposes that all flint has originated from sponges, alcyonia, and the spongy zoophytes, converted by some unknown process into silicious earth.

On the 18th of March and 1st of April, a paper by Nat. Wynch, Esq. on the geology of part of Northumberland and Durham was partly read.

Mr. W. begins his paper with a description of the magnesian lime-stone which makes its first appearance at Cullercoates, in Northumberland, and stretches in a S. W. direction between the rivers Tyne and Tees. At Whitley quarry, near Cullercoates, this formation may be seen resting on the coal strata; and in other parts the workings of adjacent collieries have been actually driven to a short distance under the lime-stone, although no sinkings begun in the lime-stone have ever been carried down as far as the coal. It is not, therefore, ascertained that the coal is of equal extent with the lime-stone; but unfortunately it is too well known that the coal has constantly proved to be excessively deteriorated where covered by the magnesian lime-stone. This formation is composed of strata of lime-stone of various qualities and appearance, such as white, brown, and fetid, very ferruginous, oolitic, &c. alternating with shale, and traversed by thin strings of galena. In the beds of the Tees occurs a red sand-stone, the geological relations of which are but little known. Sinkings have been made in it in different places to the depth of from 70 to 80 fathoms in search of coal, but wholly without success.

*The coal formation rests upon the lead-mine measures, and is in*

part covered by the magnesian lime-stone. It is in the form of a trough, the extreme length of which from N. to S. is 58 miles; and its breadth from the sea-coast westwards is about 24 miles. The general inclination of the coal strata is one yard in 20, but subject to considerable local irregularities. The earthy beds which separate the coal seams from each other are potter's clay, slaty-clay in various states of induration, and sand-stone both massive and slaty. Of the latter beds some are quarried for flag-stone; and one, a buff-coloured fine-grained sand-stone, called the grind-stone sill, furnishes the celebrated Newcastle grind-stones. Thin beds and nodules of clay-iron-stone, the latter containing impressions of ferns and bivalve shells, occur in the shale or slaty-clay. A few dykes of basalt or green-stone intersect the coal formations. The best known is called Walker's Dyke. It is composed in Walker Colliery of two solid and parallel walls of green-stone of the thickness of three yards and six yards, with an interval of about 11 feet, composed of fragments of green-stone and sand-stone imbedded in blue slate. The dyke is perfectly vertical, and divides, but does not dislocate or heave the strata which it traverses. The coal on each side of it, to a distance of three to six yards, is converted into a hard cellular cinder, the cells of which are often occupied by calcareous spar and sulphur.

Fissures or slips are of frequent occurrence in this as in every other coal-field, and occasion much trouble and expense to the miner. Numerous mineral springs, containing more or less of common salt, occur in the whole of the coal-field.

*April 15.*—A communication from S. Solly, Esq. on the newer formations, particularly that of floetz trap, was read.

From the remarkable differences, notwithstanding their general resemblance, which prevail amongst the floetz trap, and other recent floetz rocks of the same species in different countries, Mr. Solly is induced to suppose that these are not parts of an universal formation, as is held by Werner and his pupils, but that they originate from local deposits. These local deposits are considered by the author of the paper as owing their peculiar structure and other characters partly to the influence of heat derived from the electric fluid, and partly to the action of crystalline polarity operating within them, while they were apparently in a quiescent state.

A paper by Dr. Berger on the geology of the North of Ireland was begun.

*May 6.*—The reading of Dr. Berger's paper on the geology of the North of Ireland was continued.

A paper entitled A Description of the Tunnel of Tavistock Canal through Morwel Down, in the County of Devon, by John Taylor, Esq. M. G. S. was read.

Morwel Down is a hill near Tavistock, and to the west of that place, which separates the valley of Tamar from that of the Tavy. The height of this hill is about 700 feet above the tide-way in the river Tamar. It is composed of schist (in the dialect of the country

killas), and in the immediate vicinity of the mines which have recently been opened in that part of the county.

In 1803 an Act of Parliament was obtained for cutting a canal from the town of Tavistock to Morwelham, a quay on the river Tamar. An essential part of this plan was a tunnel through hard rock, about a mile and three quarters in length; passing through Morwel Down at an average depth, from the surface of about 400 feet, and in a direction calculated to cut through all the E. and W. or metalliferous veins that might traverse the hill. Of this important undertaking somewhat less than one-fourth remains to be performed; but the portion already executed has disclosed several important geological facts, which are detailed in this paper, and in the section and specimens by which it is accompanied.

Six beds or dykes have been cut through, the thickness of which varies from six to 26 fathoms. Their direction is inclined to that of the metalliferous veins, and they dip pretty uniformly to the North; they are composed of clay-porphry, of another variety of porphry, of quartz, and of quartz mixed with chlorite.

Several metallic veins, some of them already productive, and affording copper, and in a few instances tin, have also been discovered; they traverse all the strata, and exhibit a remarkable difference in their dip or under-lay on the two sides of the hill, those on the north side dipping to the north, and those on the south side dipping to the south.

*May 20.*—A communication from Lord Webb Seymour was read.

This paper is accompanied with explanatory drawings, and describes an instrument of his Lordship's invention called a clinometer, for the purpose of determining the position of the planes of stratification. The instrument itself had been previously presented to the Society.

A paper, accompanied with specimens, containing mineralogical remarks on part of the settlements of the Hudson's Bay Company, compiled from the reports of several observers, by the Earl of Selkirk, was read.

From Mr. Aulds it appears that the bed of Nelson river, seven miles below Hamborough Head, and not far from its mouth, presents several rapids, which are formed by flat strata of lime-stone, and in one instance by whin-stone (probably one of the trap rocks by which stratified lime-stone is so often accompanied). The effect of the ice floods in this river is very remarkable. The water charged with pieces of ice appears to be capable of detaching large blocks of lime-stone, and conveying them to a considerable distance. A deposit of this kind, the work of a single season, is described as forming a shoal 200 or 300 yards in length, and consisting of small stones covered by several hundred blocks of the same kind, weighing several tons each, and four feet and more in thickness.

There is reason to believe that the same lime-stone formation extends through all the country west of Lake Wimpie, as far as the

rocky mountains. On the verge of these mountains coal has been observed ; but the interior part of the ridge is described as composed of very hard rock, the stratification of which is nearly vertical.

A notice, accompanied by a very fine specimen of sulphate of strontian, by G. Cumberland, Esq. was read.

This specimen, with several smaller ones, was found in digging a vault in a burial-ground in Portland-street, Bristol. It is a nodule of considerable magnitude, pretty compact externally, and loosely filled up with crystals of larger size and more perfect figure than is usual in English specimens.

*June 3.*—A letter from Edward Smith, Esq. on the stream works at Pentowan, addressed to Dr. Wollaston, was read.

From the summit of Hensbarrow Hill to the sea-coast is about seven miles. A small river takes its principal rise in the above-mentioned hill, and after a rapid course, in many parts interrupted by cascades, falls into the sea near St. Austle. On hasty rains the rise of this stream is sudden, and often dangerous, but in dry seasons it is an inconsiderable rivulet. In the valley through which it flows are two stream-works, the upper and the lower.

The upper has been opened to the depth of 40 feet, and consists of six strata, or more properly alluvial beds. The uppermost, three feet thick, is the surface soil, with trees growing on it : the second is mud mixed with gravel, about 20 feet thick : the third is three feet thick, and consists of fragments of spar and killas ; the fourth, five feet thick, consists of gravel and decomposed granite, the same as occurs at Hensbarrow Hill ; the fifth bed, five feet thick, is gravel, covering large trunks and branches of oak : the sixth bed, five feet thick, is coarser gravel, intermixed with which are rounded pieces of tin-stone. This bed rests not on the rock, but on a kind of clay : it is, therefore, possible that there may be below other alluvial beds which have not as yet been examined.

The lower stream-work is about three quarters of a mile below the upper ; and, like the latter, consists of a bed of tin-stone, five feet thick, in small rolled pieces, mixed with other fragments, and covered by about 54 feet of alluvial matter in several beds. Some of these are mere gravel ; others are largely mixed with trees and other vegetable matter, among which have been found the horns of an animal of the stag kind ; some large bones, supposed by Mr. Smith to be those of hippopotamus, and parts of two human skulls. There are also two beds of blue mud, containing marine shells.

A paper by Dr. Macculloch, being a supplement to a former communication of his on quartz rock, was read.

The object of the author in this paper is to confirm, by additional examples, his former position, that quartz rock is essentially composed of fragments and rolled pieces, and that it alternates with mica-slate and other rocks, which have hitherto been considered as a primitive.

*June 17.*—The reading of Mr. Wynch's paper on the mineralogy of Northumberland and Durham was concluded.

Below the *coal measures*, described in the former part of this paper, occur the *lead-mine measures*, which consist for the most part of lime-stone, sand-stone, and whin. The whin is a true trap rock, composed of felspar and hornblende, and is found in three different situations; first as dykes, or nearly vertical beds intersecting the regular strata; secondly, inclosed between the regular strata, and considered in general as one of them, although its thickness varies much more than that of the other strata, the *great whin sill* being in some places only six fathoms thick, and in others amounting to 20, and even 30 fathoms. Like the toad-stone of Derbyshire, it is not penetrated by any of the metallic veins that abound in the adjacent lime-stone. The third situation in which the whin occurs is in overlying masses with the external appearance of common coarse-grained basalt, and often shooting into regular columns of considerable magnitude.

The principal repository of lead is called the Great Lime-stone, about ten or eleven fathoms in thickness, and divided by indurated clay into three distinct strata. It is of a brownish or dark bluish-grey colour, and abounds in encrinites and bivalve shells. From an analysis of it by Sir H. Davy, it appears to be nearly pure carbonate of lime, containing neither magnesia nor alumina, and only about one per cent. of oxide of iron.

The *scar lime-stone* resembles the former in external characters, and the fossils which it contains; and is also divided into three strata, the aggregate thickness of which is about five fathoms.

The *wehle shell lime-stone* is a thin bed, scarcely amounting to two feet, and is characterized by its containing bivalves, like oyster shells, four or five inches in diameter.

The next bed, called the Tyne Bottom Lime-stone, is an encrinal lime-stone divided into three strata, of the aggregate thickness of about 21 fathoms. It forms the bottom of the Alston Moor mining-field, but is nearly the uppermost bed at Dufton.

Below this are five other beds of mineral lime-stone, resembling the preceding.

Between the beds of lime-stone occur numerous beds of sand-stone, varying in structure from completely slaty to solid amorphous rock fit for mill-stones. Of these the brown ferruginous beds called *hazles* are found to be much more productive of lead than the others.

The common lead ore of this district is galena, holding silver in various proportions, from two ounces in the fother (21 cwt.) to 42 ounces. The general average, however, is about 12 ounces.

The *lead measures* rest upon the red sand-stone, and their total thickness is supposed to be about 457 fathoms. The metallic contents of the veins, besides several varieties of galena, are carbonate of lead, both crystallized, massive, and earthy, phosphate of lead, copper pyrites, azure copper ore, malachite, iron pyrites, sparry iron ore, blende, calamine, and siliciferous oxide of zinc. The vein-stones are quartz, several varieties of calcareous spar, coral-

loidal arragonite (flos ferri), pearl spar, crystallized and sandy fluor, witherite, and heavy spar.

The N. W. of Northumberland is occupied chiefly by the Cheviot Hills, which consist of various porphyritic and anygdaloidal rocks, accompanied on the side adjacent to the basset of the lead-mine measures by red sand-stone and grey-wacke-slate.

A communication from W. H. Pepys, Esq. Treas. G. S. was read.

This paper contains experiments to ascertain the composition of the sand tubes found at Drigg, from which it appears that the vitrified sand of which they are composed consists of 93 silex, 4.5 alumina and oxide of iron, 1 lime, 0.2 alkali.

Nov. 4.—A paper by Dr. Berger, on the whin dykes of the North of Ireland, was read.

The dykes of the district here mentioned are found at all the elevations from the level of the sea to the summit of Arragh, the highest mountain of Donnegal, being a range of 2220 feet. Where one occurs others are generally to be found in the immediate vicinity, and all of them running in parallel, or nearly parallel, directions to each other. This parallelism moreover obtains not only in adjacent dykes, but in almost all of this district, their general direction being N. W. and S. E., or perpendicular to that of the strata and the metallic veins, both of which are intersected by them.

The width of dykes varies considerably, from a few inches to several hundred feet; and upon the whole it appears that those which intersect primitive strata are narrower than those which intersect secondary strata. The substances which compose dykes being for the most part in a state of greater induration than the beds which they traverse, they may be perceived protruding above the general surface, either in the shape of vertical walls, in some instances 40 feet high, or of a mound composed of loose stones. The depth to which the dykes descend is wholly unknown; nor do they appear to become narrower in proportion as they descend.

The usual materials of dykes are trap and green-stone, with lydian-stone, flinty-slate, grey-stone, and wacke. Ferruginous sand-stone also occurs, but more rarely. The structure of a dyke presents columnar, or ovoid, or nearly rectangular concretions, laterally aggregated, and with their axis perpendicular to the walls; and the more compact the trap is the more apt it is to assume the columnar form.

Nov. 18.—The Secretary reported that a communication had been received from Wm. Phillips, Esq. M.G.S. on the granite of Cornwall.

A notice of some fossils found in the neighbourhood of Cambridge, by the Rev. I. Hailstone, Woodwardian Professor, M.G.S. was read.

The chalk hills of Cambridgeshire, where they terminate in the

north, appear to rest on a bed of calcareous blue clay, provincially called *galt*. This bed, covered in some places with gravel, forms the extensive flat which connects the upper parts of the country with the fens.

The thickness of the *galt* bed, judging from the depth of the wells which have been sunk in it, is probably not less than 200 feet, and it is considered by the Professor as the lowest bed of the chalk formation. Above this bed lie several strata of a chalky stone, called *clunch*, which is an argillaceous lime-stone in great request, and of which large quantities are dug and burnt at Cherry Hinton, and other places in the county. The *clunch* contains no flint, but abounds in the common radiated pyrites. On some of the highest of the hills near Cambridge is found a deposit of gravel and loose stones in horizontal layers, which differs in so many respects from the gravel of the subjacent flat country that it must probably be considered as the deposit of another epoch. It consists principally of pale blue flint, enclosing numerous traces of ascidia, but also contains fragments belonging to the oolite formation of the neighbouring counties of Northampton, Rutland, and Lincolnshire, with occasional fragments of basaltic rocks.

The characteristic fossil of the clay or *galt* bed is that variety of belemnite described by Lister under the name of *lapis lycuricus*. There have also been found an obscure organic remain, considered by the Professor as a medusa or beroë; also a beautiful ammonite, a pentacrinite, and several species of bivalve shells. The mutilated remains of a species of fish have also been found; and charred wood is not uncommon among the animal remains.

Among the rarer fossils of the Cambridgeshire chalk, are fruit cones and linear leaves from Cherry Hinton.

The results of an analysis by Mr. Holme of three varieties of Cherry Hinton lime-stone are subjoined, from which it appears that the stone consists for the most part of carbonate of lime, but also contains from about four to nine per cent. of alumina, and a portion of siliceous matter not amounting to two per cent.

Two letters addressed to the Secretary from the Rev. Richard Hannah, jun. Chaplain to the troops at Plymouth, were read.

Some geologists having doubted or denied the existence of organic remains in the lime-stone of Plymouth, the object of Mr. Hannah's letters is to state that both madrepores and shells have recently been found by him in the quarries of Stonehouse Hill, and at the Dock-yard; a fact entirely corroborated by the specimens which Mr. H. has also transmitted for the cabinet of the Society.

Some observations on the interruptions or faults to which mineral veins are liable, contained in a letter addressed by Mr. I. Sadler to Ant. Carlisle, Esq. M.G.S. were read.

The cases solved in this paper are illustrated by diagrams, which renders it impossible to give an abridgment of them.

Dec. 2.—A letter from L. Horner, Esq. to the Secretary was

read, describing certain specimens presented by him to the Society. They consist of two series : one exhibiting the junction of sandstone and of trap at the Kinnoul, near Perth ; the other exhibiting a similar junction occurring in Salisbury Craig, near Edinburgh.

A paper by J. Macculloch, M.D. V. Pr. G.S. entitled *Observations on the Mountain Cruachan*, was read.

Cruachan, the highest of a cluster of mountains resembling each other in external form and geological composition, is situated in the east bank of the river Awe, between Loch Awe and Loch Etive. Instead of the rugged forms and rough faces that characterize the hills of mica-slate, situated to the south of this district, and forming the basins of Loch Lomond, Loch Lang, and Loch Fyne, it presents a more uniform flowing outline, a more complete covering of herbage, and a less serrated summit, which at the same time is strewn with heaps of fragments. On a near approach the rolled stones which abound in the beds of the torrents appear to be almost entirely composed of granite and porphyry ; but the rock which appears *in situ* forming the lower part of the mountain is schist. This schist sometimes assumes the appearance of a compact mica-slate, but generally of clay-slate, the colour of which varies, from dark lead-grey to pale greenish-grey. Innumerable veins of granite and of syenitic granite of various sizes traverse the schist in all directions ; sometimes passing clean through it, sometimes intermingled with it, or involving fragments of it. The schist is often excessively curved and contorted ; and at the points of junction with the granite is either indurated and converted into lydian-stone, or appears insensibly melting, as it were, into the granite, and assuming the character of hornblende-slate.

The schist does not appear to reach above one-third of the entire height of the mountain ; but nearly half of the remaining ascent is thickly strewn with loose rocky masses, and it is not till these have been surmounted that the crags and precipitous faces of granite begin to show themselves. This granite nearly resembles that of Carngorum, being composed of reddish felspar and white quartz in nearly equal proportions, with a small intermixture of mica ; that which composes the summit strongly affects the magnetic needle.

Veins of compact felspar-porphyry make their appearance in great abundance in this granite : this breadth varies from three or four feet up to 40 or 50, or more ; they are nearly vertical, but run horizontally in all directions. They traverse not merely the granite, but the schist likewise, and even the granite veins of the schist, differing remarkably, however, from these last in occasioning no disturbance in the strata through which they pass, nor intermixing in the least degree either with the granite or with the schist.

Besides the veins of granite and of porphyry, are veins of a grey trap rock, and others of a perfectly characterized basalt.

Similar appearances to these now described occur in the adjacent districts of Midlorn, of Argyle, and of Glenco.

## WERNERIAN SOCIETY.

At the meeting of this Society on the 3d of December, there was read a communication from Captain Brown, of the Forfarshire Militia, describing five new species of shells, observed by him in Ireland. The description was accompanied by excellent drawings. A mineralogical communication from Mr. Giesecke was read, in which he described several new and rare minerals observed by him in West Greenland.

At the meeting of the 17th of December, the Secretary read a letter from Mr. Scott, at Ormiston, addressed to Lauder Dick, Esq.; containing a description of the junction of the transition and floetz rocks near Jedburgh, and mentioning the occurrence of granite in Roxburghshire.—Professor Jameson read the first part of a paper describing the mineralogy of the south district of Scotland, including a particular account of the rocks which occur in the neighbourhood of Dunbar, in East Lothian. The coast of Dunbar exhibits an alternation of the old red sand-stone with rocks of the trap formation; and interesting illustrations of the supposed chemical nature of sand-stone, and of the contemporaneous formation of masses of lime-stone in sand-stone, and of trap in sand-stone. At the same meeting, the first part of a memoir on the nature of the polar ice, by Mr. Scoresby, was read.—Specimens of sand-stone, brought from Berskimming, in Ayrshire, by Mr. Miller, were exhibited. When cut into long thin bars they possess considerable flexibility, which is a good deal increased when the stone is wetted.

## ARTICLE XIV.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS  
CONNECTED WITH SCIENCE.I. *Phosphuret of Ammonia*,

M. VOGEL has lately made a curious experiment in Paris. He put a piece of phosphorus into ammoniacal gas, and then exposed the phosphorus to the solar rays. A black matter was formed, which was conceived to be a combination of phosphorus and ammonia.

II. *New Expedition into Africa*.

We understand that the British Government are at present projecting a new expedition to explore the interior of Africa. They are to employ in this expedition the black regiment which has been formed on the west coast of Africa; and which, being inured to the climate, will constitute a better and more efficient guard to the travellers, than the same number of British troops. It is at present most probable opinion, that the Niger, after running east for a considerable way, takes a southerly, and at last a westerly direction,

and runs into the Atlantic on the coast of Guinea. It will be very singular if this opinion be confirmed. It would be of great importance if the travellers would carry with them a set of portable barometers, in order to determine the elevation of the country through which they pass. If we knew the elevation of the banks of the Niger, it would be easy to determine whether the opinion of the ancients, that it is a branch of the Nile, be possible or not. Nothing in Mungo Parke's travels would lead us to suppose that the elevation of the banks of this river is great. In that case it must either flow into an inland sea, or discharge itself into the Atlantic.

### III. *Communication between the Blind and the Deaf and Dumb.*

Dr. Guillié, Director of the Royal Institution for the Blind, at Paris, by a method of his own invention, has been enabled to establish an immediate and perfect mode of intercourse between blind and deaf and dumb persons. These two species of unfortunate individuals, between whom nature appeared to have placed insuperable barriers, may, thanks to the ingenious author of this invaluable discovery, henceforward draw near and perfectly understand each other.

The first trial of this ingenious practice was made at a numerous and public meeting, held at Paris, the 26th of last August. A sentence was dictated to one of the deaf and dumb, named Masuen, a pupil of the Abbé Sicard, and by him communicated to one of the blind, who immediately repeated it in a loud voice; and he in his turn communicated to the deaf and dumb the sentence asked by the public, who instantly wrote it on a tablet.

We are assured that Dr. Guillié intends coming to London in the winter, to make an experiment of his practice before the Royal Society.

### IV. *Swinestone at Building Hill, Durham.*

I am informed, by Mr. Sowerby, that some of the Building Hill stone retains its fætid odour for years when scraped, while other specimens lose it very soon. This shows us that I was not mistaken when I called it Swinestone, on the spot. But the reason why some specimens soon lose their odour remains still to be given.

### V. *On the Solar Spectrum.*

(To Dr. Thomson.)

SIR,

Since my last communication it has occurred to me, that if Dr. Herschel's position respecting the heating rays attendant on the prismatic spectrum stands in need of any confirmation, the well-conducted experiments of Mr. Leslie on radiant caloric substantiate them in a most remarkable manner. Mr. L. distinctly affirms that he found the greatest intensity, or, as I should call it, the *focus of heat*, by his reflectors, half an inch within, or nearer to the mirror than, the *optical focus*. As far as I can judge too, the

the numbers stated by Mr. L., the progress of the differential thermometer from and towards the mirror, indicates the very ratio that the prismatic spectrum displays with regard to intensity of light. What then must be our conclusion, but that the reflector in this case performs an office similar to that of the prism? \*

Now, Sir, may we not take advantage of this instrument to obtain a better knowledge of those rays termed deoxidating? In conformity to my theory *another focus* remains to be discovered, where muriate of silver would be found to blacken most readily; for I contend, that those rays are mere modifications of heat, or, to speak more fully, that they are rays of *latent* caloric, or the true phlogiston; while those which affect the thermometer are obvious or sensible caloric. With the greatest respect,

I am, Sir, Yours, &c.

DAVID HUSTON.

#### VI. Position of Port Jackson, New Holland.

From the observations of Captain Flinders, it appears that Cattle Point, on the east side of Sidney Cove, is in latitude  $33^{\circ} 51' 45.6''$ , south; longitude,  $151^{\circ} 11' 49''$ , east,—*Flinders's Voyage*, I. 237.

#### VII. Chinese Paste.

The method of making paste in China is much more economical than the mode followed in this country. Were it universally adopted by trunk-makers, book-binders, and others, who use great quantities of paste, it would produce a very material saving of flour, which in years of scarcity might be of the greatest consequence. The following formula used in China was lately communicated to the Right Hon. Sir Joseph Banks, Bart., by a gentleman at present in Canton.—Mix together bullock's blood and quick-lime, in the proportion of one pound of the latter to 10lbs. of the former. It becomes a stiff jelly, in which state it is sold to the consumers, who beat it down with an addition of water into a state sufficiently fluid for use. At Canton it will keep five or six days in the hot weather and ten or twenty days in the cold. In Britain it would probably keep longer.

#### VIII. Chinese Method of casting Sheet Lead.

The Chinese method of casting sheet lead is very simple. Two large flat tiles are used for the purpose, covered on the inside with thick paper. The workman opening them a little at top, pours in a small quantity of melted lead, and pressing the tiles together with his foot, forms the sheet. A kind of resin called *dummer* is used to prevent the oxydizement of the metal. The tea canisters

\* As sound (a) is reflected as well as light and heat by a concave mirror, I have little doubt but that their analogy will be one day perfectly demonstrated.

(a) Query. Is the focus of sound at the same distance from the reflector as the focus of light?

which have a crystallized appearance, and which at Canton are called *cho yeip*, or *bamboo leaf*, from their resemblance to that leaf, are formed from tin in the same way. They are not made in the neighbourhood of Canton.

#### IX. Häüy's Crystallography.

It gives us great pleasure to state, that Mr. Larkins, N° 14, Gee Street, Somer's Town, has commenced teaching the mathematical part of Häüy's theory of crystallization. It is scarcely necessary to observe, that without a correct knowledge of the mathematical principles from which this most beautiful theory has been deduced, very little progress can be made in the study of crystals. Mr. Larkins has likewise cut in wood very beautiful models of all the crystals described by Häüy in his System of Mineralogy. Complete sets of these models are sold at the rate of from thirteen pounds to fifteen pounds sterling, according to the kind of wood from which they are cut.

#### X. Table of Temperatures.

M. Guyton Morveau has been employed for many years in constructing a pyrometer of platinum to measure the higher degrees of heat. He was induced during his experiments to compare it with the mercurial thermometer, and with Wedgewood's pyrometer. The following table\* exhibits the result of his experiments.

	Degrees of Wedgewood.	Degrees of Fahrenheit.
Mercury melts .....		-39°
Water freezes .....		+32
Water boils .....		212
Bismuth melts .....		475.99
Tin melts .....		512.48
Lead melts .....		611.98
Mercury boils .....	2°	642.75
Zinc melts .....	3	705.26
Antimony melts .....	7	955.23
Silver melts .....	22	1822.67
Copper melts .....	27	2205.15
Gold melts .....	32	2517.63
Welding point of iron .....	95	6508.88
Cast-iron melts .....	130	8696.24
Porcelain melts .....	155	9633.68
Manganese melts .....	160	10517.12
Macquer's furnace .....	165	10829.60
Fourneau a trois vents .....	170	11142.08
Malleable iron melts .....	175	11454.56
Nickel melts .....	175 + x	11454.56 + x
Platinum melts .....	175 + x	11454.56 + x

\* Annales de Chimie, xc. 236.

to remember right, Wedgewood's clay pieces were baked in a furnace. If this be the case, it is impossible that the boiling point of mercury, which is below the commencement of Wedgewood's scale, can be represented by  $2^{\circ}$  of that scale. There is reason to believe from this, that Morveau was not in possession of the true clay pieces of Wedgewood. If so, his determination of the value of Wedgewood's degrees cannot be considered as correct. The melting point of tin is certainly stated too high in the preceding table. Mr. Crichton of Glasgow, whose accuracy is uncommon, found that this metal melts at  $442^{\circ}$ , and that it melts at a lower temperature than bismuth.

XI. *Saccharometer.—Thermometer.—Starch-Sugar.—Passage of Air through Tubes.*

(To Dr. Thomson.)

SIR,

I beg to make some observations on the queries in your *Annals*, of September, respecting the use of the saccharometer in brewing, which I had delayed.

According to the common acceptation of the word *strength*, in beer or ale, the saccharometer foretells it sufficiently near the truth, for similar processes in the same brewhouse; but it is not a sure guide for any dissimilarity in the mode of brewing or of drawing the extract: for then the constituents of malt are *not* often dissolved in the same proportion to each other; and, in all cases, the extracts are superior in value according to their priority. For, let the first, from the same parcel of malt, be reduced to the specific gravity of the last, and equal quantities of each will give a beer or ale very different in quality.

The quantity of alcohol increases during fermentation, as the specific gravity of the fermenting fluid diminishes, but in what ratio, no correct experiments have as yet ascertained. The term, designated *strength*, in malt liquors, depends, however, on many inexplicable circumstances; I fancy, from the variety in quality in different beers, made from equal extracts in every respect. I do not mean to allude to the mixture of any deleterious drugs, because I really believe the common brewers, in general, *do not*, and those extensive establishments in London, *cannot*, make use of any substitute for malt and hops. The gentleman, who makes those queries, is perfectly correct in asserting, that the common saccharometers do not give the quantity of solid matter contained in a barrel of wort; they express merely the difference of weight between that and a barrel of water. The former, however, can be ascertained nearly by multiplying the latter by 2, 7, according to my trials, which I hope agree with the experiments of this gentleman.

It is rather a disgrace to the present state of chemistry, that the chemical *brotherhood* of Europe should still suffer the variety in the thermometric scale to exist; I therefore, as an humble individual, beg to suggest, that a resolution, published by the leading chemists

of England and France, to express temperature in future in none but *centigrade* degrees, would quickly become general, and form an universal language in this respect, to the expulsion of Fahrenheit's ridiculous scale, as well as the others, which occasion such unnecessary delay and difficulty in fixing the different degrees of heat in the memory, when running through chemical experiments in both the languages.

I may also perhaps be permitted to mention, to those who are making experiments on the conversion of starch into sugar, by Kirchhoff's method, that if the boiling be performed by passing steam into the fluid, all risk of empyreuma may be avoided, desiccation prevented, (as the volume will continue any length of time nearly the same,) and the operation can be performed in porcelain, earthen ware, or even wood vessels.

Mr. Wilkinson, the celebrated Iron-master, could not produce any current of air through cast-iron pipes, (extending about a mile in length,) even to the distance of 600 feet from the impelling force, which was very great; yet the gas-light companies find no difficulty in this respect.—May I request an explanation of this anomaly from any of your correspondents?

I. H.

## XII. Formation of Sal-Ammoniac in burning Bricks.

(To Dr. Thomson.)

SIR,

In your Sketch of the latest Improvements in the Physical Sciences, under the division Salts, article Sal-ammoniac, you observe, "that the sand employed in making bricks probably contains common salt, derived from the sea water with which it is washed; and that this may be the source of the muriatic acid, which goes to the formation of the sal-ammoniac that sublimes during the burning of the bricks; but that if this be the case, nature employs, during this process, some method of decomposing common salt, at present unknown to manufacturers, &c."

In reply to this observation, I beg leave to remark, that as sulphate of ammonia generally accompanies the volatile products of burning coals, it is not improbable that this salt decomposes the common salt that adheres to the sand, and gives rise to the production of the muriate of ammonia in question.

I shall be much obliged to you to inform me, of some practicable method of separating manganese when in solution from the solution of a neutral salt. A minute portion of this metal communicates a permanent stain to a considerable quantity of any neutral sulphate that may happen to contain it.

I remain, Sir, your most humble servant,

Bristol, Jan. 14, 1815.

E. T. I.

## ARTICLE XV.

*List of Patents.*

ROBERT SALMON, Woburn, Bedfordshire ; for improvements in the construction of machines for making hay. Aug. 22, 1814.

JOHN DICKINSON and GEORGE LONGMAN, Nash Mills, Hertfordshire ; for improvements in the said John Dickinson's patent machinery for manufacturing paper, and also a certain apparatus for separating the knots or lumps from paper or paper stuff. Aug. 24, 1814.

JAMES PENNY, of Low Nibthwaite, Lancaster, mechanic ; and JOSEPH KENDAL, same county, turner ; for a new and improved principle of making pill and other small boxes. Sept. 8, 1814.

WILLIAM LISTER, Paddington, Middlesex, Esq. ; for certain further improvements on an engine for separating corn or seeds from the straw and chaff. Sept. 21, 1814.

JAMES DOBBS, Birmingham, Gentleman ; for his improvements in the manufacturing of machines used for cutting and gathering in grain and produce arising from the earth, whereby much labour and expense are saved. Sept. 21, 1814.

JOSEPH TAYLOR and PETER TAYLOR, Manchester, machine-makers ; for having invented and brought to perfection certain improvements in a loom to be used in weaving cotton, linen, worsted, silk, or other cloth or clothes, made of any two or more of the said materials. Sept. 21, 1814.

W. E. SHEFFIELD, Somer's Town, Middlesex, Gentleman ; for divers improvements in the working or manufacturing of copper and its compounds, and other metallic substances, or any or either of the same. Sept. 21, 1814.

W. SAMPSON, Acorn-street, London, millwright ; for certain improvements for raising water. Oct. 3, 1814.

ABRAHAM SHAW, Leicester, glazier's vice and diamond manufacturer ; for his apparatus for the better cutting of window, plate, and sheet glass. Oct. 3, 1814.

AMBROISE FIRMIN DIDOT, Holborn, London, Gentleman ; for an improvement in the method of making types or characters to be used in the art of printing. Oct. 3, 1814.

R. PHILIPS, Newbury, Bucks, engineer ; for certain improvements in a plough. Oct. 5, 1814.

JAMES LONGHURST ; for an Æolian organ or barrel organ with a self-acting swell. Nov. 1, 1814.

JOHN WALTERS, Fenchurch-street, London ; for certain improvements in the construction and fastening of frame timber or binds of ships or vessels, whether building or under repair. Nov. 7, 1814.

WILLIAM HOWARD, Old Brentford, Middlesex ; for improved apparatus for working the pumps on board ships, which may also be applied to churning, and various other useful purposes. Nov. 10, 1814.

## ARTICLE XVI.

## METEOROLOGICAL TABLE.

1814.	Wind.	BAROMETER.			THERMOMETER.			Evap.	Rain.	
		Max.	Min.	Med.	Max.	Min.	Med.			
12th Mo.										
Dec. 4		29.50	29.34	29.420	44	36	40.0			C
5		29.94	29.50	29.720	41	33	37.0			
6		30.04	29.91	29.975	38	28	33.0			
7		30.04	29.60	29.820	45	33	39.0		—	
8	S	29.60	29.25	29.425	52	45	48.5		.44	
9	S W	29.70	29.25	29.475	52	25	33.5		6	
10		29.37	29.35	29.360	51	29	40.0	.11	.53	
11	W	29.63	29.37	29.500	54	48	51.0		.33	
12	S W	29.65	29.50	29.575	56	53	44.5		.17	
13		29.72	29.50	29.610	56	41	48.5		3	
14	S W	29.56	29.54	29.550	53	42	47.5		7	
15	S W	29.68	29.40	29.540	55	43	49.0		3	
16	S W	29.92	29.40	29.660	55	37	46.0		.10	
17	S W	29.78	29.68	29.730	55	44	49.5		5	
18	S W	29.82	29.67	29.745	56	51	53.5	.33	2	
19	S W	30.05	29.67	29.860	55	30	42.5		.11	O
20	N	30.08	29.98	30.030	38	28	33.0			
21	N E	29.98	29.73	29.855	39	31	35.0			
22	E	29.73	29.16	29.595	37	32	34.5			
23	E	29.69	29.67	29.680	38	32	35.0			
24	E	29.67	29.62	29.645	33	29	31.0		—	
25	N E	29.61	29.58	29.595	33	30	31.5	.20	—	
26	N E	29.62	29.45	29.535	35	32	33.5		.16	C
27	S E	29.45	28.94	29.195	41	35	38.0		—	
28	N E	29.70	28.94	29.320	40	33	36.5		1.14	
29	S E	29.78	29.70	29.740	43	34	38.5		2	
30	S	29.90	29.71	29.805	49	33	41.0		9	
31	E	30.18	29.90	30.040	44	31	37.5		—	
1st Mo.										
Jan. 1	N	30.43	30.18	30.305	42	30	36.0	4		
		30.43	28.94	29.665	56	25	40.13	0.68	3.35	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

## REMARKS.

*Twelfth Month.*—4, 5, 6. Slight hoar frosts in the ponds: windy at intervals. 7. Occasional small rain. 8. The max. temp. of the last 24 hours this morning, with a hollow S. W. wind. An unsteady rain followed, of many hours' continuance, with much wind at S. 9. Windy: the barometer appears to have stood through the night at the min. noted. 10. Hoar frost: *Cirrostratus*: overcast, a. m.: after one, the day wet and stormy: much wind in the night. 11. Cloudy: rain before nine, and at intervals through the day. Though it was very cloudy, the bees came out in great numbers, as observed about the same time last year. 12. Temp. this morning 54°, with low driving clouds, and a gale at S. W. 13. Early this morning the wind was violent: it moderated about three p. m.: and the clouds, after a squall, assumed the *Cumulostratus*. Much water out in the marshes. 14. Red sun-rise: *Cirri* over the sky, with their extremities drawn out westward: p. m. it blew hard again from S. W. followed by rain at night. 15. a. m. Much wind and cloud. The millers, who have remarked that for two years past our atmosphere has been calmer than usual, will now probably complain of an excess of wind. The *Cumulostratus* appeared after three p. m. and the evening and night were calm. 16. Early this morning the wind rose again, and blew with great violence, with rain at intervals, till about three p. m., when the same change ensued as yesterday. 17. Overcast: windy: some rain p. m.: a gale through the night. 18. The tempestuous weather continues. 19. A shower about eight a. m., and again in the evening, after which light clouds were observed to pass the moon's disk with less and less velocity. 20. Hoar frost: a fine and nearly calm day: swarms of gnats in the air, and *gossamer* floating. The dew froze on the grass by half past four p. m.: there was a corona round the moon, and a *Stratus*, at night. 21. Hoar frost. 22. Dry air: steady breeze: *Cirrus*, passing to *Cirrostratus*. A lunar halo of large diameter. 23, 24. Cloudy: steady breeze. 25. Some snow in the night, followed by a little more in the day. 26. a. m. Snow more plentiful: a thaw. 27. Rain: sleet and snow: rain again. 28. The max. temp. early this morning: much rain. 29. Overcast sky. 30. Misty. 31. Hoar frost: large *Cirrus* clouds: a few drops of rain: p. m. *Nimbus*.

*First Month, 1815.*—1. Hoar frost: a rose-coloured sky, with *Cirrostratus* at sun-set.

## RESULTS.

Winds in the fore part Westerly and violent: in the latter, Easterly and moderate: but in both attendant with rain: but one clear day in the period.

Barometer: Greatest height.....30.43 inches;  
Least.....28.94 inches;  
Mean of the period.....29.665 inches.

Thermometer: Greatest height.....50°  
Least.....25°  
Mean of the period.....40.13°

Evaporation, 0.68 inch. Rain, 3.35 inch.

TOTTENHAM, *First Month, 25, 1815.*

L. HOWARD.

# ANNALS OF PHILOSOPHY.

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MARCH, 1815.

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## ARTICLE I.

*A Biographical Account of David Rittenhouse,\* LL. D. F. R. S.  
late President of the American Philosophical Society.  
By Thomas Thomson, M. D. F. R. S.*

**THE** Rittenhouse family from which our philosopher was descended emigrated from the province of Guelderland to New York, towards the end of the 17th century, when that state was in the possession of the Dutch. They soon removed to Germantown, in Pennsylvania, where they established the first paper-mill ever erected in America. Matthias, the youngest son of Nicholas Rittenhouse, our philosopher's grandfather, was born at Germantown, about eight miles from Philadelphia, in 1703. Having abandoned the occupation of a paper-maker when about 29 years of age, and two years after his father's death, he settled at Norriton, a farm which he had purchased of about 150 acres, and situated about 20 miles from Philadelphia. In the year 1727 he had married Elizabeth Williams, the daughter of Evan Williams, a native of Wales. This woman had been left an orphan, and was brought up by an old Gentleman in the neighbourhood called Richard Jones. She possessed a cheerful temper, and a mind uncommonly vigorous and comprehensive.†

\* The veneration in which this Gentleman was held in America, and the very high opinion entertained of his genius and scientific attainments, induce me to suppose that a short account of him will be acceptable to my readers. My information is derived from a biographical account of him published in Philadelphia in 1813, by his nephew, Mr. William Barton. I perused this book with much pleasure. The views which it gives of politics, philosophy, and religion, are excellent, and such as I scarcely expected from an American philosopher of the present day.

† Mr. Barton conceives that the abilities of David Rittenhouse were derived

By this wife Matthias Rittenhouse had four sons and six daughters. David, the eldest son, and the subject of this article, was born on the 8th of April, 1732. He was an infant when the family removed to Norriton. He was destined by his father for a farmer, and at the age of 14 was actually employed in ploughing his father's fields.

During this period, which continued for about five years, he appears to have been occupied chiefly in the study of mathematics, and in the pursuit of mechanical inventions. We do not know what books he possessed; but his brother Benjamin, who was often sent to call him in to dinner, often observed the fences at the ends of the furrows, and the plough handles themselves, chalked over with numerical calculations. In his 17th year he made a wooden clock of very ingenious workmanship; and soon afterwards he constructed one of the same materials that compose the common clock, and upon the same principles. It is obvious from this that he must have possessed the means of making himself acquainted with the principles of clock-making from books, since he had no other instructor; and possibly there might have been a clock in his father's house, which he would have it in his power to examine. Be this as it may, his success in constructing all the parts of the clock himself, and putting them together without any previous instruction, displayed an uncommon mechanical turn, which probably would not have appeared had he been placed in more favourable circumstances. He requested his father to allow him to devote the whole of his time to mechanics, and to set up the trade of a clock and mathematical instrument-maker. His father, after a good deal of hesitation, at last gave his consent. He erected in consequence a workshop on the side of a public road, and in the township of Norriton, after having made many implements of the trade with his own hands to supply the deficiency of such as were wanting in his purchased stock.

From the age of 19 to 25 Mr. Rittenhouse devoted the whole of his time to his trade and to his studies. The days were occupied with the former, and much of the nights with the latter. He rose very early in the morning, and did not go to bed till midnight, or even later. This intense application impaired his health, which had before been good. He was seized with a constant heat in the pit of the stomach, affecting a space not exceeding the size of half-a-guinea, attended at times with much pain. This malady never left him during the rest of his life.

In 1751, when Mr. Rittenhouse was 19 years of age, Mr. Thomas Barton, an Irish Gentleman of English extraction, opened a school in the neighbourhood of Mr. Matthias Rittenhouse. This

not from his father, but from this woman; and he is at some pains to show that abilities are often hereditary. For my own part I never knew a man of abilities whose mother had not been an uncommon woman. If we consider that the early part of education, the most important of all, depends almost solely on the mother, we need not be greatly surprised at this.

Gentleman had received a regular university education, and possessed considerable knowledge and abilities. He was two years older than Mr. Rittenhouse, and two years afterwards became his brother-in-law by marrying his sister. An intimate friendship took place between these two young men; and there can be no doubt that Mr. Barton must have been of considerable use in forwarding the education of Mr. Rittenhouse, both by supplying him with books, and by instructing him in several branches of knowledge, to which previously he could not have the means of paying any attention; but we neither know the course of study that Mr. Rittenhouse pursued, nor the books which he used. He appears early to have acquired a very considerable knowledge of mathematics and astronomy, and to have perused Newton's *Principia* partly in the English translation and partly in the original. Mr. Barton says that he discovered the method of fluxions without knowing that it had been previously discovered by Newton; but such ignorance in the year 1751, more than half a century after the discovery was publicly known in Europe, seems hardly conceivable. Whatever mathematical books he studied must have alluded to a method at that time followed by every mathematician; or supposing, which seems hardly credible, that all the books in his possession had been printed before the discovery of fluxions, still Mr. Barton, who had been educated at Trinity College, Dublin, could not possibly be ignorant of the discovery of fluxions. Indeed, in the second edition of the *Principia*, there is a kind of discussion on the subject by Newton himself. Mr. Barton then, I conceive, must have been misinformed upon this subject.

Astronomy was Mr. Rittenhouse's darling pursuit; and we have the strongest proofs that his knowledge of this science was profound, and his skill as a practical astronomer very great. He acquired considerable dexterity in making astronomical instruments, especially telescopes and clocks. The principles by means of which the pendulums of these clocks were kept always at the same length are curious, and deserve the attention of astronomers and mathematical instrument-makers.

But though neither the books which he perused, nor the order of his studies, be known, yet we are acquainted with the particular circumstance which first turned his attention to mechanics and mathematics. He had a maternal uncle, David Williams, a carpenter, who had a mathematical turn: this man died when David Rittenhouse was a boy, and his tools and books, consisting of some treatises on geometry and some manuscript calculations, fell into the possession of his mother; to these books, &c. David had free access, and they were probably the only books of any value within his reach. Hence we need not be surprised that a young man of a vigorous understanding soon acquired a taste for mathematics, and learned to handle the tools of his uncle.

The great mathematical and astronomical skill of Mr. Rittenhouse was soon discovered by Mr. Barton, who had taken orders,

and was settled as a clergyman in an episcopal congregation in Philadelphia. He communicated the fact to Dr. Smith, Provost of the College of Philadelphia: a friendship soon took place between them, and Dr. Smith was at great pains to draw him into public notice.

In 1763 Mr. Rittenhouse was employed by Mr. Peters, Provincial Secretary to the Governor of Pennsylvania, to determine part of the limits between Maryland and Pennsylvania, which had long been a subject of dispute between Lord Baltimore and Mr. Penn, the proprietors of the respective provinces. This laborious task he performed to the satisfaction of the parties concerned.

In 1766 Mr. Rittenhouse married Eleanor Colston, daughter of Bernard Colston, a reputable farmer in the neighbourhood. She belonged to the society of Quakers. Mr. Rittenhouse had been brought up an Anabaptist; but he never declared himself a member of any particular church. The marriage was solemnized at Norriton by his brother-in-law, Mr. Barton. His father had previously resigned to him his house and farm of Norriton, having removed with his family to a house that he had built on his place in Worcester township. At Norriton Mr. Rittenhouse remained for four years; during which period his reputation as a mechanic and an astronomer was very much increased by two circumstances, which deserve to be particularly mentioned. The first was the construction of an orrery, much more complete than any thing of the kind that had been previously contrived. It exhibited the true motions of the different planets and satellites with the greatest exactness, so as to show their place in the heavens, and their relative position with respect to each other without any sensible error for thousands of years to come. The relative position of these bodies for any particular time could be determined by turning a winch till an index on a dial plate pointed out the time wanted. Before he attempted the construction of this expensive machine Mr. Barton, his brother-in-law, bound himself to reimburse him for the expense, provided he could not get it sold; but as soon as the machine was completed, there was a competition for it between the College of Princeton, New Jersey, and the College of Philadelphia. The orrery was disposed of to the College of Princeton for 300*l*.; and he made another within the year for the College of Philadelphia.

The other circumstance was of still greater importance: it was the observation of the transit of Venus in 1769. This transit, being of rare occurrence, and being the best means of determining the parallax of the sun, was eagerly expected by astronomers, and had attracted the particular attention of almost all the crowned heads in Europe. Philadelphia being a proper station for observing this phenomenon, the opportunity was laid hold of by the Philosophical Society of that city, and three committees were appointed to make the observation in three different places: one of these was at the observatory constructed by Mr. Rittenhouse at Norriton. The committee appointed to make the observation in this place were

Mr. Rittenhouse, Dr. Smith, Mr. Lukens, and Mr. Sellers. Every thing was properly prepared by Mr. Rittenhouse. The sun rose and set on the day of the transit without a cloud. The observation was made in the most complete manner. An account of it was published in the first volume of the Transactions of the American Society. It was allowed by astronomers to be very complete, and served to raise the reputation of Mr. Rittenhouse in Europe. He was henceforth considered as a consummate astronomer.

On the 17th of November, 1767, the College of Philadelphia bestowed upon Mr. Rittenhouse the honorary degree of Master of Arts. In 1770 he took up his residence in Philadelphia. About the same time he lost his wife, who left him two daughters, both at that time infants. In 1771 he was elected one of the Secretaries to the Philosophical Society. About this time the Assembly of Pennsylvania voted him 300*l*. chiefly on account of the ingenuity which he had displayed in the construction of the orrery.

It was at this time that the disputes between the British Government and the American colonies, which terminated in the revolutionary war, began. No further attention could be paid to science or literature; politics alone filled the minds of every one. Mr. Barton assures us that at the commencement of the dispute the whole American population was loyal, and that it was with the utmost reluctance they were brought to a declaration of independence; but I cannot, for my part, adopt this opinion. If we consider the great number of emigrants that went to America during the troubles in Charles the First's reign; that these emigrants were all puritans who possessed the most lofty ideas of liberty and independence, and were in fact republicans in their hearts; we cannot avoid concluding that these sentiments, for which they had left their native country, and suffered all the hardships to which they were exposed in the New World, would be cherished by them with particular veneration, would be inculcated upon their children, and would become the distinguishing characteristic of the colony. Hence I conceive that republican principles were very generally entertained in America, especially in New England, where the dispute first began. The colonists were of opinion that the whole expense of defending them should fall upon the mother country, and that the British Parliament had no right to lay taxes on them till they put them in every respect upon a footing with the inhabitants of Great Britain. In this opinion I think they were right. It was fortunate for Great Britain that the dispute terminated as it did; perhaps it was equally fortunate for America, though a sufficient period has not yet elapsed to enable us to judge correctly.

Mr. Rittenhouse took the side of the colonists with great keenness; while his brother-in-law, Mr. Barton, embraced that of the mother country. This produced a separation. Mr. Barton went to New York, where he died in 1780. He left all his family behind him in Philadelphia. In 1776 Mr. Rittenhouse was chosen State Treasurer of Pennsylvania, a situation which he filled for 13 years.

The profits were so small as not to enable him to keep a clerk, and the finances of the country so embarrassed as to render the office far from agreeable. In 1777 the British army occupied Philadelphia. The State Legislature went to Lancaster, and Mr. Rittenhouse accompanied them, leaving his wife and family (for he had married again) at Norriton. A Council of Safety, consisting of twelve persons, of whom Mr. Rittenhouse was one, was appointed with despotic powers; but it does not appear that they ever exercised them. When the British army evacuated Philadelphia, in 1778, Mr. Rittenhouse returned to that city, and continued to reside in it during the rest of his life.

In 1779 he was appointed one of the commissioners to settle the boundary between Pennsylvania and Virginia, which had been disputed for so long a period with so much eagerness. The business was finally settled in 1784, Mr. Rittenhouse and his associates having marked out the whole boundary line with the greatest accuracy. On the 10th of March, 1780, Mr. Rittenhouse was elected, by the General Assembly of Philadelphia, a Trustee of the Loan Office of the State. In 1782 he was chosen a Fellow of the Academy of Arts and Sciences of Boston. In 1786 he was appointed a commissioner to determine the exact boundary between the State of Massachusetts and New York. In 1789 the degree of Doctor of Laws was conferred upon him by the College of New Jersey. The same year he resigned the office of Treasurer of the State, which he had held for 13 years, having been unanimously elected to it every year by the vote of the Assembly. In 1790 he was elected Vice President of the American Philosophical Society; and the next year, in consequence of the death of Dr. Franklin, he was chosen President of that learned Body, a situation which he held till his death.

He was employed by Government, on various occasions, in the construction of roads, and in rendering rivers navigable, or in improving their navigation; and in 1792 he received the very important appointment of Director of the American Mint, a situation which he held till 1795, when he resigned it.

The French Revolution had been going on for some years, and it is not surprising that it was generally approved of by the inhabitants of America: so strong were their prejudices, indeed, that even the destruction of the King of France, and of the whole family, the bloody despotism of Robespierre, and the fanatical madness of the pretended philosophers who abolished by law the Christian religion, the belief in the existence of God, and of a future state, did not open their eyes, nor make them sensible of the desperate evils to Europe, morality, and civilization, with which that ill-starred revolution was pregnant. A democratic society was established at Philadelphia in 1793 in imitation of the Jacobin Club of Paris, and Mr. Rittenhouse was elected the President. The bad tendency of this society was soon evident; and it became the source of many political evils, particularly of an insurrection in the west

of Pennsylvania against the excise laws. Mr. Barton has endeavoured to vindicate Mr. Rittenhouse for his connection with this Society; but his vindication is far from satisfactory. He did not attend their meetings; but as he retained the place of President, this must be considered as a direct avowal that he approved of their proceedings; for had he disapproved of them, it is quite obvious that he was bound as an honest man to throw up the office. Now as Mr. Barton does not say that any resignation took place, it is pretty clear that he retained his situation to the last. How far his being President of the democratic society of Philadelphia can be considered as approving of the enormities committed in France, and the desperate spirit of immorality displayed by the successive demagogues of that unhappy country, I cannot say; because I do not know the peculiar creed which was maintained and propagated by the democratic society of Philadelphia. That a republican should view the French Revolution with a favourable eye, in spite of the enormities which disgraced it, I can easily conceive; but that a philosopher, and a disciple of Newton, should view with complacency the unblushing propagation of atheism and immorality, is what I cannot bring myself to believe; nor that a Christian, and a man of virtue, like Mr. Rittenhouse, should rejoice at the destruction of the Christian religion, and all the institutions for education in one of the finest countries of Europe.

Dr. Rittenhouse was elected a foreign member of the Royal Society on the 16th of April, 1795; but he enjoyed this honour only a short time. His constitution was worn out, though he had not attained any extraordinary age; and he died on the 26th of June, aged 64 years. The account of his death by his nephew, Dr. Smith Barton, who attended him in his last illness, is so simple and interesting that I cannot avoid laying it before my readers:—

“The last visit I ever received from Mr. Rittenhouse was about the middle of June, 1796. He called at my humble habitation in Fifth-street, to inquire about my health, and to learn from me the result of the experiments and inquiries in which he knew I was at that time engaged, concerning the mode of generation and gestation of our opossum, an animal to whose economy and manners he had himself paid some attention, and whose history he justly considered as one of the most interesting in the whole range of zoology.

“It was on this occasion that our excellent friend first informed me that he had received a diploma from the Royal Society. He observed, with a tone of voice, and with a certain expression of countenance, which were not calculated to afford me any pleasure, that a few years ago such a mark of respect from that illustrious body would have been received by him with pleasure and with pride.

“In fact, Mr. Rittenhouse, now and for some months past, was strongly impressed with the idea that his career of usefulness and virtue was nearly at an end. He had several times during the pre-

ceding part of the spring and summer, intimated to me (and doubtless to others of his friends) his impressions on this head. In what precise condition of his system, whether physical or intellectual, these impressions were founded, I have only been able to form a distant and unsatisfactory conjecture.

" A few days after this interview, viz. on the 22d of June, I was sent for to visit Mr. Rittenhouse. I found him in his garden, where he loved to walk, and soon learned that he laboured under a severe attack of cholera, accompanied, however, with more fever than we generally find with this disease; and with a great increase of that violent pain and sense of oppression at the region of his stomach, to which he had been subject for at least thirty years. Notwithstanding his age, the debility of his system, and the unfavourable state of the season, I ventured to flatter myself that the attack would not prove mortal. On the following day, however, finding him no better, but rather worse, I requested permission to call in the aid of another physician; and having mentioned the name of Dr. Adam Kuhn, that Gentleman accordingly visited our friend in company with me during the remainder of his illness.

" His febrile symptoms being very urgent, it was thought necessary to bleed our patient; and notwithstanding his great and habitual repugnance to the practice on former occasions, he now readily consented to the operation, on condition that I should perform it myself. The blood which was drawn exhibited a pretty strong inflammatory crust, and the operation seemed to give him a temporary relief from his pain. Soon after this his strength gradually declined; and on the third day of his illness it was but too obvious that our illustrious relative was soon to be separated from his friends. He expired without a struggle, and in the calmest manner, ten minutes before two o'clock on the morning of Sunday the 26th, in the presence of his youngest daughter, Mrs. Waters, and myself. His excellent wife, who had ever been assiduous in her attentions on her husband, both in sickness and in health, had retired from his chamber about two hours before, unable to support the awful scene of expiring genius and virtue.

" There can be no doubt, I think, that Mr. Rittenhouse, from the first invasion of his disease, or at least from the day when he was confined to his bed or room, entertained but little hopes of his recovery. He signed his will in my presence. He discovered no more solicitude about his situation than it is decorous and proper in every good or great man to feel when in a similar situation. During the greater part of his illness he manifested the most happy temperament of mind: and it was only in the last hour or two of his life that his powerful intellects were disturbed by a mild delirium. About eight hours before he died, the pain in the region of his stomach being unusually severe, a poultice composed of meal and laudanum was applied to the part. In less than two hours after the application I called to see him, and upon asking him if he did not feel easier, he calmly answered in these memorable words, which it

is impossible for me to forget, for they were the last he ever distinctly uttered, and they make us acquainted with the two most important features of his religious creed,—‘ Yes, you have made the way to God easier.’

“ Such were the dying words, as it were, of our illustrious relative and friend. He was dear to us both, to all his relatives and friends, and to his country. To me, let me add, he was *peculiarly* dear. The most happy and profitable hours of my life were passed in the society of this virtuous man. I followed his footsteps in the wilderness of our country, where he was the first to carry the telescope, and to mark the motions and positions of the planets. In the bosom of his family I listened to his lessons, as an humble disciple of Socrates or Plato. Science mixed with virtue was ever inculcated from his lips; but to me Mr. Rittenhouse was more than a friend and preceptor. He was a father and supporter. He laid the foundation of what little prosperity in life I now, or may in future, enjoy: and if it shall ever be my fortune, either by my labours or my zeal to advance the progress of science, or to reflect any honour upon my country, I should be the most ungrateful of men if I did not acknowledge and wish it to be known that it was DAVID RITTENHOUSE who enabled me to be useful.”

That the character and dispositions of Dr. Rittenhouse must have been excellent, is obvious from the high veneration in which he was held by all his countrymen, and by the annual and unanimous vote of the General Assembly of Pennsylvania in his favour for 13 years, at a period when they were divided into two most furious factions. That his honour and integrity were pure and unsullied we require no further proof than this: he led in Philadelphia a retired and sober life, never launching out into any extravagance, and indeed holding luxury in the utmost abhorrence; yet all the property which he ever acquired cost him no more than 13,525*l.*; though he held for many years public offices of the most important kind, some of them lucrative; and one, Treasurer to the State, in a time of public danger and distress, in which a man destitute of principles might have realized an immense fortune. Mr. Barton has brought the most satisfactory evidence that he was a sincere believer in the Christian religion, that he had studied its evidences, and satisfied himself of their force, though he seems to have reckoned as of no consequence the peculiar dogmas which distinguish the different Protestant sects which swarmed in America. His liberality and benevolence were in all cases conspicuous. He advanced a considerable sum of money to enable the American Philosophical Society to discharge a debt which they had contracted. In 1793, when the yellow fever raged in Philadelphia, he employed Dr. Barton to attend several poor families in his neighbourhood who had contracted the disease. He supported, for some time, an Italian statuary, who had come to Philadelphia in quest of employment, but had been unsuccessful. Finally, he was, during the whole of his life, a violent and declared enemy to the slave-trade.

As to his works, he has left but few behind him. Nor shall we be surprised at this, if we consider the country in which he spent his life, the difficulties with which he had to struggle in the early part of his career, and the arduous contest which, during a period of 12 years, drew the whole attention of the Americans to politics, and which left behind it such a deterioration of moral character, such a state of agitation and discontent, as must long prevent any great advancement of the sciences in that boisterous and unsettled republic. Mr. Rittenhouse left behind him 22 papers, all printed in the four volumes of the American Philosophical Society's Transactions, which have been already published. These papers are chiefly astronomical; and the most important of them all is the account of the transit of Venus in 1769, printed in the first volume, and drawn up by Dr. Smith.

## ARTICLE II.

*Experiments for determining the Draught of Carriages, with and without Springs. Extracted from an Essay on the Construction of Roads and Carriages. By R. L. Edgeworth, Esq. F. R. S. M. R. I. A. and Civil Engineer.*

THEORY shows that whilst the wheels of a carriage pass over an obstacle the load on the carriage must rise along with the wheels, unless it is supported by springs; but that if the load is hung upon springs, whilst the carriage-wheels tend to throw the load upwards, as they rise suddenly over an obstacle, the springs will bend, because they are opposed, not only by the weight, but by the *vis inertiae* of the load acting downwards; and the load will consequently not be thrown up *suddenly* so high, as if there were no springs; for the weight cannot be thrown upwards instantly; it requires a certain time to throw any weight upwards through a given space, and it is well known that in all cases this time must be equal to that in which a body would fall through the same space. So that making allowance for the imperfection of springs, it is easy to calculate their utility in lessening the draught of a carriage over an obstacle, if the height of the obstacle, the velocity of the carriage, the height of its wheels, and of the load which rests on springs, be known.

Upon subjects of this sort, which are of such universal concern, the best possible proofs should be given of the truth of whatever is proposed for general adoption, particularly where popular prejudice is directly in opposition to what is advanced. Let the accuracy of models, and their resemblance to *reality*, be ever so great, the *mind still requires* what is real. The ploughman will not be convinced by experiments made on the models of ploughs, nor the

waggoner or coachman by experiments on the *models* of coaches or waggons. It has been hitherto, therefore, a desideratum in mechanics to discover means of ascertaining precisely the power exerted by animals in drawing instruments of agriculture, as well as what is requisite to draw wheel carriages.

A machine for this purpose was presented to the Society for the Encouragement of Arts and Manufactures in 1771. It consisted of levers acting upon a spiral spring, in such a manner as to move an index that went through a considerable space. But as the power of the horse, and the resistance of the machine, varies continually; and as this machine showed only the *greatest* exertion of the power, without giving the sum of all its efforts, it was not satisfactory. It is possible to construct a machine upon this principle that will give the amount of the time and force employed in trials of this sort; but more simple and satisfactory means have been discovered.

In page 50 of the first Report for May, 1808, of the Committee of the Highways, I have spoken of a perfect method of ascertaining the comparative force necessary to draw any two carriages. It appears to me to be my *duty* \* to describe this contrivance in the present paper.

To elucidate the principle of this apparatus, two post-chaises were selected, the weights of which were nearly the same, their wheels were nearly of the same size, and they were alike in their general construction. To determine which of these could be drawn over the same obstacles with the greatest ease, one end of a rope 60 yards long was fastened to the splinter-bar of one of the chaises, and the other end of it was fastened to the splinter-bar of the other carriage. This rope had been previously made to pass round a light pulley six feet six inches diameter, which was placed horizontally, by means of a proper frame, on a two-wheeled carriage.

This pulley was sufficiently large to permit the carriages which were to be compared to run in a parallel direction at a sufficient distance from each other.

The foremost carriage was drawn forward by horses, and the two chaises were obliged to follow it. As the chaises were nearly of the same weight, they kept for some time together; but when either of them was retarded, it fell behind the other. An inconsiderable difference in the goodness of the parts of the road on which either carriage ran became sensible by the retardation of that carriage which ran upon the worst road.

If the machine carrying the pulley be drawn forward, the two other carriages must follow it; and if they are of equal weight, and equally well constructed, they must move on together, as they are drawn by the carriage with the pulley, provided the part of the road on which each of them moves be equally smooth and good; but if either of the carriages that are to be compared is inferior in

\* This essay was composed at the desire of the Chairman of the Committee of the House of Commons.

construction to the other, it will not keep pace with that of a better construction, but it will in a short time be left behind, till at last the better carriage will overtake the machine which carries the pulley. Now if the carriages are replaced in their former situation, that which appeared to be the best may have additional loading put upon it by degrees till it nearly keeps pace with the inferior carriage; so that after a sufficient number of trials, the advantage of one over the other may be determined by weight.

This is in fact weighing the draught of one carriage against the draught of another; and thus, taking any one carriage as a standard, the comparative advantage of any number of carriages, ploughs, or implements to be drawn by horses, may be accurately ascertained.

By this apparatus a cart with wooden springs was tried against a common cart: both were in the first place weighed, and the lightest had as much loading put into it as made it equal in weight to the other. They were then fastened to the ends of a rope which went round a pulley which was mounted, as above described, on another carriage. After a number of trials, on a road which was neither very good nor very bad, it was fairly ascertained that the cart on wooden springs surpassed a common cart of nearly the same form, and with wheels of nearly the same size; and that it carried more than one-fifth greater loading than the common cart; that is to say, the spring cart, loaded with seven men and a boy, kept pace with a common cart loaded with six men, whose weights compared were in the proportion above-mentioned.

To prevent any mistake that might arise from the difference of roughness in the different paths on which the carts ran, they were placed alternately at each end of the road, without any material difference being observed.\*

Distance from workmen skilled in working steel springs prevented a comparison being made between them and wooden springs.

Having thus stated the result of an inquiry, which appears to me of extensive utility, I beg leave to point out in general,

That if one horse out of five which are now employed in drawing heavy burdens could be spared, the saving to the nation would be (according to the calculations of Mr. Ward, in his excellent paper printed in the Third Report for 1809, of the House of Commons' Committee upon Broad-Wheels and Roads) nearly three millions sterling.

That as the advantage of springs must be inconsiderable upon smooth, soft, and sandy roads, their utility may perhaps be confined to one-tenth part of the roads in England, still they may save nearly 300,000*l.* annually to the nation, with all the advantages arising from the greater supply of human food, which must arise from the saving of land now appropriated to the maintaining of horses. Sup-

\* A similar experiment to what is above described was tried on a gravel walk with models, and nearly the same results were produced.

posing the saving to be only one-half or one-third of this sum, surely it is a great consideration in political economy.

It may also be observed, that the wear and tear of carriages on rough roads will be considerably diminished by the use of springs :

That the carriages with springs may be made much lighter than those without them, and that the weight so saved may be part of the loading of the carriage, instead of being uselessly a part of the carriage itself :

That by such carriages the roads will be less injured than they are at present, and that the thill-horse, though drawing heavier loads, will not be liable to the violent succussions to which it is now exposed in bad roads.

If this paper should in any degree obtain the attention of the Board of Agriculture, it may perhaps conduce to promote the adoption of a scheme which appears to me of the utmost national importance. I allude to a proposal, which I have elsewhere made, of carrying on publicly and daily for some months in the vicinity of the metropolis a system of large experiments with real carriages on a real road.

For this purpose a piece of a common near a great road should be fenced off, and there a quarter of a mile of road should be constructed on the best principles. And on this road, during two months at least, carriages upon the most approved construction should constantly be employed. Their number should be such that the traffic on the experimental road should equal that which is carried on upon the common road with which it is compared.

Beside this, part of the enclosure should be appropriated to experiments upon large carriages of different construction, with and without springs, in the manner above described.

These last-mentioned experiments might be repeated once a week for the period above-mentioned, so that the philosopher, the legislator, the farmer, the manufacturer, the coach-maker, the wheel-wright, the coachman, and the carter, might satisfy himself by the conviction of his own senses, observation, and understanding, of the true practical result of all that has been said, written, and tried, upon the subject of wheel carriages.

Such a large and unequivocal exposition of the truth would put an end to many a vain and interested project. It would be the result of real patriotism—of that genuine English patriotism which generously promotes what is essential to the economical interests of the state. Here nothing is promised but what every man in England must wish to have accomplished ; and a scheme is proposed which no adventurer can turn to private advantage.

176      *On the Definite Proportions in which the* [MARCH,  
 acquired a slightly reddish tinge. The 97 parts of residue analyzed  
 by combustion left 91·3 per cent. of oxide of lead. Hence the  
 salt is composed of

Acetic acid .....	5·70 .....	100
Oxide of lead .....	91·30 .....	1608
Water .....	3·00 .....	53

Here the acid is combined with six times as much base as in the  
 neutral acetate, and the water of combination contains half as much  
 oxygen as the oxide of lead.

1·06 parts of effloresced acetate of lead, equivalent to 0·333 of  
 acetic acid, produced by combustion 0·18 of water and 0·574 of  
 carbonic acid. Hence 100 of acetic acid are composed of

Hydrogen .....	6·35
Carbon .....	46·83
Oxygen .....	46·82
	<hr/>
	100·00

Now  $15·56 \times 3 = 46·68$ . Hence it follows that acetic acid  
 contains three volumes of oxygen. The other numbers correspond  
 with four volumes of carbon and six volumes of hydrogen. Sup-  
 posing, then, acetic acid composed of  $6\text{ H} + 4\text{ C} + 3\text{ O}$ , its  
 composition will be as follows :—

Hydrogen .....	6·195
Carbon .....	46·871
Oxygen .....	46·934
	<hr/>
	100·000

The real capacity of saturation of this acid is 15·63, instead of  
 15·56, determined by experiment.

There is an approximation between the results of Gay-Lussac and  
 Thenard and mine. According to them, 100 acid are composed of  
 5·629 hydrogen, 50·222 carbon, and 44·147 oxygen; but when we  
 consider that their experiments were made with acetate of barytes,  
 which, to judge from the quantity of barytes found in it, must  
 have contained a quantity of combined water, the oxygen in which  
 was equal to that in the barytes—when we consider this, it is obvious  
 that they must have obtained much more hydrogen and oxygen than  
 my experiments indicate, instead of a smaller quantity. Probably  
 their balls were so much dried as not only to dissipate all the  
 moisture, but likewise to oxidate a portion of the hydrogen of the  
 acid.

6. *Gallic Acid*.—It is difficult to obtain pure gallic acid. What  
 I employed was obtained by leaving infusion of nutgalls exposed to  
 the influence of the air. The small crystals thus procured were  
 dissolved in water, and crystallized again. I then exposed the

crystals in a small retort to a moderate heat. They at first gave out water, and then sublimed, and left a blackish brown mass, in which it was easy to recognise the presence of tannin; so that these crystals had been composed of gallic acid and tannin. The sublimed acid was colourless, had a bitter taste, and a smell somewhat empyreumatic, owing no doubt to a portion of tannin having been decomposed by the heat.

The sublimed acid does not alter the colour of litmus. It dissolves readily in water, and the solution immediately blackens a solution of iron; but it does not decompose the earthy carbonates. The alkaline carbonates are decomposed by it; and if the mixture contains an excess of alkali, it speedily becomes brown. This decomposition does not appear to take place if the air be excluded.

In consequence of the property which gallic acid has of being decomposed by the influence of the bases with which it is combined, it is difficult to obtain a gallate which is not somewhat altered. To obtain a gallate of lead as pure as possible, I dissolved gallic acid in a very small portion of water, and added carbonate of ammonia in a solid form. I then put the mixture under the receiver of an air-pump, along with sulphuric acid. When the ammonia ceased to effervesce, I pumped out the air. The surplus of ammonia flew off first, and then the water evaporated. The saline mass remaining had a yellowish colour. I dissolved it in warm water, and poured into the solution a boiling solution of nitrate of lead. The whole being put into a phial (which was filled with it), was digested till the gallate of lead assumed the form of grains, which readily separated from the liquid. I decanted off the liquid, and filled the phial with boiling water; and this I repeated till the gallate was sufficiently washed. I then threw it on a filter, and dried it at first by pressure between folds of blotting-paper, and then in a vacuum.

The gallate of lead thus obtained formed a white crystalline powder with a shade of grey. After being exposed to the air for some days, it became brown; but the analytical experiments were made as soon as it became dry.

200 of gallate of lead formed 172.1 of sulphate of lead, equivalent to 127 of oxide of lead. Hence it follows that the salt is composed of

Gallic acid .....	36.5	.....	100
Oxide of lead .....	63.5	.....	173.97

The analysis by combustion furnished only 63 per cent. of oxide of lead. The oxygen contained in 173.97 of oxide of lead is 12.44.

Gallate of lead is not easily decomposed by ammonia. The alkali must be concentrated and hot. I obtained by that means a subgallate, which, being dried in the temperature of  $230^{\circ}$  in a vacuum, where it deposited its water of combination, gave by combustion 84.08 per cent. of oxide of lead. Hence it is composed of

Acid .....	15.92	.....	100
Base .....	84.08	.....	528
<hr/>			
100.00			

But  $173.97 \times 3 = 521.91$ . Hence in the subgallate the acid is combined with thrice as much base as in the neutral gallate.

One part of gallate of lead, equivalent to 0.365 of acid, produced by combustion 0.1568 water and 0.77 carbonic acid. These quantities indicate 5.05 per cent. hydrogen and 57.37 carbon. Hence the oxygen is 37.68.

0.4 of melted gallic acid produced by combustion 0.17 water, equivalent to 4.994 per cent. of hydrogen, and 0.833 carbonic acid, equivalent to 56.64 carbon. The difference between these two results is owing to the decomposition of the gallic acid, by means of which the quantity of carbon is increased. This acid, then, is composed of

Hydrogen .....	5.00
Carbon .....	56.64
Oxygen .....	38.36
<hr/>	
100.00	

But  $12.44 \times 3 = 37.32$ , which approaches very nearly to the result of the first experiment; but if these 38.36 are three volumes, we find that the carbon and hydrogen constitute each six volumes; so that gallic acid is composed of  $6\text{ H} + 6\text{ C} + 3\text{ O}$ ; which constitutes per cent.

Hydrogen .....	5.02
Carbon .....	56.96
Oxygen .....	38.02
<hr/>	
100.00	

From all this it follows, that if we take two volumes of carbon from gallic acid, it becomes acetic acid; if we farther take two volumes of hydrogen, the remainder will be succinic acid.

Thenard and Gay-Lussac, in their *Recherches Phys. Chimiques*, ii. 321, have established three laws for vegetable combinations:—

1. That a substance is acid when its oxygen has to its hydrogen a greater ratio than exists in water between these two bodies. 2. That a substance is resinous, oily, or alcoholic, when its oxygen has to its hydrogen a smaller ratio than in water. 3. That every vegetable substance in which the oxygen bears to the hydrogen the same ratio as in water is analogous to sugar, gum, starch, wood. The analysis of acetic and gallic acids shows us what value ought to be attached to the first and last of these laws. We shall find afterwards, by the analysis of benzoic acid, that the second law is equally erroneous.

Before proceeding further, it will be necessary to make some observations on the substances which have been analyzed. They

are all composed of a small number of volumes, compared with those that are to follow; so that the addition of a single volume or atom of one of the elements would produce a great change in the composition. This is the reason why the greater number of vegetable acids do not constitute genera of analogous substances having the same general character, but differing in some points, so that they must be considered as different *species* of the same genus. There is only one acetic acid, one gallic acid, and wherever these bodies occur in the vegetable kingdom they constitute absolutely the same substance.

As the number of volumes increases in the ternary oxides, they acquire the property of producing a certain number of varieties, or of different species of the same genus, which appear to owe their origin to the adhesion of one or more volumes of one of the elements. This addition, however, in consequence of the great number of volumes of the element contained in the substance, only produces an insensible alteration in its composition when we consider 100 parts only.

We have, then, various species of tannin, gum, sugar, &c.; and I have placed the analysis of mucous (*sacclactic*) and benzoic acid among the substances which form genera and species, because I think it probable that there are other acids differing from them only as different species. I have found, for example, that *sebacic acid* found in the products of the distillation of fatty substances possesses all the chemical characters of benzoic acid; but that it varies from it a little, owing, I supposed, to the presence of foreign bodies, from which I was unable to free it. It is possible that it may differ from benzoic acid, as the tannin of the oak does from that of catechu, or of the *arbutus uva ursi*.

I shall not attempt at present to determine the difference of composition of the species. Such experiments appear too delicate to be executed with success before the generic differences are known. To them, therefore, I have directed my first efforts.

7. *Mucous Acid (Sacclactic Acid\*)*.—Mucate of lead is easily decomposed at a temperature which does not act upon other vegetable salts. It becomes brown; and when afterwards treated with ammonia, this alkali acquires a yellow colour. The same thing happens to the pure acid. Much circumspection is, therefore, requisite, in order to obtain these substances sufficiently dry, without having undergone any decomposition.

A hundred parts of mucate of lead, analyzed by combustion, gave from 51.4 to 51.66 of oxide of lead. Hence 100 acid saturate

\* The name *mucous acid* is very improper, because it indicates an acid in *ous*, which cannot exist without one in *ic* with the same radicle. I have not, however, ventured to change it, because *mucic acid* might, I thought, appear ridiculous. Its salts, however, I shall call *mucates*.

I wonder Dr. Berzelius did not think of having recourse to the old name *sacclactic acid*, which is very proper, and recalls the history of its original discovery. I have used it ever since I was aware of the absurdity of the term *mucous*.—T.

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from 105·75 to 106·87 of oxide of lead; and mucate of lead is  
composed of

Acid .....	48·33	.....	100·00
Oxide of lead .....	51·66	.....	106·87
<hr/>			
99·99			

According to these experiments, mucous acid ought to saturate a quantity of base containing from 7·56 to 7·64 of oxygen.

Mucate of lead treated by ammonia produces a submucate, but of so glairy a consistence that it was impossible to separate it from the liquid before part of it had absorbed carbonic acid; so that I was obliged to abandon the analysis of it.

0·333 of mucous acid produced by combustion 0·149 of water and 0·393 of carbonic acid. This makes 5·26 per cent. of hydrogen, 31·9 carbon, and 62·84 oxygen.

One part of mucate of lead, equivalent to 0·4833 acid, produced 0·21 water, 0·594 carbonic acid. This gives us per cent.

Hydrogen .....	5·105
Carbon .....	33·430
Oxygen .....	61·465
<hr/>	
100·000	

Now  $7·66 \times 8 = 61·28$ . Hence it follows that the acid ought to contain eight volumes of oxygen. The other elements approach six volumes of carbon and ten of hydrogen. This would make the composition of the acid

Hydrogen .....	5·018
Carbon .....	34·164
Oxygen .....	60·818
<hr/>	
100·000	

But there was some reason for suspecting that the hydrogen was contained in it in nine volumes instead of ten; so much the more as I was not sure that either the mucate or the mucous acid itself were quite dry. I thought it necessary to examine that point in the following manner:—I heated mucate of lead till it lost its white colour by dishydrogenization. The mucate analyzed gave 52·1 per cent. of oxide of lead. Burnt in the apparatus so often mentioned it gave 4·7 per cent. of the weight of the acid of hydrogen, and 35 of carbon. On the supposition that mucous acid is composed of  $9\text{ H} + 6\text{ C} + 8\text{ O}$ , it ought to contain 4·54 per cent. of hydrogen and 34·39 of carbon; but 4·54 is less than 4·7, which the acid (already a little burnt) produced; therefore that acid must contain more than nine volumes of hydrogen; therefore the composition of the acid must be  $10\text{ H} + 6\text{ C} + 8\text{ O}$ , and the excess of hydrogen and oxygen found in the analysis must be owing to humidity.

Gay-Lussac and Thenard found mucous acid composed of 3·62 per cent. hydrogen, 83·69 carbon, and 62·69 oxygen. This (the hydrogen excepted) does not differ much from my result.

When we compare the analyses of the tartaric and mucous acids, we find between them so little difference, that we should guess their composition to be very analogous. Yet we see by the analysis of mucate of lead, and by the capacity of saturation of mucous acid, that the arrangement of their atoms is very different.

8. *Benzoic Acid.*—The benzoate of lead is a light crystalline powder, slightly soluble in water. When heated in a temperature somewhat higher than that of boiling water, it melts, and gives out 3·85 per cent. of water. If the heat is too long continued, or raised a little higher, a portion of the acid begins also to evaporate. We must, therefore, employ a retort, to determine the water of combination of this salt; for if a little acid should sublime, it will be deposited on the upper part of the retort.

Three parts of benzoate of lead, cautiously melted, and treated with sulphuric acid diluted with alcohol, produced 1·933 of sulphate of lead, equivalent to 1·4505 of oxide of lead. Hence benzoate of lead is composed of

Benzoic acid .....	51·65	....	49·66	....	100
Oxide of lead .....	48·35	....	46·49	....	93·61
Water .....		....	3·85		
			<hr/>		
			100·00		100·00

Now these 46·49 of oxide of lead contain 3·32 of oxygen, and the 3·85 of water contain 3·39 oxygen; so that the water and oxide of lead contain equal quantities of oxygen.

Fused benzoate analyzed by combustion gave 49·125 per cent. of oxide of lead, which makes 94·05 for 100 of acid. Hence this acid neutralizes a quantity of base containing from 6·69 to 6·72 of oxygen.

Neutral benzoate treated with concentrated ammonia gives a subbenzoate, which being analyzed by combustion gives 74 per cent. of oxide of lead. Though long exposed to a high temperature, it gives no trace of water, not even when its acid begins to be disengaged. Hence it is composed of

Benzoic acid .....	26	.....	100
Oxide of lead .....	74	.....	284·6
			<hr/>
			100

But  $94 \times 3 = 282$ . Therefore benzoic acid ought to contain three volumes of oxygen.

The analysis of benzoic acid cost me much more trouble than any of the others owing to a circumstance which I was long in discovering. Its weak affinity and great volatility occasions the evaporation of a portion of it during the combustion. The portion

evaporated is not decomposed, and, being mixed with the gas, it is not deposited in the water, but is carried probably in the form of gas into the tubes and pneumatic trough. In consequence of this circumstance, the analysis of this acid always gave a loss too great to be ascribed to oxygen. I thought at first that my analyses of the benzoates were inaccurate; but having repeated them with the same result, it became obvious that the cause of the anomaly must be something different. After having made analytical experiments on benzoic acid in a free state, and in that of benzoate and subbenzoate, I found that the loss diminished in proportion as the acid was more strongly retained by the base; so that the subbenzoates gave a less loss than the other salts. This induced me to examine whether there was really an evaporation of undecomposed acid during the experiments. I observed that the vessels through which the gas had passed, and in which it remained, had diminished in transparency, though not very remarkably. As these experiments were mostly made in the very severe winter of 1813-14, it happened sometimes that the temperature of the mercury in the trough was at  $3^{\circ}$  or at zero. On this account a greater quantity of the acid had been deposited in the part of the tube which entered the mercury, where I found sufficient to be collected and examined. It possessed the characters of benzoic acid; but its odour was a little empyreumatic.

The analysis of a quantity of subbenzoate of lead, containing 0.317 of acid, gave 0.1414 water and 0.8645 carbonic acid. Hence the acid is composed of

Hydrogen .....	5.16
Carbon .....	74.41
Oxygen .....	20.43
	<hr/>
	100.00

The most simple ratio in which these bodies can be combined is  $3\text{ O} + 12\text{ H} + 15\text{ C}$ , or (which comes to the same thing)  $\text{O} + 3\text{ H} + 5\text{ C}$ . By turning these volumes into numbers we have the composition of the acid per cent. as follows:—

Hydrogen .....	5.27
Carbon .....	74.71
Oxygen .....	20.02
	<hr/>
	100.00

Now  $6.69 \times 3 = 20.07$ . Hence it follows that the analysis of the subbenzoate has given an exact result; while neither the benzoate nor fused benzoic acid ever gave more than 4.8 hydrogen and 70 carbon.

9. *Tannin from Nutgalls.*—I prepared a cold infusion of nutgalls in water, which I mixed with ammonia till it ceased to act as an acid. I then added a little of the infusion not mixed with

ammonia till I restored the property of reddening litmus paper; that there might be no excess of ammonia. The tannate of ammonia thus formed mixed with a solution of muriate of barytes lets fall abundance of tannate of barytes. I separated it by a filter, and washed it with care. From the experiments of Sir H. Davy, we know that the gallate of barytes is soluble in water; so that in this experiment it is only the tannate which is precipitated. The tannate of barytes mixed with diluted sulphuric acid furnished a super-tannate of barytes soluble in hot water. To this solution I added diluted sulphuric acid till almost the whole was decomposed, I filtered the liquid thus obtained, which had a very astringent taste, and strongly reddened litmus paper. This last property was not owing to the presence of sulphuric acid, for the liquid still retained tannate of barytes in solution. I now mixed it with caustic ammonia till the tannate of barytes began to precipitate. This solution of tannate of ammonia, with a small excess of tannin, was mixed with neutral nitrate of lead as long as any precipitate fell. The tannate of lead thus obtained had a yellowish colour, and became somewhat brown while washed. On examining the combination of tannin with oxide of lead, I found that the oxide has a strong propensity to combine with an excess of tannin, and that the precipitate obtained was in reality a mixture of tannate and super-tannate of lead. I found that the super-tannate might be rendered neuter by boiling it in water as long as that liquid separates tannin. The water dissolves no part of the oxide; and the tannate which remains undissolved is neutral. If the precipitation of the tannate takes place at  $212^{\circ}$  there is very little super-tannate formed.

Neutral tannate thus prepared was dried in a vacuum, not to be exposed to the influence of the oxygen of the air, by which it is a little altered.

Tannate of lead obtained at different times, though by the same means, was analyzed by combustion. It gave 34.12, 34.21, 34.56 per cent. of oxide of lead, probably as the excess of tannin was more or less completely removed. Hence this tannate is composed of

Tannin .....	65.79	.....	100
Oxide of lead .....	34.21	.....	52
<hr/>			
100.00			

Now 52 oxide of lead contain 3.718 of oxygen.

When I attempted to obtain a subtannate of lead by means of the action of ammonia on the neutral tannate, I obtained a tannate of a browner colour, and very mucous; but which, after being washed and dried in a vacuum, gave 34.6 per cent. of oxide of lead. Hence it would appear that ammonia does not decompose the neutral tannate; just as happens when we add an excess of alkali to the tannate of the same alkali.

There is, however, a subtannate which I obtained by precipitating tannate of ammonia with a boiling solution of subnitrate of lead.

This compound is white; and, when dried, it becomes greenish; but I have never obtained it of the same degree of saturation when prepared by different processes. The tannin in it was always combined with more than  $1\frac{1}{2}$  times as much base as in the neutral tannate; though it never contained so much as twice that quantity.

I analyzed tannin by employing for the combustion both super-tannate and tannate as neutral as possible, and the analyses furnished the same result: 0.4 of tannin produced 0.1425 water and 0.7625 carbonic acid. Hence tannin of nutgalls is composed of

Hydrogen .....	4.186
Carbon .....	51.160
Oxygen .....	44.654
	<hr/>
	100.000

But we have seen that 100 of tannin combine with 52 oxide of lead, the oxygen in which is 8.718. Now  $3.718 \times 12 = 44.616$ . Hence we may conclude that tannin contains 12 volumes of oxygen. But there is no doubt that there must be an analogy between gallic acid and tannin, and this analogy can scarcely be any thing else than the same compound radicle combined with different volumes of oxygen. Supposing, then, that tannin, like gallic acid, contains equal volumes of carbon and hydrogen, and that it is  $12\text{O} + 18\text{C} + 18\text{H}$ , or (which comes to the same thing)  $4\text{O} + 6\text{C} + 6\text{H}$ , its composition ought to be per cent.

Hydrogen .....	4.45
Carbon .....	50.55
Oxygen .....	45.00
	<hr/>
	100.00

Here we find a little more hydrogen and a little less carbon than the analysis indicates. This is a necessary consequence of the dis-hydrogenation which tannin undergoes by exposure to the air, in consequence of which the colour of its combinations becomes darker and darker; so that we never can procure this substance in a perfect state, excepting in fresh nutgalls in which the tannin is not yet coloured.

(To be continued.)

#### ARTICLE IV.

*Observations on some Points connected with the Atomic Theory.*

By Thomas Thomson, M.D. F.R.S.

PROFESSOR BERZELIUS, having in his important dissertations on this subject published in the second, third, fourth, and fifth

volumes of the *Annals of Philosophy*, pointed out some difficulties which present themselves when we apply the atomic theory to the salts, and having in a recent paper refused to admit Mr. Dalton's attempts to remove these difficulties as valid—perhaps the reader will not be displeased if I state here, in as few words as possible, how I have been in the habit of viewing these difficulties when they happened to present themselves during my examination of the different genera of salts. When a difficulty occurs in any branch of chemical investigation, the greater number of persons there are who attempt to explain it, so much the sooner, in all probability, will it be removed. Even should my explanations not prove perfectly satisfactory, they may serve to convey a lucky thought to some other person, who may be more fortunate in his endeavours.

1. It appears from the tables of the sulphates, carbonates, and nitrates, published in the second and third volumes of the *Annals of Philosophy*, that the yellow oxide of lead combines always with two integrant particles of acid when it constitutes neutral sulphate, carbonate, and nitrate of lead. The same law holds in the phosphate, borate, oxalate, and all the other neutral salts of lead.

Nitrate of lead is composed of .....	$2\ n + 1\ l$ .....	41·580
Sulphate of potash .....	$1\ s + 1\ p$ .....	11·000

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52·580

Now if we mix 41·58 grains of nitrate of lead with 11 grains of sulphate of potash a double decomposition will take place, and two neutral salts will be produced, namely, nitrate of potash and sulphate of lead, composed as follows:—

Nitrate of potash ....	$1\ n + 1\ p$ .....	12·803
Sulphate of lead ....	$2\ s + 1\ l$ .....	37·974

---

50·777

Now it is obvious to the eye that if the two salts be composed as we have supposed, such a double decomposition is impossible. The first two salts contain two integrant particles of nitric acid; the last two, only one integrant particle; while, on the contrary, there is only one integrant particle of sulphuric acid in the first two salts, but two in the last two. The weight of the first two salts is 52·58 grains; that of the last two, 50·777 grains. Thus about two grains of weight are lost by the decomposition; while a particle of nitric acid must be transmuted into a particle of sulphuric acid. The same absurdities and contradictions will be found to take place whenever we attempt to reduce any double decomposition, by means of a salt of lead, to calculation.

It is very obvious, from the appearance of these contradictions and absurdities, that there must exist some error in our tables; that the salts of lead cannot be constituted as we have supposed them. Now a very slight alteration will remove all the anomalies,

and render the composition of the salts of lead quite conformable to experiment. Reduce the weight of an integrant particle of yellow oxide of lead to one-half the weight which I have given it in my original table, published in the second volume of the *Annals of Philosophy*. Let the weight be 13·987 instead of 27·974. In that case all the salts of lead will be composed of one integrant particle of acid and one integrant particle of oxide of lead; we have

Nitrate of lead composed of	.....	1 <i>n</i> + 1 <i>l</i>	.....	20·790
Sulphate of potash	.....	1 <i>s</i> + 1 <i>p</i>	.....	11·000
				<hr/> 31·790

Forming,

Nitrate of potash	....	1 <i>n</i> + 1 <i>p</i>	.....	12·803
Sulphate of lead	....	1 <i>s</i> + 1 <i>l</i>	.....	18·987
				<hr/> 31·790

Here the double decomposition is obviously possible. The weight of the first two salts is just the same as that of the last two, and the number of integrant particles is the same in both.

It seems to me to be absolutely necessary to remove this anomaly from the salts of lead; nor do I see any other method of doing so except the one I have just now proposed; but if we reduce the weight of an integrant particle of yellow oxide of lead to one-half, it is obvious that it must no longer be considered as a compound of 1 atom lead + 2 atoms oxygen, but of 1 atom lead + 1 atom oxygen. It will be a protoxide instead of a deutoxide. If so, provided it be true that the brown oxide contains just twice as much oxygen as the yellow oxide, numbers adopted on the authority of Berzelius, the brown oxide must be a deutoxide of lead composed of 1 atom lead + 2 atoms oxygen. The red oxide, which is intermediate, must be considered as a compound of one integrant particle of yellow oxide and one integrant particle of brown oxide. This notion was first suggested by Proust, and appears to have been adopted by Mr. Dalton. I own that in consequence of the anomaly which I have just pointed out I have been for some time inclined to the same opinion; but a desire previously to examine the properties of the red oxide of lead under this point of view has hitherto prevented me from making any alteration in the weight of an atom of lead. Berzelius refuses to adopt this opinion. There is, no doubt, another method of getting rid of the anomaly which has been pointed out above without having recourse to it; namely, to double the weight of an atom of all the other metals; but that method would be attended with much greater inconveniences, and could not therefore be adopted with propriety. I do not see any alternative, therefore, in the present state of our knowledge, but that of adopting the opinion of Proust respecting the nature of *red lead*, and considering yellow oxide of lead as a protoxide.

If any person has taken the trouble to study the tables of the

salts which I have published, he will have been struck with some other anomalies of a similar nature to those belonging to the salts of lead; but I forbear touching upon them at present, till we are in possession of more perfect analyses than have been hitherto published.

2. Certain salts have been analyzed, the composition of which, when stated in symbols, presents us with the anomaly of  $1\frac{1}{2}$  atoms of one substance combined with one atom of another. Thus subsulphate of iron is composed thus,  $1 (\text{S O}^3) + 1\frac{1}{2} (\text{F O}^3)$ ; or it contains one integrant particle of sulphuric acid and  $1\frac{1}{2}$  integrant particle of black oxide of iron. As such a composition is obviously impossible, it is clear that in such cases a certain number of integrant particles of sulphuric acid must be combined with a certain number of integrant particles of oxide of iron. If we multiply by two, the anomaly will disappear. We shall then have  $2 (\text{S O}^3) + 3 (\text{F O}^3)$ ; that is to say, the salt is a compound of two integrant particles of sulphuric acid and three integrant particles of black oxide of iron. Such combinations do not present any thing incompatible with the atomic theory, which admits of such combinations; but they constitute exceptions to one of the general laws which Berzelius has deduced from his numerous analyses. The law is, that in all inorganic compounds one of the constituents always enters in the state of a single atom. In the subsulphate of iron there is no single atom, either simple or compound. It consists of 12 atoms of oxygen, three of iron, and two of sulphur; or of two integrant particles of sulphuric acid and three integrant particles of black oxide of iron. The composition of the subsulphate of copper may be explained in the same way. Its symbol is  $1 (\text{S O}^3) + 1\frac{1}{2} (\text{C O}^3)$ , or one integrant particle of sulphuric acid combined with  $1\frac{1}{2}$  integrant particle of black oxide of copper. If we multiply by two we obtain  $2 (\text{S O}^3) + 3 (\text{C O}^3)$ , which I conceive to be the real constitution of the salt; namely, a compound of two integrant particles of sulphuric acid with three integrant particles of black oxide of copper. The subarsenate of lead admits of the same explanation. Its symbol is  $1 (\text{As O}^3) + 1\frac{1}{2} (\text{P O}^3)$ , or one integrant particle of arsenic acid combined with  $1\frac{1}{2}$  integrant particle of yellow oxide of lead. When we multiply by two we obtain  $2 (\text{As O}^3) + 3 (\text{P O}^3)$ ; that is, two integrant particles of arsenic acid united with three integrant particles of yellow oxide of lead.

Various other similar examples might be adduced; but they are all explicable in the same way. They appear to me to constitute so many exceptions to Berzelius's law above-mentioned, and to show that it does not hold so universally as he had supposed.

3. Professor Berzelius has just favoured the chemical world with an analysis of a considerable number of the vegetable acids, and some other vegetable substances. That these experiments have been conducted with the greatest care is evident from the details into which Berzelius has entered; nor have I the least doubt, from the uncommon precision which characterizes whatever is done by this

very skilful chemist, that his results furnish very near approximations to the truth; but from the extreme difficulty attending such analyses, I do not conceive that perfect precision can be attained at first; but I think that the atomic theory furnishes us with such additional checks that, by a judicious application of them to the constitution of these acids as determined by Berzelius, we may obtain results approaching exceedingly near the truth. I shall at present apply that theory to the investigation of the composition of oxalic acid.

According to Berzelius, that acid is composed as follows:—

Oxygen .....	66·534	.....	6 atoms
Carbon .....	33·222	.....	4
Hydrogen .....	0·244	.....	1
	<hr/>		<hr/>
	100·000		11

But as I consider the weight of an atom of hydrogen as twice as great as Berzelius makes it, we must, in order to represent the composition of this acid according to my numbers, double the number of atoms of oxygen and carbon which it contains. It will then be composed of

Oxygen .....	12 atoms
Carbon .....	8
Hydrogen .....	1
	<hr/>
	21

So that it is a very complicated body. The weight of an integrant particle of it must be 18·140. We might, indeed, reduce the number of atoms in this acid to 11 by doubling the quantity of hydrogen, obtained by Berzelius; for the difference seems to be within the limits of the unavoidable errors to which such experiments are liable. But Berzelius does not think that he could have committed any such error. Let us, therefore, have recourse to another method of determining the weight of an integrant particle of oxalic acid.

From the analysis of oxalate of lead made with great care by Berzelius it appears to be a compound of

Oxalic acid .....	100
Yellow oxide of lead .....	307·5

Now there can be no doubt, from a comparison of all the genera of salts hitherto examined, that oxalate of lead is a compound of two integrant particles of oxalic acid and one integrant particle of yellow oxide of lead; so that to find the weight of an integrant particle of oxalic acid we have this proportion:— $307·5 : \frac{100}{2} :: 27·974 : 4·548$  = an integrant particle of oxalic acid. Now this is just the fourth part of the weight of that acid resulting from the

analysis of Berzelius; yet it is the weight of an integrant particle of oxalic acid, as any one may satisfy himself by examining the composition of the oxalates, a table of which I subjoin for the satisfaction of the reader:—

	Number of atoms.		Weight of an integrant particle.	
Oxalate of potash .....	1	ox + 1 p	.....	10·634
Binoxalate of potash .....	2	ox + 1 p	.....	15·268
Quádroxalate of potash .....	4	ox + 1 p	.....	24·536
Oxalate of soda .....	2	ox + 1 s	.....	17·150
Super-oxalate of soda .....	3	ox + 1 s	.....	21·784
Oxalate of ammonia .....	1	ox + 1 a	.....	6·783
Binoxalate of ammonia .....	2	ox + 1 a	.....	11·417
Oxalate of magnesia .....	1	ox + 1 m	.....	7·211
— lime .....	1	ox + 1 l	.....	8·254
Binoxalate of lime .....	2	ox + 1 l	.....	12·888
Oxalate of barytes .....	1	ox + 1 b	.....	14·365
— strontian .....	1	ox + 1 st	.....	11·534
— alumina .....	1	bx + 1 al	.....	6·770
— yttria .....	1	ox + 1 y	.....	13·084
— glucina .....	1	ox + 1 gl	.....	14·467
— zirconia .....	1	ox + 1 z	.....	10·290
— copper .....	2	ox + 1 c	.....	19·268
— potash-and-copper ..	2	ox + 1 p + 1 c	.....	29·902
— soda-and-copper ..	3	ox + 1 s + 1 c	.....	32·410
— ammonia - and - copper .....	2	ox + 1 a + 1 c	.....	26·051
— iron .....	2	ox + 1 i	.....	18·383
Peroxalate of iron .....	3	ox + 1 i	.....	28·017
Oxalate of nickel .....	2	ox + 1 n	.....	18·578
— cobalt .....	2	ox + 1 c	.....	18·594
— lead .....	2	ox + 1 l	.....	37·242
— zinc .....	1	ox + 1 z	.....	9·661
— mercury .....	1	ox + 1 m	.....	80·684
— silver .....	1	ox + 1 s	.....	19·848
— bismuth .....	1	ox + 1 b	.....	14·628
— manganese .....	2	ox + 1 m	.....	17·101
— uranium .....	1	ox + 1 u	.....	19·684
— cerium .....	2	ox + 1 c	.....	28·115
— platinum .....	1	ox + 1 p	.....	17·795

All these salts corroborate the weight of an integrant particle of oxalic acid as deduced from the analysis of oxalate of lead. Here, then, we have two experiments of Berzelius, which are inconsistent with each other; namely, the analysis of oxalic acid and the analysis of oxalate of lead. Both were made with the greatest care; but as they are inconsistent with each other, they cannot both be correct, and there can be no hesitation about which of them

ought to be followed. The analysis of oxalate of lead is much simpler, not liable to the same uncertainties, and susceptible of greater exactness than the analysis of oxalic acid. We ought, therefore, to be guided by it; especially as it is corroborated by various other very exact analyses, as those of oxalate of potash, oxalate of ammonia, and oxalate of strontian. But if we adopt this determination, and compare it with the analysis of oxalic acid, we shall find that this acid must be composed of six atoms; namely, three atoms oxygen, two carbon, and one hydrogen; and its composition will be

			Weight.
Oxygen	..... 64·739	or 3 atoms =	3·000
Carbon	..... 32·413	2	= 1·502
Hydrogen	..... 2·848	1	= 0·132
	<hr/>		<hr/>
	100·000		4·634

Now this is the composition of the acid which I deduced some time ago by comparing my own analysis of oxalic acid with the composition of oxalate of lead as determined by Berzelius. I obtained for the composition of oxalic acid

Oxygen	..... 64
Carbon	..... 32
Hydrogen	..... 4
	<hr/>
	100

My experiment was conducted with great care, and I still consider my result as nearer the truth than either that obtained by Gay-Lussac and Thenard or by Berzelius. My excess of hydrogen amounts to about one per cent., and was probably owing to the salt which I employed in the analysis not being quite free from water. Mr. Dalton has adopted the same constitution of oxalic acid with the above, and probably he has been led to it by the same mode of reasoning from which I deduced it.

I shall take another opportunity of examining the other acids analyzed by Berzelius, by applying to them the test of the atomic theory. What I have here said is sufficient to show us that the most cautious and elaborate experiments are not sufficient of themselves to make us acquainted with the composition of these intricate bodies; though such experiments afford us considerable assistance, and, when compared with the structure of the salts as explained by the atomic theory, will generally be sufficient to give us all the information respecting the constitution of these acids which we can expect to obtain.

## ARTICLE V.

*Observations on the Uses of the Dorsal Vessel, or on the Influence which the Heart exercises in the Organization of articulated Animals, and on the Changes which that Organization experiences when the Heart or the Organ of Circulation ceases to exist.* By M. Marcel de Serres.

(Continued from Vol. iv. p. 355.)

I BEGAN the examination of the dorsal vessel with those species in which we see it beating externally. Among those I chose the larvæ of the coleopteræ and lepidopteræ. The larva of the *geotrupa nasicornis*, being very common, seemed proper for my object. The dorsal vessel of this species is elongated and cylindrical. When separated from the muscles and fatty membranes which surround it, we see that its diameter is the same in almost the whole of its length, being only a little contracted at the two extremities. Having fully ascertained this disposition, I endeavoured to ascertain if there were any ramifications. For this purpose I examined it with the greatest attention, and with the best glasses. The contractions were always confined to the dorsal vessel, and never extended beyond the canal which runs along the back. I then placed this vessel under the lens of my microscope, and could not perceive any ramifications, not even in the membranes which surround it. In vain I endeavoured to find some trace of them in the membrane of the intestinal tube, the fibres of the muscles, especially in those of the rings of the abdomen and mandibles, which ought to have presented them, if any had existed, in consequence of the energy of their contraction, and the need which these organs have of vessels.

I then examined the dorsal vessel of the *geotrupa nasicornis et punctata*; but all my attention was unable to discover the least ramification. I subjected to the same examination a very considerable number of coleopteræ, the largest that I could procure, as the *ateuchus semi-punctatus*, *cetonia aurata et fastuosa*, *scarites gigas*, *cerambyx heros*, *blaps gigas*, and *mortisaga*. In all of them I observed the dorsal vessel without any ramifications. These dissections, however, convinced me that, without a certain attention, ramifications of that vessel may be supposed to exist, on account of the colour and disposition of the hepatic vessels, which, being long and almost capillary, spread over every part of the body, and are often found fixed to it after the intestinal tube has been removed. To determine with certainty this disposition, we must allow the intestinal tube to remain, and dissect in water. This liquid lifts up the hepatic vessels; so that it becomes easy to follow them to their insertion.

Though I could not perceive any ramifications in these species, I was not entitled to conclude that they did not exist in insects. I

therefore made new dissections. The *locusta gygantea* being the largest insect in Europe, I made it the subject of my new observations; but I was not more fortunate in that species than I had been in the preceding. The same thing happened with the *locusta brevipennis*, *verrucivora*, *grisea*, *gryllus lineola*, and *migratorius*. Though all these species are of a considerable size, I tried new dissections on the *blatta occidentalis*, *acheta campestris*, *empusa pauperata*, and *mantis religiosa*. In all these I found the dorsal vessel without any ramification; and what assured me of this was, that after removing it with the greatest care, I could perceive no trace of rupture, which would have been the case if it had been torn from the ramifications of that vessel. No liquid was ever seen to run out; but this, as we shall show hereafter, might depend upon a variety of circumstances.

But it was necessary to verify the observations of Comparetti. For that purpose I examined the dorsal vessel in a great number of *butterflies*, *tenebrios*, and *crickets*. All these dissections confirmed the results that I had already obtained. The same was the case when I studied the organization of the *sphinx*, *noctua*, and *telligonia*, all of them insects of a large size. It remained to be seen whether the dorsal vessel of flies and of syrphus presented any ramifications, as Comparetti has described such with considerable detail. I saw in general in the dipteræ and hymenopteræ the dorsal vessel exhibiting numerous contractions; but I was never able to perceive either vessels or pulsations along the side of the dorsal vessel. Yet I studied this vessel in the *scolia flavifrons*, *apis violacea*, and *syrphus bifasciatus* of Panzer, the largest species to be found in the south of France. I then took the *apis mellifica*, which Comparetti mentions expressly. Though the research in this case was more difficult, on account of the smallness of the species, I did not perceive the cylindrical vessels, of which that anatomist speaks, and which according to him proceed from the extremity of the dorsal vessel, one going to the upper part of the body, the other to the lower. Though the disposition here mentioned by Comparetti indicates the regular course of a vessel, I still believe he was deceived by the appearance of the hepatic vessels.

I terminated these first researches by the examination of the larvæ of the lepidopteræ. I chiefly dissected those of the *sphinx*, of the *tithymale*, and of the *bombyx* of the mulberry. All these larvæ appeared to me to have a dorsal vessel without any kind of ramification. I sought likewise to discover some traces of vascular vessels in the cellular or muscular coats of the intestines—coats which certainly would have received them if any had existed, for it is very well known that the digestive apparatus is very distinct in larvæ. The muscles of the mandibles, examined with the same design, appeared equally destitute of all vestiges of vessels, with whatever care I examined them.

These facts prove that the dorsal vessel in insects is a canal almost cylindrical, only a little tapering at its two extremities. Its con-

tractions are so distinct as to be perceptible on the outside of the body when the skin is not thick, and has a certain degree of transparency. The contractions of this vessel are irregular, and scarcely ever isochronous. This irregularity shows that they are not produced by a liquid in circulation. We shall find hereafter that they do not depend upon that liquid, and that they appear even independent of the organization of the dorsal vessel. These contractions are not equally strong, nor equally numerous, in all the species. Though it is difficult to determine any thing precise on this head, yet they seem more distinct in the larvæ than in the perfect insect. This difference is least distinct in the voracious larvæ, as those of the *geotrupa nasicornis*, and certain species of *sphinx* and *bombyx*.

As to the liquid contained in the dorsal vessel, its colour has always a relation to that of the adipose substance which surrounds this same vessel; consequently it is not uniform in the different species. We observe in fact that the fat, which surrounds the dorsal vessel, has always a colour analogous to that of the liquid contained in the dorsal vessel. This constant similarity of colour may lead to the suspicion that the dorsal vessel is destined for the secretion of that matter so necessary in animals, whose parts increase rapidly when they are transformed into new organs different from those which existed before.

The humour of the dorsal vessel, then, exhibits various shades of colour. It is deep brown in most *caleopteræ*, greenish in certain *orthopteræ*, yellow in the silk-worm, orange in the caterpillar of the willow, transparent in the larvæ of the great butterfly (*paon*), and of a very light colour in most of the *lepidopteræ*. When examined before the microscope, this humour appears composed of a great number of globules, the transparency of which depends upon the colour of the humour itself. This organization announces an analogy of this humour with fat. Like it, we see it composed of small grains, which before the microscope appear each to consist of other smaller grains. These grains are merely little globules of fat, which swim, or are contained in small spherical and membranous sacks. The fat is contained in membranous sacks; for of itself it is fluid, and runs out easily when the sacks are pierced with the point of a fine needle. When thus extracted, it renders water muddy, and divides into small masses. The humor of the dorsal vessel put into water easily mixes with that liquid. A drop of it spread upon a piece of porcelain hardens by evaporation, and then resembles gum.

The coats of the dorsal vessel are in general very thin. It would be difficult to say to what class they belong. As far as I can judge, the external membrane is cellular, and the internal muscular. The dorsal vessel is kept in its position by numerous tracheæ, several of which lose themselves in it. It is probable that these tracheæ, by their crossing each other in every direction, form the outer coat of the vessel. What proves this is that, in certain species, as the larvæ of the *bombyx pavonia major*, we see the dorsal vessel as if

formed of longitudinal white streaks. These streaks are merely the tracheæ extending along the dorsal vessel, and forming on it an inextricable network. It is easy to satisfy oneself that these vessels are tracheæ, by plunging them into a coloured liquid. The whole becomes coloured except the tracheæ, which we see always distinctly, and the principal direction of which is longitudinal. This fact, easily verified, proves, I conceive, that the first membrane of the dorsal vessel is cellular; since in certain species we see distinctly the white fibrillæ of the tracheæ which compose it lose themselves in it. At least we observe that the tracheæ have a great influence on the contractions of the dorsal vessel—contractions always strongest in the points where these exist in great numbers. As it is always the inferior portion of the body that receives the greatest number of tracheæ, so it is the extremity of the dorsal vessel in which the contractions are most frequent and most sensible.\* We may say, then, in general, that the contractions of the dorsal vessel are always stronger in the abdomen than elsewhere; and that on account of the great number of tracheæ found there, and of the direct communication of these tracheæ by means of stigmata with the air. In the larvæ which receive air by the anus, as the libellulas, this fact is still more evident.

If the tracheæ, or, to speak more correctly, the air which they introduce, have a direct influence on the contraction of the dorsal vessel, this ought to be the better perceived if we examine the vessel in the different transformations which the insects undergo. It is known that the same organs, or the same system of organs, undergo great changes in insects, according to the state in which they are. When in the larva state, the digestive system is most preponderant, and the tracheæ passing into the intestinal tube are so numerous that they form very numerous bundles round the intestines, and very multiplied nets. This disposition is particularly evident in the caterpillars. The caterpillars which inclose themselves in cocoons have likewise this peculiarity, that the silky vessels become very large at the time that they are going to change into crystalids. These vessels, then, fill all the cavity of the abdomen, and the intestinal tube diminishes so much that it appears to contract upon itself, and to be reduced almost to nothing. This is particularly the case with the worms that spin a great deal, as the silk-worm, the *bombyx pavonia major*, *media*, *trifolia*, *quercus*, &c. In the perfect insects the organs of generation, and those of motion, assume the greatest preponderance; the tracheæ become likewise very numerous in these parts. Here is a very sensible change produced in the organs of insects, a change which is the consequence of their successive transformations. But does the same thing take place with the dorsal vessel? To determine the point, I compared the dorsal vessel of larvas with those of perfect insects.

\* Lyonnet has made that remark in his excellent *Anatomie de la Chenille du Saule*.

I found that in general the diameter of the dorsal vessel was much more equal in larvæ than in perfect insects. I first thought that the form of the body which in the larvæ exhibits few contractions, but is almost always cylindrical, might be the cause of this equality. But though this cause may have some influence, I do not think it the principal one. It seems to be chiefly owing to the much greater equality with which fat is distributed over the bodies of larvæ than of perfect insects. In those insects which have no fat except in the abdomen, the dorsal vessel is scarce sensible in the thorax, or at least its contractions. But in all, the contractions are always greater in the abdomen; in consequence of the tracheæ which pass into it, as has been already said. But as an organ never becomes larger without the object which it has in view feeling the consequence of it, the fat is much more abundant in the abdomen than any where else. Though this is less evident in the perfect insects than in the larvæ, for reasons already explained, the abundance of adipose matter in the abdomen is still very sensible in them. We shall see in a future part of this memoir how far these facts will enable us to determine the functions of the dorsal vessel.

The dorsal vessel of insects receives nerves, and these in considerable number. Lyonnet \* mentions nine pair in describing the dorsal vessel of the caterpillar of the willow. We cannot fix the number so precisely, especially when we take a general view of this vessel; but it appears to us that it always receives more in the larvæ than in the perfect insects. Nerves seem even to enter and lose themselves in the membranes of this vessel; but we have not been able to verify the observation of Lyonnet, who affirms that several nerves issue from this organ. This skilful observer perceiving the dorsal vessel to receive a great number of nerves, thought that it furnished to the nerves the substance necessary for them. It would be very difficult to prove the truth of such an assertion, or to demonstrate its falsehood. We think it sufficient to mention it, without admitting it; for to judge from analogy, one would not be disposed to believe it.

Before speaking of the muscles of the cellular membrane which keep the dorsal vessel in its position, we shall say a few words on the contractions and dilatations of that organ. This vessel, we have said, exhibits stronger contractions and dilatations in the larvæ than in perfect insects. This is true in general with this restriction, that if these contractions are more distinct, they are on the contrary less frequent. Thus I reckoned only 36 pulsations in the minute in the dorsal vessel of the larva of the large butterfly (*paon*), while I observed more than 82 in the locusta, and more than 140 in the *bremus terrestris*; but in the larvæ the contractions were so strong that the two sides of the membrane of the dorsal vessel touched one

\* See the *Traité Anatomique de la Chenille du Saule*, chap. xi. p. 412, pl. xii. fig. 1.

another when the contraction was at its maximum. In the grasshopper, on the contrary, the contractions carried these membranes scarcely one-fourth of the diameter of the dorsal vessel. The motion perceived in the *bremus terrestris* was rather a continual pulsation than a contraction and dilatation of the membranes of the dorsal vessel. These pulsations, which I reckoned at 140 in a minute, but which in reality are much more frequent, proceed from the bottom to the top, and are very irregular. The contractions and dilatations of the dorsal vessel are so strong in the caterpillars that they shake all their peritoneal membrane and their adipose tissue; which never takes place in the perfect insects. It is obvious that when the dorsal vessel contracts, its diameter must diminish, while it increases in the dilatations.

The dorsal vessel is maintained in its position by muscular fibres, which in general are disposed in triangles, and which increase in breadth from the superior part of the body to its inferior extremity. The muscular fibres have been called wings by Lyonnet, because they form a kind of appendix, which puts one in mind of that organ. These appendices, formed of muscular strings, contain a great number of very small fatty molecules, and some nerves; but these muscular strings, when carefully examined before the microscope, appear composed of very fine and close transverse fibres. Besides these fibres, we see that the fatty matter is contained in a very extensible cellular substance. It is in the numerous cells of this substance that we find a very abundant adipose matter, the colour of which, always analogous to that of the dorsal vessel, is distinguished from the white and more animalized fat contained in the peritoneal membrane. We may conclude that this fat is more animalized than that which touches the dorsal vessel, because its properties are nearly similar to that of animals with vertebræ.

The colour of this fatty substance is almost always the same. It is generally white, and varies only in its shade from snow-white to yellowish-white. Its odour is faint, seldom disagreeable, and often almost nothing. It has no action on vegetable colours. It is very dense; and when disengaged from the membrane that contains it, it falls to the bottom of water. When exposed to the air, it becomes yellow and rancid, yielding a portion of its carbon and hydrogen to the oxygen of the air. I cannot say whether, as is the case in animals with vertebræ, the fat of frugivorous insects is firmer than that of carnivorous insects. All the acids act on the fat of insects with more or less energy. Sulphuric acid chars it very readily, as I have observed in the fat of the *bombyx pavonia major*. The fat of the larva of this species dried in the air, and of consequence become rancid, and then acted upon by sulphuric acid, gives that acid a strong yellow colour, and does not char very quickly. When the fat of insects is treated with nitric acid, a little nitrous gas is exhaled. The fat becomes yellow, coagulates rapidly, combines with a certain quantity of oxygen, and

forms a kind of oxygenized pommade. The fat of the *gryllo-talp* treated with muriatic acid coagulates very quickly, but becomes less yellow, the muriatic acid giving it less oxygen.

From all that has been said, it is evident that it is exceedingly difficult, not to say impossible, by direct observations to know the ramifications of the dorsal vessel; but it would have been improper to have admitted proofs purely negative, without having attempted direct observations in the first place. The first means that presented itself was injections. To them, therefore, I had recourse in the first place.

The membranes of the dorsal vessel being very thin, oppose a great obstacle to the success of mercurial injections. Accordingly, the dorsal vessel almost always bursts before the injection has reached its extremity; so that we cannot depend upon the results of such injections. Yet I ought to observe that, in those parts of the dorsal vessel in which the injection has succeeded, I have never observed the mercury make its way into a single fibrilla, or indicate the smallest ramification. This method appearing to me insufficient, I soon abandoned it, and tried various injections with liquids coloured with cochineal or indigo, according to the colour of the dorsal vessel. These injections, forced in by a very fine syringe, succeeded perfectly, especially in the larvæ of the great butterfly (*paon*), and of the sphinx of the lithymale, and likewise in the mole cricket. In all of them, the injection having penetrated to the extremity of the dorsal vessel, this vessel always appeared to me a simple cylindrical canal without any ramification. I repeated these injections in a great number of individuals of each class, and I always had the same result.

Though these coloured liquids did not show any ramifications of the dorsal vessel, it was possible that these ramifications might become sensible by making this vessel absorb these liquids. For this purpose I put living insects into liquids strongly coloured, and allowed the absorption to take place through the pores of the organs. I had the greater hopes of success, because the conformation of the tracheæ gave me hopes that these vessels would absorb very little of the coloured liquids. In all the insects exposed to this kind of proof, I saw the pulsations of the dorsal vessel cease by little and little, and the humor contained in it coagulate. It was only after this coagulation that the membranes of the dorsal vessel absorbed the coloured liquids, and even much more rapidly than the other organs. This vessel thus coloured formed along the back a bluish or reddish line, which had no ramification whatever; but the white fibrillæ of the tracheæ were always distinguishable, which, as has been said, form the first membrane. In this absorption the tracheæ do not sensibly change their colour. They merely assume a deeper tint; but they sink down completely. The adipose tissue which surrounds the dorsal vessel assumes the form of clots. The muscles separate in such a manner that their fibres become perfectly distinct, and that their attachments may be easily recognized. The

liquid being at last evaporated, the indigo or carmine appears disseminated between the adipose membranes; but not in an uniform manner, as would be the case if these colouring matters were contained in real vessels.

In other attempts which I made I saw the membranes of the tracheæ absorb the colouring matter; but the absorption was so indistinct, that I merely mention it to put those on their guard who may wish to repeat these experiments. In the locusta I perceived often that the absorption by the membranes of the adipose tissue belonging to the muscular fibres, called wings by Lyonnet, was almost as great as that by the dorsal vessel. It is evident that in this last circumstance the coloured line formed by that vessel ought to be less perceptible. On examining these membranes with a strong glass, we always see distinctly the colour forming a continued line in the dorsal vessel, a continuity which we never see at the side nor in the membranes.

These first attempts being insufficient to detect the ramifications of the dorsal vessel, I injected into it, and made it absorb liquids capable of acting chemically on the humour which it contained. Tincture of nutgalls strongly coloured with indigo and carmine, and filtered, in order to have as few undissolved particles of colouring matter as possible, was the first re-agent which I employed. This liquid, forced into the dorsal vessel by an injecting syringe with a capillary opening, made its way to the extremity of the vessel. I repeated this injection at different times on the larvæ of the *geotrupa nasicornis* and great butterfly (*paon*), and likewise on the mole crickets (while alive); but never perceived the coloured liquid to make its way into the smallest branch of the dorsal vessel.

I then repeated these injections in a manner somewhat different; and to be certain that they would penetrate into the dorsal vessel, I introduced a globule of fat into this vessel; and in proportion as I pushed the injection, the globule advanced in the interior of that organ. Thus by little and little it came to its extremity, and then I could have no doubt of the success of my injection. As I did not perceive ramifications, I dissected with great care the membrane which surrounds the dorsal vessel, endeavouring to separate it as completely as possible. Passing then a little filtering paper on the side of the vessel, I compressed it gently, in order to see whether any coloured liquid would issue out; because by the preceding dissection I had of necessity cut the openings of the ramifications if they existed. Whatever care I applied to these experiments, varying them, and repeating them frequently, I never perceived any liquid issuing out. Sometimes, when my assistant did not close completely the inferior opening of the dorsal vessel, some portions of coloured liquor made its escape; but it was always at that extremity.

Finally, having made the tincture of nutgalls to be absorbed by the membranes of the dorsal vessel (the insects being alive), I saw the fat and the humour of the vessel coagulate, and then absorb the



Fig. 4.

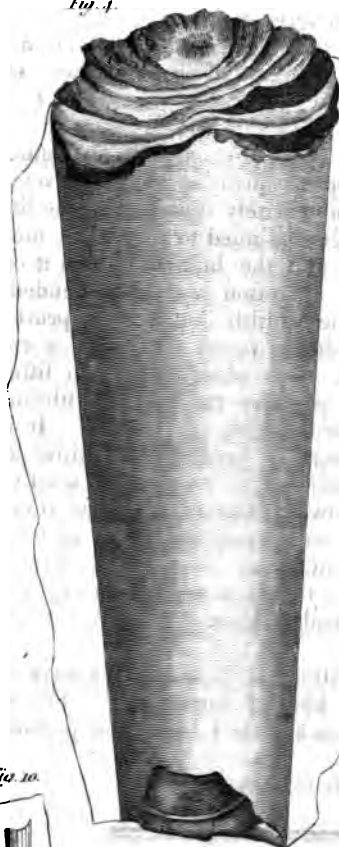


Fig. 5.

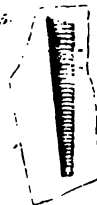


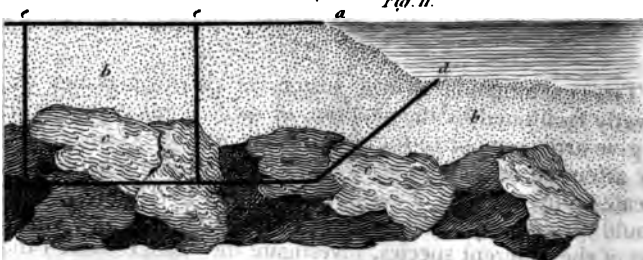
Fig. 6.



Fig. 10.



Fig. 11.



alcohol; but it was never possible to perceive the least lateral ramification. So constant an agreement in researches so delicate seemed sufficient to lead to the conclusion that the dorsal vessel of insects has no ramifications; but as I had not attempted every thing which it seemed possible to do to ascertain the point, I continued my researches.

The acids appearing to me proper to coagulate the humour of the dorsal vessel, I put some drops of nitric acid on this vessel. The contraction of the organ immediately ceased, but the life did not appear injured, and the insect continued to agitate its members as before. The dorsal vessel, and the humour which it contained, became whitish; and as the alteration gradually extended itself to the muscles, they also became whitish, but with a pearl-coloured lustre quite peculiar. The dorsal vessel filled with a coagulated liquor has thus become more easily observable. On lifting it up with precaution, I did not perceive the smallest fibrilla issuing from it, excepting the tracheæ which pass into it. It is always easy to distinguish these organs, because the nitric acid does nothing else than increase their lustre. I afterwards tried the action of various other acids, and always obtained the same results; only they appear more speedily when sulphuric acid of 69° is used. The action of acids is advantageous to enable us to follow the direction of the muscles, as they communicate to these organs a brilliant pearl colour, and render visible the bundles of which they are composed.

Injections made with solutions of tannin have always shown me the dorsal vessel without any kind of ramification. The same was the case when the solution was absorbed by the membranes of the vessel.

(To be continued.)

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## ARTICLE VI.

### *Observations on the *Orthoceratites* of Scotland.*

By John Fleming, D.D. F.R.S.E.

(Communicated to the Wernerian Natural History Society,  
May 21, 1814.)

THE examination of the history of petrified bodies would be greatly facilitated by the adoption of some regular plan, calculated to give arrangement to the information already obtained, and to fix the attention on those objects which ought to be regarded as of the greatest importance. Thus our knowledge of this branch of science would very rapidly increase, were we to examine with care the characters of the different species, investigate the changes which they have undergone in the mineral regions, and ascertain the various rocks in

which they are imbedded, and the relation of these rocks to the strata which constitute the crust of the globe.

By attending to the first of these objects, or to what may be termed the *systematic history of petrifications*, we avoid all ambiguous references, and introduce accuracy into our descriptive mineralogy by the formation of a general language. Had this department been studied with greater care, geologists would not have been so frequently perplexed in accounting for the phenomena of nature. How often do we hear it asserted that the plants, corals, and shells, which are found in a fossil state in the rocks of this country, bear the strongest resemblance to those of Africa and India? Yet when these are subjected to a close examination, they are found to be specifically distinct, and the distracted philosopher is saved the trouble of deluging the earth by a comet, or of changing its axis of motion. Naturalists were long employed in searching for the means which transported the monsters of the equatorial forests to the frozen regions of the north, until the systematic accuracy of a Camper and a Cuvier proved the fossil elephant to be a new species, differing in form and character from the elephants of Africa or India. Before the physical distribution of petrifications can be investigated with success, the particular species must be previously ascertained.

The second object deserving attention in the history of petrifications is the examination of their present condition, with the view of ascertaining the various changes which they have undergone. This may be termed the *chemical history of petrifications*, as it makes us acquainted with the operations performed in the great laboratory of Nature, and enables us to comprehend the intricate process of mineralization.

The third object of importance in the history of petrifications is their situation, the strata in which they are imbedded, the particular formations in which they occur, and the organic remains with which they are associated. This may be termed the *geognostic history of petrifications*, and will be carefully studied by those who are anxious to trace the revolutions which the surface of the earth has undergone, and the various catastrophes which have befallen the former tribes of animals and vegetables. This arrangement will be adopted in the following paper, after having made a few preliminary remarks.

Numerous species of spiral multilocular shells belonging to the genus *nautilus* of Linnæus have been found in a fossil state in the strata of Britain; but the mineralized remains of the straight shells forming the second family of the Linnæan nautili, and included in the modern genus *orthocera*, are of rare occurrence. Lhwydd, in his *Ichnographia Lythophylacii Britannici*, p. 89, informs us that they are found in Gloucestershire, Oxfordshire, and Northamptonshire. He describes them under the title *alveolus*, and appears to have obtained them from alluvial strata. The late Rev. David Ure,

in his History of Rutherglen and Helbrida, characterizes two species of orthoceratites as occurring in the Independent Coal Formation of Lanarkshire. Martin, in his Petrifications of Derbyshire, mentions their occurrence in the lime-stones of that county: and in the third volume of the *Annals of Philosophy* Dr. Thomson, on the authority of Mr. Greenough, informs us that the orthoceratite occurs in Ireland in rocks decidedly belonging to the floetz formation.

In addition to these notices of the occurrence of the orthoceratite in the strata of this country, I have the satisfaction of stating to the Society that my cabinet of petrifications contains ten species from the rocks of Linlithgowshire. These are not all to be considered as new species, and therefore interesting to the conchologist, as it is probable that many of them have been described in the works of foreign mineralogists. But they may be regarded as of some importance by the geologist, since they were all found by myself in strata belonging to the Independent Coal Formation of Werner. In order to elucidate this subject as much as possible, I shall give a figure and description of the different species, that their existence in other districts, or in other strata, may be ascertained by comparison.

#### I.—SYSTEMATIC HISTORY.

**ORTHOCEBA.**—Shell straight or slightly bent, subconical, formed into chambers by simple transverse positions perforated by a tube.

This genus may be conveniently divided into three families, distinguished by the external aspect of the shell.

##### A.—*Surface of the Shell Smooth.*

The shells belonging to this family are regular in their form, and the surface is smooth and even; but when the shells are worn, and the external covering removed, they appear as if ribbed across, owing to the partitions of the chambers becoming visible.

1. *O. Lævis.*—Shell, conical; partitions, waved; chambers, large; pipe, small and central. Plate XXXI. fig. 1.

The length of the specimen which I possess is upwards of three inches. Its breadth at the base is nine-tenths, and at the apex two-tenths, of an inch; the shell is very thin; the chambers are about two-tenths of an inch in depth; the partitions are waved on both sides, and convex; the pipe is central; and in the middle of the shell, where it is most apparent, it is about the fortieth of an inch in diameter.

The shell is now in the form of a yellowish-white soft lime-stone; the inside is filled with granular lime-stone; but the pipe contains the same kind of lime-stone in which the specimen is imbedded.

This is probably the shell referred to by Ure in his History of Rutherglen, p. 306, pl. xvi. fig. 3, as the orthoceratites *superficie lavi*. In some of the specimens which he found, the original shell

still remained of a horny appearance, very thin, and well polished within.

2. *O. Pyramidalis*.—Shell, tapering; partitions, slightly waved; chambers, large; pipe, small and central. Fig. 2.

This species bears a very close resemblance to the preceding in its structure; but the form of the shell is somewhat different, being much longer in proportion to its breadth; the length of the specimen is upwards of six inches; the diameter at the largest end is one inch and two-tenths, and at the apex two-tenths; the last formed chambers are nearly three-tenths of an inch in depth, while the oldest, towards the point, are scarcely one-tenth. A fragment of this species, found contiguous to the one described, and probably a portion of its inferior extremity, measured two inches in diameter.

3. *O. Cylindracea*.—Shell, nearly cylindrical; partitions, slightly waved; chambers, numerous; pipe, minute and central. Fig. 3.

The largest specimen I possess is about three inches and a quarter in length, four-tenths of an inch in diameter at the base, and three-tenths at the apex; the chambers are scarcely one-tenth in depth. When the outer covering of the shell is removed, the chambers appear very distinct, with a flat surface and a small groove in the place of the partitions.

4. *O. Convexa*.—Shell, nearly cylindrical; partitions, thin and convex; pipe, large and lateral. Fig. 4.

The length of this specimen is four inches and a half, the diameter at the base one inch and a quarter, and at the apex upwards of eight-tenths of an inch; the partitions are remarkably thin, and more convex than any of the preceding; the lower chambers are about an eighth of an inch in depth; the pipe is about three-tenths in diameter, and placed about half way between the centre and the margin.

5. *O. Attenuata*.—Shell, tapering; partitions, nearly circular; chambers, large. Fig. 5.

The shell of this species, in one specimen, is very thin, transparent, and glossy, and in some places it is minutely striated across. The length of another is upwards of six-tenths, the breadth at the base about one-eighth, and at the apex about one-twentieth, of an inch. This space contains 15 chambers.

#### B.—Surface of the Shell with Transverse Ridges.

6. *O. Sulcata*.—Shell, tapering; ridges, waved and striated; pipe, small and central. Fig. 6.

The length of the specimen is two inches and six-tenths, the diameter at the base seven-tenths, and at the apex two-tenths, of an inch. It contains 31 ridges, which are twice waved in going round the shell. Both the ridges and intervening grooves are finely striated.

I consider this as the *orthoceratites superficte sulcata* of Ure, pl. xvi. fig. 2. The waving of the ridges he does not mention; but he observes, that the shell "is finely striated in the same direction with the sulci."

7. *O. Undata*.—Shell, tapering; ridges, waved and smooth; pipe, small and central. Fig. 7.

This specimen is one inch and two tenths in length, seven-tenths in diameter at the base, and upwards of five-tenths at the apex; the ridges are 11 in number, more deeply waved, and less numerous, than the preceding species; there is the appearance of an epidermis, of a black colour, and obscurely striated; where the shell is exposed, both the ridges and the grooves are perfectly smooth. An imperfect specimen of an *eschara* adheres to the shell.

8. *O. Annularis*.—Shell, subcylindrical; ridges, distant, nearly even, and smooth. Fig. 8.

The largest portion of this shell which I possess is about an inch in length, and upwards of three-tenths of an inch in diameter; the ridges are nearly one-eighth of an inch asunder, and are more obtuse than the preceding. In the spaces between the ridges there are at least two chambers. I have not been able to observe the pipe, as all the specimens are much incorporated with the lime-stone in which they are imbedded.

9. *O. Rugosa*.—Shell, subcylindrical; ridges, waved and tuberculated, with longitudinal lines; pipe, minute and lateral. Fig. 9.

The length of this specimen is upwards of an inch and a quarter, and the breadth about six-tenths. The ridges are formed into knobs by faint longitudinal lines, which cause the intermediate spaces to exhibit a grooved appearance; they are upwards of two-tenths of an inch asunder, and contain two chambers in the interval; the pipe is very small, and placed close by the edge.

#### C. Surface of the Shell with Longitudinal Planes.

10. *O. Angularis*.—Shell, nearly cylindrical, angular, with about 16 smooth longitudinal planes; pipe, small and lateral. Fig. 10.

I possess about half an inch of this shell, which is nearly of equal thickness, scarcely exceeding the tenth of an inch in diameter.

There is a portion of a *trilobite* adhering to the lime-stone in which this shell is imbedded. It resembles in its general appearance the species from Dudley, in Shropshire, figured in Parkinson's *Organic Remains*, vol. iii. tab. xvii. fig. 11.

#### II.—CHEMICAL HISTORY.

The original shell appears still unchanged in species 5, 6, and 7, of a thin and very delicate texture; but in the remaining species the original structure is completely altered. In general, the shell and the partitions are changed into fibrous or granular lime-stone, the chambers occupied with crystals of quartz and lime-spar, and the

pipe filled with the matter of the surrounding rocks. This is the case with all the perfect multilocular testacea which I have had an opportunity of examining in a petrified state. The pipe, having an external opening, must have been filled at the period of the deposition of the rocks. The shell and the partitions, on the other hand, have been altered, and their particles have assumed a new and more crystalline arrangement, intimating a previous state of fluidity. The cavities of the chambers, having no external opening, are filled with various crystallized substances, which must have entered in a state of solution through the pores of the shell or the partitions.

When the shell is destroyed, or the partitions broken, then the chambers are found filled with the matter of the surrounding rock. The cavities of the chambers of the *O. sulcata* and *O. undata* are filled with clay-iron-stone. One specimen of the *O. cylindracea* is imbedded in a *septarium*, the base of the shell reaching to the centre of the mass, and the apex projecting beyond its circumference. That part of the shell situated in the external covering, and extending to about five-eighths of an inch, is perfectly regular and entire; but near the base of the shell, and towards the centre of the *septarium*, the partitions of the chambers are broken, and the cavities filled with clay-iron-stone and iron-pyrites.\*

### III.—GEOGNOSTIC HISTORY.

The species of the genus *orthocera*, which have now been described, were found in the strata of the county of Linlithgow, a district of country entirely composed of rocks belonging to the Independent Coal Formation of Werner: species 5, 6, and 7, I obtained from a bed of slate-clay at the Blackburn Colliery, in the parish of Livingstone. This bed alternates with white sand-stone, slate-coal, and compact lime-stone. The slate-clay and the lime-stone contain *entrochites*, and other spoils of the Ocean; while the beds of sand-stone and coal contain the remains of vegetables. Such strange arrangements of organic remains are not uncommon in the coal-field of the Lothians. One specimen of species 3, included in the *septarium*, was found in this bed of slate-clay; but other specimens of the same shell, and the remaining species, were found at different places in the middle of the county, imbedded in the strata of lime-stone which traverse the high grounds to the south of Linlithgow.†

In the whole extent of these strata no remains of the *belemnite* have yet been observed. These must have existed had the *orthoceratite* been merely the concamerated alveolus of that curious petri-

\* If this *septarium* was in a state of fusion at its formation, in what manner was the shell of this *orthoceratite* preserved from incorporating with the melted mass, and enabled to retain its position?

† The characters of this lime-stone are taken notice of in my account of the *Old Silver Mine* in Linlithgowshire, inserted in the *Annals of Philosophy*, p. 119 of this vol.

faction. The belemnite, we may add, is one of the rarest fossils in Scotland. Professor Jameson observed it in lime-stone connected with sand-stone and basalt in the Island of Mull; and Mr. Neill found it in slate-clay covered with columnar green-stone in the Shiant Islands; but no other localities have hitherto been mentioned.

It appears to be the opinion of Werner that different rock formations can be discriminated by the petrifications which they contain. How far this opinion may hold true with respect to the petrifications of Germany, we have not the means of ascertaining; but in this country it cannot be considered as the expression of a general law in the distribution of organic remains. In the *transition lime-stone* which occurs between the Crook and Noblehouse \* there are a few impressions of bivalve shells, so closely resembling in appearance the shells found in the floetz lime-stones as to lead to the conclusion that they belong to the same species. In the lime-stone connected with the *old red sand-stone* in the Island of Arran there is a particular species of the genus *productus* of Sowerby, which is one of the most common petrifications in the lime-stones of the Independent Coal Formation of the Lothians and Fife. If we descend from classes and formations to the individual members of a group, it will be found that the remains of the same species of vegetable are distributed through beds of clay-iron-stone, sand-stone, lime-stone, slate-clay, and slate-coal, as I have frequently observed.

The occurrence of the genus of shells which we have been considering in beds of lime-stone, is regarded by some as a proof of such lime-stones belonging to the transition class of rocks. Thus Von Buch, in his Travels through Norway, appears to have referred the lime-stones of that country to the transition class, merely from the circumstance of their containing the remains of orthoceratites. In describing the mineralogical appearances which presented themselves in the neighbourhood of Christiana, where he had discovered rocks of the transition class, he says, "How great was my joy when, at the steep falls of Aggers Elv, above the lower saw-mills, I discovered the orthoceratites, which so particularly distinguish throughout all Europe this formation (transition lime-stone), and this formation alone. They are many feet in length, divided into compartments, and for the most part at the edge and the walls of the compartment changed into calcareous spar. They are by no means unfrequent; several of them generally lie in various directions through one another. Pectinites, and several other not very distinguishable petrifications, appear frequently between them." English Trans. p. 47.—In the transition lime-stones of this country no orthoceratites have been found. They occur in beds of slate-clay and lime-stone of a more recent period. Thus it appears that the distinguishing character of a rock formation

\* See Professor Jameson's Mineralogy of Dumfriesshire, p. 77.

in Norway and Germany is not applicable to the same formation in Britain.

The same species of organic remains which occur imbedded in different strata of the floetz class, appear to have sustained the shock of several revolutions before the total destruction of their tribe; and we may also suppose that part of the race of orthoceratites survived the period of the deposition of the transition class, but became at last enveloped in the beds of the coal formation.

I cannot conclude these observations without taking notice of the great difference in point of size between the existing species of the genus orthocera and those orthoceratites which are found in the mineral regions. The living species of the genus to be met with on the British shores do not exceed a quarter of an inch in length, and required the aid of the microscope for their examination. Only one species has been found in Scotland, the *O. linearis*. (Testacea, Brit. Tab. 30, fig. 9.) It was found by Mr. Montagu in a parcel of minute shells sent to him from Dunbar. It is only a quarter of an inch in length; and its breadth is about one-eighth of its length.

*Mause of Flisk, Fifeshire.*

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## ARTICLE VII.

*On Deepening, Cleaning, Excavating, and Removing Obstructions that prevent Vessels from entering Harbours.* By John Rook, jun. of Akehead, Wigton, Cumberland.

WHEN we examine the force and power of water, we are convinced that it is an engine capable of performing various powerful operations. In its course from the uplands, and through the lowlands, till immersed in the Ocean, it forms for itself spacious channels and deep pools. When man brings its gravitating motion under subjection he obtains a useful and faithful servant, that performs for him numerous important labours. Passing from these reflections to a new modification of the use of that element, there seems to be a probability that it might be rendered useful in the improvement of the entrance of harbours, &c. by means of carrying it through tunnels, formed according to the circumstances of situations. These tunnels or pipes might be constructed either by collected materials, such as stone, wood, and metal, or formed by sinking down to the rock, and directing the tunnel in it according to the object in view. The use to which I would apply the principle is that of passing through these tunnels or pipes as powerful a stream of water as could be obtained; by which means considerable quantities of sand, gravel, &c. might be removed by the violent stream of water issuing forth from its confined channel.

Thus Plate XXXI. fig. 11,

*a, a*, the river flowing into the sea.

*b, b, b*, sand or gravel, being the object of removal.

*c, c, c*, the rock.

*d, d, d*, the tunnel formed in the rock by mining.

*e, e, e*, large augur holes, according to circumstances.

In this way I expect several new harbours might be formed, old ones deepened, cleaned, &c.; and eventually, in all likelihood, many of those dangerous bars, such as Dublin and Linmouth, removed.

Letting the tide flow into a part of a harbour, and then shutting it in by gates, might frequently be resorted to as a means of obtaining a stream of water.

I remain yours truly,

JOHN ROOK, Jun.

*Akehead, Wigton, Cumberland,*  
Dec. 20, 1814.

P. S. I should wish to hear the opinion of any engineer upon the above.

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## ARTICLE VIII.

*A Memoir on Iodine.* By M. Gay-Lussac.

(Continued from p. 109.)

**AZOTE** does not combine directly with iodine. We obtain the combination only by means of ammonia. It was discovered by M. Courtois. I shall give it the name of ioduret of azote. It has been accurately analyzed by M. Colin, and I shall briefly state from him the circumstances of its formation and its nature.

When ammoniacal gas is passed over iodine, a viscid shining liquid is immediately formed, of a brownish black colour, which, in proportion as it is saturated with ammonia, loses its lustre and viscosity. No gas is disengaged during the formation of this liquid, which may be called *ioduret of ammonia*. It is not fulminating. When dissolved in water, a part of the ammonia is decomposed; its hydrogen forms hydriodic acid; and its azote combines with a portion of the iodine, and forms the fulminating powder. We obtain the ioduret of azote directly by putting iodine in fine powder into a solution of ammonia in water. This indeed is the best way of preparing it; for the water is not decomposed, and seems to concur in the production of this ioduret only by determining the formation of hydriodate of ammonia.

The ioduret of azote is pulverulent, and of a brownish black colour. It detonates from the smallest shock, and from heat with a feeble violet colour. I have often seen it detonate spontaneously when properly prepared. When put into potash, azote is dis-

engaged, and the same products are obtained as when iodine is dissolved in that alkali. The hydriodate of ammonia, which has the property of dissolving a great deal of iodine, gradually decomposes the fulminating powder, while azote is set at liberty. Water itself has this property, though in a much weaker degree, as M. Courtois observed long ago. Thus the elements of ioduret of azote are but very little condensed. It ought to be prepared with great precautions, and should not be preserved.

It would be difficult to determine by direct experiment the proportion of the principles of this compound; but we ascertain them correctly in the following manner:—

We have seen that the ratio of hydrogen to iodine is 1·3268 to 156·21; and as ammonia is composed of

Hydrogen .....	18·4756
Azote .....	81·5244

it follows that the ratio of azote to iodine is that of 5·8544 to 156·21; and such is the ratio of the elements of the fulminating compound. If we reduce these elements to volumes by dividing 5·8544 by 0·96913, the density of azote, and 156·21 by 8·6195, the density of the vapour of iodine, we find that the proportion in volume of the elements is one azote and three iodine. We obtain this proportion directly by observing that the vapour of iodine and hydrogen combine in equal volumes; and that in ammonia the volume of hydrogen is to that of azote as three to one.

If we decompose a gramme (15·444 grains) of the fulminating powder, we obtain, at the temperature of 32°, and under the pressure of 30 inches of mercury, a gaseous mixture amounting to 0·1152 litre (7·03 cubic inches, and composed of 0·0864 of the vapour of iodine and 0·0288 of azote. Though this volume be inconsiderable, yet the explosion is very loud, because it is instantaneous. The same difficulty occurs here as in the detonation of the chloruret of azote, and of all the fulminating bodies which are decomposed into simple substances, producing at the same time heat and light. I do not pretend to resolve this difficulty; but is it not possible that the heat and light which make their appearance in these cases is produced by the shock of the gas produced against the air, or any other fluid, as happens when air is compressed or introduced into a vacuum? \* Is it in fact necessary to have recourse to heat to communicate elasticity to gaseous substances condensed in a compound, or, which is the same thing, to put their elements in a state of repulsion? Do we not see, on the contrary, a weak electricity destroy the combinations which resist the repulsive force

\* To explain my idea the better, let us conceive a volume of air in the middle of which is a small metallic ball, containing any elastic fluid in a great degree of compression, and at the same temperature with the surrounding fluid. If we suppose the ball suddenly to burst, which will represent a detonation, we shall have heat and light produced. Now in the detonation of ioduret or chloruret of azote the development of the gas does not appear to me to differ from that of air strongly compressed in our ball.

of a very high temperature? \* Supposing these conjectures to have some foundation, it will remain to explain why, when we mix equal volumes of water and of a solution of nitrate of ammonia at the same temperature, the thermometer, as I have observed, sinks more than  $9^{\circ}$ , though there is a notable increase of density. On the supposition that the capacity of bodies for heat is a function of the absolute quantity of heat which they contain, this fact would lead us to admit that the capacity of the solution of nitrate of ammonia is greater than that of its constituents; but this consequence does not appear to be confirmed by experience; therefore the capacity of bodies for heat does not depend solely upon the absolute quantity of heat which they contain.

I return now to the combinations of iodine with the combustibles, or with those oxides which, not being saturated with oxygen, act like combustibles. I have already spoken of the action of iodine on hydro-sulphuric and phosphorous acids. It remains only to speak of its action on sulphurous acid. In the gaseous state this acid has no action on iodine; but when dissolved in water, the addition of iodine occasions a decomposition of that liquid, and sulphuric acid and hydriodic acid are produced. They cannot be separated by distillation; for at the temperature at which hydriodic acid comes over, sulphurous is reproduced. The liquid in the

\* Chemical phenomena, in my opinion, cannot be explained by heat alone, supposing them to depend solely on the variation of distance which it produces in the particles of these bodies. M. Laplace remarks (*Système du Monde*, 3d edit. ii. 256) that, in order to conciliate planetary attraction with chemical affinity, "we must suppose the dimensions of the particles so small, when compared with the distances between them, that their density is incomparably greater than the mean density of the whole together. A spherical particle, whose radius is equal to the millionth of a metre, ought to have a density more than six thousand thousand millions greater than the mean density of the earth to produce at its surface an attraction equal to gravitation; but the attractive force of bodies considerably surpasses that of gravitation, as they deflect visibly the rays of light, whose direction is not sensibly changed by the attraction of the earth. Hence the density of atoms would prodigiously surpass that of bodies, if their affinities were only modifications of universal gravitation."

Such a supposition appears exaggerated; but let us admit it for a moment, and see whether the diminution of the affinity of a body corresponds with the increase of the distance of its particles produced by heat. Without knowing exactly the cohesion of copper, for example, in a solid and liquid state, we may admit that it is at least a thousand times greater in the former than in the latter state. Let us suppose also, to keep greatly below the truth, that copper in melting increases eight times in bulk. On this exaggerated supposition the distance between the particles of the copper would have only become double; so that the cohesion should have been only four times smaller if it followed the same law as gravitation. Hence it is obvious that, when heat accumulates in a body, it does not diminish the affinity merely by increasing the distance of the particles, but by increasing in a great degree the power of their repulsive faculty, which is doubtless the same with their electric faculty. The figure, the arrangement, and the inertia, of the atoms, may have influence in some chemical phenomena; as, for example, in the congelation of water, and the crystallization of sulphate of soda. But there are an infinity of others which are independent of these, as well as of the separation of the atoms. Such is the combination of hydrogen with oxygen, which takes place only at a red heat, whether the gases be in a condensed or rarified state.

retort, as well as that which condenses in its neck, is strongly coloured by the iodine which the hydriodic acid holds in solution ; but when it comes to the receiver, where it finds sulphurous acid dissolved in water, it becomes colourless, and sulphuric acid is reproduced. The sulphites, the sulphureted sulphites, the white oxide of arsenic, and the hydro-chlorate of protoxide of tin, when mixed with iodine, produce also a decomposition of water, and hydriodic acid is formed. Several hydrogenated substances, particularly the essential oils, alcohol, and ether, yield, according to MM. Colin and Gaultier de Claubry, a portion of their hydrogen to iodine, and convert it into acid. (See Ann. de Chim. t. xc.)

*Action of Iodine on the Oxides.*

Iodine may be made to act upon the oxides either with or without the presence of water ; and as the phenomena are different in the two cases, I shall explain, in the first place, what happens when iodine in the state of vapour is passed over the oxides in a moderate red heat.

The oxide of potassium, produced by the combustion of potassium in oxygen gas, is decomposed by iodine. Oxygen is disengaged, and we obtain ioduret of potassium. It may be shown, by collecting all the products, that the potassium does not retain any oxygen, whatever was its state of oxidation ; but I shall give below an easy means of ascertaining this point. Meanwhile I shall state an experiment which shows it in a decisive manner.

I passed vapour of iodine in a red heat over melted subcarbonate of potash ; and I obtained carbonic acid and oxygen gases in the proportion of two in volume of the first and one of the second, precisely the proportions which exist in the subcarbonate. The oxide of sodium and the subcarbonate of soda are also completely decomposed by iodine. From these experiments it would seem that this substance ought to disengage oxygen from most of the oxides : but this only happens in a small number of cases. The protoxides of lead and bismuth are the only oxides not reducible by heat which presented me with that property. The protoxides of copper and tin, indeed, absorb iodine ; but as the peroxides of these metals do not combine with it, and as no oxygen is disengaged, I conclude that the oxygen of one portion of the protoxide passes into the other portion, and that we obtain a mixture of ioduret and peroxide ; so that it is by the concurrence of the forces that these two protoxides are decomposed by iodine.

Barytes, strontian, and lime, combine with iodine without giving out oxygen gas. The oxides of zinc and iron undergo no alteration. From these facts we must conclude that the decomposition of the oxides by iodine depends less upon the condensed state of the oxygen than upon the affinity of the metal for iodine.

The iodurets of barytes, strontian, and lime, are very alkaline when they are dissolved in water. I consider them as subiodurets.

In this respect they resemble the sulphurets of these earths, which have likewise an excess of base.

Having ascertained that the oxides of potassium and sodium are decomposed by iodine, I wished to know if this would happen when they were combined with acids. Sulphate of potash was not altered by iodine; but, what may appear astonishing, I obtained oxygen with the fluuate of potash, and the glass tube in which the operation was conducted was corroded. On examining the circumstances of this experiment I ascertained that the fluuate became alkaline when melted in a platinum crucible. This happened to the fluuate over which I passed iodine. It appears, then, that the iodine acts upon the excess of alkali, and decomposes it. The heat produced disengages a new portion of fluoric acid or of its radicle, which corrodes the glass; and thus by degrees the fluuate is entirely decomposed.

The experiments, which I have stated, show that chlorine has more energy than iodine; for M. Thenard and I have shown that chlorine gives out oxygen with barytes, strontian, lime, and even magnesia. I find the same thing happens when it is made to act upon the sulphates of these bases; but what is remarkable, we obtain no oxygen when chlorine is made to act upon the peroxide of iron, because a chloruret of that oxide is formed.

The same experiments furnish a new proof that sulphur has less energy than iodine. In fact, if we obtain metallic sulphurets with most oxides, while we do not obtain iodurets, this depends upon the great affinity of sulphur for oxygen, and upon the gaseous state of sulphurous acid. If iodine formed a gaseous acid with oxygen not decomposable by a high temperature, there can be no doubt that we should be able to form a greater number of metallic iodurets than sulphurets. The decomposition of potash, soda, litharge, and oxide of bismuth, and the formation of iodurets with the protoxides of copper and tin, show this to be the case.

It may be worth while to remark that iodine, like sulphur, has little affinity for oxides; and that, except barytes, strontian, and lime, no oxide can remain in combination with iodine at a red heat.

Very different results take place when iodine and oxides are made to act upon each other in contact with water. The water is then decomposed. Its hydrogen unites with iodine, and forms hydriodic acid; while its oxygen forms with iodine a peculiar acid, to which I have given the name of iodic acid. All the oxides, however, do not give the same results. We obtain them only with potash, soda, barytes, strontian, lime, and magnesia. The oxide of zinc precipitated by ammonia from its solution in sulphuric acid, and well washed, gives no trace of iodate and hydriodate.

Let us examine more particularly what happens when iodine is made to act upon a concentrated solution of potash. In proportion

as the iodine dissolves in the alkali, which it does with rapidity, we obtain a granular white precipitate, which melts on burning coals like nitre, and which is decomposed by heat, giving out oxygen and ioduret of potassium. This precipitate is an alkaline iodate of potash. The liquid contains hydriodate. Hence water must have been decomposed; \* its hydrogen, uniting with iodine, has formed hydriodic acid, and its oxygen iodic acid.

The potash solution remains slightly coloured; orange yellow, when the alkali is in excess; but it is of a deep reddish brown when the alkali is saturated with iodine. This colour is owing principally to the solution of iodine in the hydriodate of potash. I found that when a solution of a determinate quantity of potash, so weak as not to allow the iodate of potash to precipitate, is entirely saturated with iodine, we must add a quantity of potash equal to the original dose to bring it to the point when its colour is a light orange-yellow. Even when the solution is saturated with iodine, it is always alkaline; though if we dissolve in water ioduret of potassium or of zinc, we obtain neutral compounds. This difference, which we find in the analogous combinations of sulphur and chlorine, is owing to this, that the forces which tend to decompose water are smaller in the first case than in the second.

Concentrated soda produces, like potash, a fulminating salt, which is partly precipitated, and a hydriodate which remains in solution. Barytes, lime, and strontian, present the same phenomena; with this difference only, that their iodates are very little soluble. This last circumstance enables us to obtain these salts in a state of purity. In order to obtain the iodates of potash and soda free from hydriodates, and perfectly neutral, it is necessary to crystallize them repeatedly, which greatly diminishes their quantity. I shall therefore state the method which I employ to prepare them in order to avoid that inconvenience.

Upon a determinate quantity of iodine I pour solution of potash or soda till the liquid ceases to be coloured. I then evaporate to dryness, and digest the dry salt in alcohol of the specific gravity 0·81 or 0·82. As the iodate is not soluble in this liquid, while the hydriodate is very soluble, the two salts easily separate from each other. After having washed the iodate two or three times with alcohol, I dissolve it in water, and neutralize it with acetic acid. I evaporate to dryness, and digest the dry salt in alcohol, to remove the acetate. After two or three washings, the iodate is pure. As for the alcohol containing the hydriodate, we distil it off, and then neutralize the potash by means of hydriodic acid.

Here it may be asked whether, at the moment that the alkali acts upon the iodine, iodate and hydriodate form and exist separately.

\* I reason on the hypothesis of the existence of hydriodates. But if we do not admit their existence, the oxygen which combines with the iodine, and converts it into an acid, must have been furnished by a portion of the potash.

We shall endeavour to answer this question hereafter. We shall terminate what concerns the action of the oxides on iodine by treating of those in which the oxygen is very little condensed, as the oxides of mercury, gold, and silver.

M. Colin has observed, that when we expose to a heat between  $140^{\circ}$  and  $212^{\circ}$  a mixture of water, iodine, and peroxide of mercury, super-iodate of mercury is formed, which is held in solution in the water and an iodate with excess of base, which is insoluble, and remains mixed with the red ioduret which is formed at the same time. Oxide of gold treated in the same manner does not appear to form an ioduret; for after a great number of washings, metallic gold remains, and the water holds an acid iodate in solution. We may suppose that the water has been decomposed, and that the oxides of mercury and gold act upon iodine like the alkalies; but if we consider that the oxide of zinc does not form an iodate, it will become exceedingly probable that the iodic acid has been formed at the expense of the oxygen of a part of the oxide. If we recapitulate the action of the oxides on iodine, it will appear,

1. That the alkaline oxides in which the oxygen is very much condensed, and which completely neutralize the acids, occasion with iodine the decomposition of water, and produce iodates and hydriodates.

2. That the metallic oxides in which the oxygen is still very much condensed, though less than in the preceding, and which do not completely neutralize the acids, do not produce with iodine a force sufficiently great to decompose water and form iodates.

3. That the oxides in which the oxygen is weakly condensed cannot concur with iodine in decomposing water; but that they convert iodine into an acid by yielding to it a portion of their oxygen.

Such are the general results of the action of iodine on the oxides. We shall afterwards treat in detail of some saline combinations of iodine; but at present we propose to state the properties of iodic acid.

We have seen that the acid is only formed by the concurrence of different forces, and that it is always combined with some base, from which, of course, we must separate it. At first sight it seems that we might treat with hydro-sulphuric acid the iodates of easily reducible metals; but the iodic acid would be at the same time decomposed by this acid, because its elements are very little condensed.\* After various trials, I adopted the following process:— Upon iodate of barytes I pour sulphuric acid diluted with twice its weight of water, and heat the mixture. The iodic acid quickly abandons a portion of its base, and combines with the water. But although a quantity of sulphuric acid was employed not sufficient to

\* Hydro-sulphuric acid may be employed to decompose phosphate of lead and obtain phosphoric acid.

saturate the whole of the barytes in the iodate, a small portion of it always remains mixed with the iodic acid. If we endeavour to separate this portion by adding barytes-water, the two acids precipitate together. I suppose that the presence of a small quantity of sulphuric acid in the iodic acid depends upon the strong affinity of this last acid for barytes. I do not think there is reason to believe that the two acids have a tendency to combine together, in consequence of which the iodate is decomposed.

The iodate of lime and sulphuric acid give similar results. When the same salt and oxalic acid are used, the decomposition seems to be more complete.

Hitherto iodic acid has only been obtained in combination with water, and it is very probable that this liquid is as necessary as a base to keep the elements of the acid united, as we see is the case with sulphuric acid, nitric acid, &c.\* Its taste is very acid when it is concentrated. Light does not decompose it. It may be evaporated to the consistence of a syrup. But if its temperature be raised to about  $392^{\circ}$ , it is decomposed entirely into iodine and oxygen. Sulphurous and hydro-sulphuric acids immediately separate iodine from it. This acid and hydriodic acid are decomposed almost completely, like sulphurous and hydro-sulphuric acids. If we mix it with concentrated hydro-chloric acid, chlorine is disengaged. Sulphuric and nitric acids have no action on it. With solution of silver it gives a white precipitate, very soluble in ammonia. It combines with all the bases, and produces all the iodates which we obtain by making the alkaline bases act upon iodine in water. It likewise forms with ammonia a salt, which fulminates when heated, the existence of which I announced some time ago.

From experiments which I shall state in speaking of the iodates, this acid is composed of

Iodine .....	100
Oxygen .....	31.927

Now the first possible combination of iodine and oxygen being

Iodine .....	100
Oxygen .....	6.4017

Iodic acid is evidently a compound of

Iodine .....	100
Oxygen .....	$6.4017 \times 5 = 32.0085$

That is to say, that iodic acid contains five proportions of oxygen.

\* The existence of acids without water shows a greater reciprocal affinity in their elements than in those acids which exist only by means of water or a base.

## ARTICLE IX.

*Annual Meteorological Table for Sidmouth, Devon, 50° 41' N. lat. 3° 13' W. long. By Dr. Clarke.*

Annual Meteorological Table for Sidmouth, Devon, 50° 41' N. lat. 3° 18' W. long. By Dr. Clarke.

Thermometer.—Northern Exposure.										Barometer.				Hygrometer of De Luc.				Weather.		Winds.		Rain.										
Month.	Day	Highest by Six's Register	Wind	Mean of the highest	Mean of the lowest	Mean of highest and lowest	Mean of morning	Mean of afternoon	Mean of A. M. and P. M.	Mean of A. M. at night	Mean of A. M. 2 P. M. and 11 P. M.	Day	Highest	Wind	Lowest	Wind	Mean for the month	Day	Greatest moisture	Wind	Least moisture	Wind	Range in 24 hours	Mean	Fine.	Cloudy.	Wet.	N and NE	E and SE	S and SW	W and NW	Inches and decimals
Jan.	29	43.0	NW	21	15.0	N	31.0	35.0	29.0	32.0	35.0	17	30.21	S	E	29.25	5.0	NW	29.63	0.1	—	—	—	—	16	14	19	4	3	6	3.95	
Feb.	5	53	NW	21	38	N	38	42	39	36	35	12	17	30.51	N	E	6.29	5.3	W	30.14	3.5	—	—	—	20	17	9	11	3	5	4.65	
March	27	54	S	W	7	37	N	E	40	44	35	39	15	30.50	N	E	9.28	5.6	W	29.56	2.9	—	—	—	17	21	13	13	5	5	4.75	
April	19	63	N	4	35	N	E	49	56	43	50	50	8	30.32	S	E	2.29	4.3	S	W	29.92	3.4	—	—	15	11	4	7	11	8	3.35	
May	26	67	S	11	55	E	59	61	44	52	51	10	11	30.51	E	5.29	4.1	S	E	30.04	4.7	—	—	20	10	1	6	14	9	4.60		
June	30	75	S	7	43	S	E	59	65	51	58	10	24	30.41	E	3.29	7.6	N	E	30.10	3.3	—	—	18	11	5	9	4	19	2.85		
July	25	76	S	E	2	47	S	W	61	71	55	9	23	30.36	S	E	30.29	7.4	S	30.04	3.5	—	—	20	11	1	4	12	14	1.60		
Aug.	4	76	S	28	46	N	61	70	54	62	63	10	31	30.39	S	24	29	7.0	S	W	30.08	4.0	—	—	16	11	2	2	20	7	1.50	
Sept.	1	73	S	10	42	S	W	57	66	50	52	9	1	30.41	S	24	29	7.0	S	W	30.13	2.7	—	—	16	11	2	2	20	7	1.50	
Oct.	14	61	S	6	23	NW	46	55	44	50	49	12	10	30.34	N	W	19	39	19	S	W	29.57	5.3	—	—	17	14	1	5	11	14	3.20
Nov.	14	57	S	21	25	NW	41	49	39	44	43	11	11	30.44	S	W	30.29	3.6	W	29.55	5.3	—	—	15	11	1	5	11	14	1.60		
Dec.	11	57	S	26	28	N	E	42	48	40	44	17	20	30.31	N	W	22	12	N	E	29.59	5.9	—	—	19	14	5	4	12	10	4.05	

Thermometer.

Highest by six's Register, July 25..... 76°

Highest by six's Register, Jan. 31..... 43

Lowest by six's Register, July 2..... 15

Lowest by six's Register, Jan. 31..... 15

Mean of the highest..... 48

Mean of the lowest..... 43

Mean of the two..... 49

Mean of morning..... 49

Mean of afternoon..... 53

Mean of 9 a.m. and 3 p.m..... 51

Mean of 9 a.m. and 3 p.m. and 11 p.m..... 45

Mean of 9 a.m. and 11 p.m..... 49

Mean of 9 a.m. and 11 p.m. and 11 p.m..... 50

Mean of 9 a.m. and 11 p.m. and 11 p.m. and 11 p.m..... 50

Mean of 9 a.m. and 11 p.m. and 11 p.m. and 11 p.m. and 11 p.m..... 50

Barometer.

Highest, Feb. 17..... 30.51

Lowest, Jan. 29..... 29.50

Greatest range, Jan. 28—29..... .91

Annual mean..... 29.95

Weather.

Fine..... 306

Cloudy..... 19

Wet..... 140

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days.

N and N E..... 81

E and S E..... 77

S and S W..... 107

W and N W..... 100

Annual mean..... 365

Days

## ANNUAL RESULTS.

Thermometer.		Barometer.		Hygrometer.		Days.	
Highest by Six's Register, July 25.....	76°	Highest, Feb. 17.....	30.51	Fine.....	206	N and NE.....	81
Lowest by Six's Register, Jan. 31.....	15	Lowest, Jan. 29.....	29.50	Cloudy.....	19	E and SE.....	77
Mean of the extremes.....	48	Greatest range, Jan. 29—29.....	0.1	Wet.....	140	S and SW.....	107
Mean of the mean of the highest.....	56	Annual mean.....	29.95		365	W and NW.....	100
Mean of the mean of the lowest.....	43						385
Mean of the two.....	49						
Mean of the morning.....	49						
Mean of the afternoon.....	53						
Mean of morning and afternoon.....	51						
Mean of night.....	45						
Mean of 1 P. M. and 11 P. M.....	49						
Mean of 2 P. M. and 8 P. M.....	43						
Mean of 24 hours.....	47						
Mean of 24 hours.....	47						

*De Luc's Hygrometer.*  
Mean for the months... 39°

Rain for the year ..... 36.73

Inches.

## ARTICLE X.

*Magnetical and Astronomical Observations at Hackney Wick.*  
By Col. Beaufoy.Latitude,  $51^{\circ} 32' 40.3''$  North. Longitude West in Time  $6^{\text{h}} \frac{22}{100}$ .

Feb. 4, Emersion of Jupiter's {  $12^{\text{h}} 22' 55''$  Mean Time at Hackney Wick.  
 4th Satellite ..... {  $12 23 02$  Ditto at Greenwich.  
 Feb. 7, Immersion of Jupiter's {  $12 05 26.4$  Ditto at Hackney Wick.  
 1st Satellite ..... {  $12 05 33.2$  Ditto at Greenwich.

*Magnetical Observations.*

1815.

Month.	Morning Observ.			Noon Observ.			Evening Observ.	
	Hour.	Variation.		Hour.	Variation.		Hour.	Variation.
Jan. 18	8 <sup>h</sup> 45'	$24^{\circ} 15' 46''$		1 <sup>h</sup> 30'	$24^{\circ} 19' 33''$			
Ditto 19	8 40	$24 16 33$		2 00	$24 18 33$			
Ditto 20	8 50	$24 15 50$		1 20	$24 19 40$			
Ditto 21	8 55	$24 15 00$		1 45	$24 19 09$			
Ditto 22	—	—		1 25	$24 22 01$			
Ditto 23	8 50	$24 18 20$		1 35	$24 21 02$			
Ditto 24	—	—		1 50	$24 20 44$			
Ditto 25	8 45	$24 15 17$		1 25	$24 21 50$			
Ditto 26	8 55	$24 15 22$		—	—			
Ditto 27	8 50	$24 16 18$		1 25	$24 21 18$			
Ditto 28	8 50	$24 16 13$		1 30	$24 20 13$			
Ditto 29	8 45	$24 18 06$		1 25	$24 22 01$			
Ditto 30	8 50	$24 21 14$		1 35	$24 21 41$			
Ditto 31	8 50	$24 17 58$		1 50	$24 22 03$			

1815.

Mean of Observations in Jan.	Morning	at 8 <sup>h</sup> 47' .....	Variation	$24^{\circ} 16' 46''$	} West.
	Noon	at 1 36 .....	Ditto	$24 20 12$	
	Evening	at — — .....	Ditto	— — —	
1814. Ditto in Dec.	Morning	at 8 44 .....	Ditto	$24 18 02$	} West.
	Noon	at 1 30 .....	Ditto	$24 20 36$	
	Evening	at — — .....	Ditto	— — —	
Ditto in Nov.	Morning	at 8 41 .....	Ditto	$24 16 20$	} West.
	Noon	at 1 40 .....	Ditto	$24 20 37$	
	Evening	at — — .....	Ditto	— — —	
Ditto in Oct.	Morning	at 8 39 .....	Ditto	$24 14 08$	} West.
	Noon	at 1 42 .....	Ditto	$24 21 45$	
	Evening	at — — .....	Ditto	— — —	
Ditto in Sept.	Morning	at 8 32 .....	Ditto	$24 14 33$	} West.
	Noon	at 1 39 .....	Ditto	$24 23 17$	
	Evening	at 6 19 .....	Ditto	$24 16 50$	
Ditto in Aug.	Morning	at 8 30 .....	Ditto	$24 14 13$	} West.
	Noon	at 1 39 .....	Ditto	$24 23 48$	
	Evening	at 6 57 .....	Ditto	$24 16 31$	
Ditto in July.	Morning	at 8 41 .....	Ditto	$24 13 29$	} West.
	Noon	at 1 42 .....	Ditto	$24 23 44$	
	Evening	at 6 58 .....	Ditto	$24 17 00$	
Ditto in June.	Morning	at 8 44 .....	Ditto	$24 13 10$	} West.
	Noon	at 1 39 .....	Ditto	$24 22 48$	
	Evening	at 6 52 .....	Ditto	$24 16 29$	
Ditto in May.	Morning	at 8 45 .....	Ditto	$24 13 12$	} West.
	Noon	at 1 44 .....	Ditto	$24 22 13$	
	Evening	at 6 38 .....	Ditto	$24 16 14$	

Mean of Observations in April.	Morning	at	8 <sup>h</sup> 45'.....	Variation	24° 12' 53"	West.
	Noon	at	1 48 .....	Ditto	24 23 53	
	Evening	at	6 29 .....	Ditto	24 15 30	
Ditto in March.	Morning	at	8 52 .....	Ditto	24 14 29	West.
	Noon	at	1 52 .....	Ditto	24 23 08	
	Evening	at	6 11 .....	Ditto	24 15 33	
Ditto in Feb.	Morning	at	8 47 .....	Ditto	24 14 50	West.
	Noon	at	1 52 .....	Ditto	24 20 58	
	Evening	at	— — .....	Ditto	— — —	
Ditto in Jan.	Morning	at	8 52 .....	Ditto	24 15 05	West.
	Noon	at	1 53 .....	Ditto	24 19 03	
	Evening	at	— — .....	Ditto	— — —	
1813. Ditto in Dec.	Morning	at	8 53 .....	Ditto	24 17 39	West.
	Noon	at	1 51 .....	Ditto	24 20 30	
	Evening	at	— — .....	Ditto	— — —	
Ditto in Nov.	Morning	at	8 42 .....	Ditto	24 17 17	West.
	Noon	at	1 54 .....	Ditto	24 20 24	
	Evening	at	— — .....	Ditto	— — —	
Ditto in Oct.	Morning	at	8 45 .....	Ditto	24 15 41	West.
	Noon	at	1 59 .....	Ditto	24 22 53	
	Evening	at	— — .....	Ditto	— — —	
Ditto in Sept.	Morning	at	8 53 .....	Ditto	24 15 46	West.
	Noon	at	2 02 .....	Ditto	24 22 32	
	Evening	at	6 03 .....	Ditto	24 16 04	
Ditto in Aug.	Morning	at	8 44 .....	Ditto	24 15 55	West.
	Noon	at	2 02 .....	Ditto	24 23 32	
	Evening	at	7 05 .....	Ditto	24 16 08	
Ditto in July.	Morning	at	8 37 .....	Ditto	24 14 32	West.
	Noon	at	1 50 .....	Ditto	24 23 04	
	Evening	at	7 08 .....	Ditto	24 16 43	
Ditto in June.	Morning	at	8 30 .....	Ditto	24 12 55	West.
	Noon	at	1 33 .....	Ditto	24 22 17	
	Evening	at	7 04 .....	Ditto	24 16 04	
Ditto in May.	Morning	at	8 22 .....	Ditto	24 12 02	West.
	Noon	at	1 37 .....	Ditto	24 20 54	
	Evening	at	6 14 .....	Ditto	24 13 47	
Ditto in April.	Morning	at	8 31 .....	Ditto	24 09 18	West.
	Noon	at	0 59 .....	Ditto	24 21 12	
	Evening	at	5 46 .....	Ditto	24 15 25	

*Magnetical Observations continued.*

1815.

Month.	Morning Observ.			Noon Observ.			Evening Observ.	
	Hour.	Variation.		Hour.	Variation.		Hour.	Variation.
Feb. 1	8 <sup>h</sup> 45'	24°	17' 36"	1 <sup>h</sup> 30'	24°	20' 27"	Not observed.	Not observed.
Ditto 2	8 45	24	15 52	1 30	24	20 10		
Ditto 3	8 40	24	15 43	—	—	—		
Ditto 4	8 40	24	18 15	1 25	24	21 48		
Ditto 5	8 30	24	14 20	1 30	24	21 26		
Ditto 6	8 55	24	14 42	1 45	24	22 03		
Ditto 7	8 45	24	15 11	1 40	24	19 37		
Ditto 8	8 45	24	16 40	1 25	24	22 23		
Ditto 10	8 35	24	15 13	1 40	24	21 49		
Ditto 11	8 40	24	15 15	1 30	24	23 16		
Ditto 12	8 45	24	15 14	1 35	24	23 12		
Ditto 13	—	—	—	1 30	24	22 25		
Ditto 14	8 20	24	15 07	—	—	—		
Ditto 15	8 40	24	14 06	1 40	24	22 47		
Ditto 16	8 55	24	14 36	—	—	—		
Ditto 17	8 30	24	14 12	1 30	24	22 42		

*Comparison of the Variations in the Years 1813, 1814, and 1815.*

		1813.	1814 and 1815.	Difference.
April	Morning .....	24° 09' 18"	24° 12' 53"	+ 3' 35"
	Noon .....	24 21 12	24 23 53	+ 2 41
	Evening .....	24 15 25	24 15 30	+ 0 05
May	Morning .....	24 12 02	24 <del>12</del> <del>49</del>	+ 0 <del>47</del> <del>13</del> <del>12</del> <del>+</del>
	Noon .....	24 20 54	24 22 13	+ 1 19
	Evening .....	24 13 47	24 16 14	+ 2 27
June	Morning .....	24 12 35	24 13 10	+ 0 35
	Noon .....	24 22 17	24 22 48	+ 0 31
	Evening .....	24 16 04	24 16 29	+ 0 25
July	Morning .....	24 14 32	24 13 29	- 1 03
	Noon .....	24 23 04	24 23 44	+ 0 40
	Evening .....	24 16 43	24 17 00	+ 0 17
Aug.	Morning .....	24 15 55	24 14 13	- 1 42
	Noon .....	24 23 32	24 23 48	+ 0 16
	Evening .....	24 16 08	24 16 31	+ 0 23
Sept.	Morning .....	24 15 46	24 14 33	- 1 13
	Noon .....	24 22 32	24 23 17	+ 0 45
	Evening .....	24 16 04	24 16 50	+ 0 46
Oct.	Morning .....	24 15 41	24 14 08	- 1 33
	Noon .....	24 22 53	24 21 45	- 1 08
	Evening .....	— — —	— — —	— — —
Nov.	Morning .....	24 17 17	24 16 20	- 0 57
	Noon .....	24 20 24	24 20 37	+ 0 13
	Evening .....	— — —	— — —	— — —
Dec.	Morning .....	24 17 39	24 18 02	+ 0 23
	Noon .....	24 20 30	24 20 36	+ 0 06
	Evening .....	— — —	— — —	— — —
Jan.	Morning .....	24 15 05	24 16 <del>46</del>	+ 1 41
	Noon .....	24 19 03	24 20 12	+ 1 09
	Evening .....	— — —	— — —	— — —

In deducing the mean of the observations, that of the morning of Jan. 30 is rejected, the variation exceeding that of any former day, without any apparent cause.

*Jan. 18.*—The needle in the morning unsteady, and followed by a fall of snow. During the fall the needles were quite steady.

*Jan. 27.*—Needles unsteady at noon: the wind afterwards blew fresh from the S. E., accompanied with snow.

*Feb. 12.*—The needles at noon vibrated 14' 40" at intervals. The wind blew strong from the S. W. The wind afterwards increased, and shifted to the westward, accompanied with hard rain and showers of hail.

Rain fallen { Between noon of the 1st Jan. } 0.694 inches.  
 { Between noon of the 1st Feb. }

## ARTICLE XI.

## ANALYSES OF BOOKS.

*A Voyage to Terra Australis, undertaken for the Purpose of completing the Discovery of that vast Country, and prosecuted in the Years 1801, 1802, and 1803, in his Majesty's Ship the Investigator; and subsequently in the armed Vessel Porpoise, and Cumberland Schooner. With an Account of the Shipwreck of the Porpoise, arrival of the Cumberland at the Mauritius, and Imprisonment of the Commander during six Years and a half in that Island. By Matthew Flinders, Commander of the Investigator. In two Volumes, with an Atlas. London, 1814.*

THE charts which accompany this work have been constructed with uncommon care, and are probably the most complete of their kind hitherto published. The workmanship does great honour to Captain Flinders, and to the artists employed.

Captain Flinders, by way of introduction to his own voyage, gives a preliminary account of the previous discoveries made in the Terra Australis by preceding voyagers. The north coast of New Holland appears to have been discovered in 1606, by the Duyfhen, a Dutch yatch, dispatched from Bantam to explore the islands of New Guinea. But the commander of this vessel was not aware of the importance of his discovery, considering the country which he observed as a part of New Guinea. Torres, a Spanish navigator, saw it the same year; but was as little conscious of the importance of his discovery. His letter to the King of Spain remained unknown, till a copy of it was found when Manilla was taken by the British forces in 1762. Mr. Dalrymple made it known to the public, and gave the name of Torres to the strait on the north-east side of New Holland, through which that navigator appears to have passed; and this name has been generally adopted. Further progress was made in the investigation of the north coast, by John Carstens, in 1623, by Gerrit Tomaz Pool and Pieterz Pieterzen, in 1636, and by three Dutch vessels, in 1705.

In 1770, Captain Cook sailed along the north coast, and ascertained that New Guinea is not connected with New Holland. In 1791, Lieutenant M'Lure sailed along the north coast of New Holland, and determined the position of several islands, shoals, and projecting points of land. These were all the navigators that preceded Captain Flinders in his examination of the north coast. But Captains Bligh and Portlock, in 1792, and Messrs. Bampton and Alt, in 1793, sailed through Torres Straits, and added considerably to our knowledge of that intricate navigation.

The west coast of New Holland appears to have been first seen by Dirk Hartog, commander of the ship Endragt, outward-bound from Holland to India. He fell in with it in 1616, about latitude

$26\frac{1}{2}^{\circ}$ , south, and coasted along it to latitude  $23^{\circ}$ . Edel, another Dutch Captain, fell in with that part of the coast which bears his name, in 1619. It was seen by the ship *Leeuwin* in 1622, and by the ship *Vianen* in 1623. In 1629, the *Batavia*, commanded by Francisco Pelsert, was wrecked on the coast of New Holland, on the rocks called Abrolhos. Captain Pelsert coasted along the shore a considerable way in a boat, and then sailing to Batavia, returned in another vessel, to take off the people left upon the Abrolhos rocks. Tasman is supposed to have examined part of the north-west coast of New Holland in 1644. The buccaneers, with whom Dampier made a voyage round the world in 1688, came upon the north-west coast of *Terra Australis*, for the purpose of careening their vessel and procuring refreshments. They made the land about latitude  $16^{\circ} 50'$ , due south from a shoal whose longitude is now known to be  $122\frac{1}{4}^{\circ}$ , east. From this place they ran along the shore N. E. by E. twelve leagues, till they came to a convenient place for their purpose. Dampier gives a particular description of the country and inhabitants. Vlaming, another Dutch Captain, surveyed a considerable portion of the west coast in 1696, being ordered to look out for the crew of a vessel which had been lost the year before, and which it was supposed might have been wrecked on that coast, and the crew saved. In 1699, Dampier visited the west coast of New Holland a second time, having been sent out in purpose on a voyage of discovery in his majesty's ship *Roebuck*. These are all the navigators that preceded Captain Flinders on the west coast.

The south coast of New Holland was discovered by Pieter Nuyts, a Dutchman, in 1627, and the coast was accurately laid down by him, from Cape *Leeuwin* to about east longitude  $132^{\circ}$ . The next person who visited it was Captain Vancouver, in 1791. He made the coast at Cape Chatham, in latitude  $35^{\circ} 3'$  south, and longitude  $116^{\circ} 35'$  east. He sailed along the coast east, and anchored in a sound to which he gave the name of King George the Third. Thence he sailed east as far as Termination Island, in east longitude  $122^{\circ} 8'$ . The French Admiral, D'Entrecasteaux, who was sent out in search of *La Peyrouse*, visited the south coast of New Holland in 1792; but did not proceed further east than Nuyts had done. His charts, as far as he went, are accurate and satisfactory. Thus 250 leagues of the south coast remained entirely unexplored when Captain Flinders commenced his voyage.

Van Dieman's Land was discovered by Tasman, in 1642, who was sent out by the Dutch expressly to determine how far south the continent of New Holland extends. Captain Cook sailed along the east coast of that continent in 1770, and ascertained the general outline of the coast. Morion, a French naval officer, visited Van Dieman's Land in 1772, Captain Furneaux in 1773, Captain Cook in 1777, and D'Entrecasteaux in 1793; but none of these navigators had been able to decide whether Van Dieman's Land was an island or part of the continent. After the British colony

was established at Port Jackson, various expeditions were sent from that place to ascertain the east coast of this country with precision; and in these expeditions, Captain Flinders, and Mr. Bass, surgeon of the *Reliance*, cut a most conspicuous figure. These two enterprising gentlemen sailed from Port Jackson in 1795, in a small boat only eight feet long, called the *Tom Thumb*, with a crew consisting only of themselves and a boy. They explored about 35 miles of coast, and ascertained the figure and value of several bays and openings. In 1797, Mr. Bass set out in a whale boat, with a crew of eight picked men, to explore the east coast. He sailed south, was absent above eleven weeks, and examined above 600 miles of coast; entering Bass's Straits, and making considerable progress in them, and satisfying himself that Van Dieman's Land was an island; though the evidence which he obtained was not sufficiently strong to satisfy others. In 1798, Captain Flinders and Mr. Bass were sent on purpose from Port Jackson, to determine the point. They sailed through Bass's Straits and round Van Dieman's Land, and thus put it beyond doubt that Van Dieman's Land is an island. Such was the state of our knowledge of the coast of New Holland when Captain Flinders's voyage was undertaken in 1801.

Captain Flinders sailed from Spithead on the 18th of July, 1801, in the ship *Investigator*, of 334 tons burthen, with a compliment of 88 men. The vessel had been a collier, and though old, was the best which the Admiralty could at that time spare for this service. After touching at Madeira, and stopping some time at the Cape of Good Hope, he made the coast of New Holland on the 7th of December. The part of the coast which he first saw was Cape Leeuwin, the south western and most projecting part of Leeuwin's Land. The highest hill lies nearly in latitude  $34^{\circ} 19'$  south, and longitude  $115^{\circ} 6'$  east. From this port he coasted along the shore till he came to King George the Third's Sound, where he put the ship in order, and took in wood and water. Bald Head, the entrance to this sound, lies in latitude  $35^{\circ} 6' 15''$  south; and in longitude  $118^{\circ} 0' 45''$  east. It was from this point that he was instructed to make a minute survey of the south coast of New Holland as far as Bass's Strait.

From King George the Third's Sound to about east longitude 131, a space of 15 degrees of longitude, the coast runs easterly with a slight inclination to the north; and the whole of this space consists of a cliff between 5 and 600 feet high, the lower part of which is white and the upper part brown. Not a single river runs into the sea in all this vast tract, nor was there a single landing place to be seen. Captain Flinders conceives that this long cliff may be a coral reef, elevated by some unknown means so much above the level of the sea; and thinks it not unlikely that an island, sea, or lake, may be on the other side of the cliff. It is much to be regretted that this conjecture was not verified by an attempt to climb the cliff, and see what was on the other side of

it. About longitude 131, which is the furthest north point of the south coast of New Holland, the shore becomes low, and constitutes a sandy beach. Thus far east the south coast had been surveyed by Admiral Entrecasteaux.

From this place the coast assumes a south-easterly direction, is usually sandy and low, but rises gradually as the land recedes from the sea. There was no appearance of any river, though several bays occurred, and different archipelagos of small islands were passed, as Nuyts, Investigators, &c. At south latitude  $35^{\circ}$ , and east longitude  $136^{\circ}$ , they came to a point of land called Cape Catastrophe, because here Mr. Thistle, the master, a midshipman, and six seamen were lost by the oversetting of a boat in the dark. At this point the land tended to the north, and there was every appearance of a large opening, or bay, which Captain Flinders resolved to examine with every possible care.

This gulf, to which he gave the name of Spencer's Gulf, is about 50 miles wide at its entrance, and extends north about 183 miles, from latitude  $36^{\circ}$  south to latitude  $32\frac{1}{2}^{\circ}$ . It gradually narrows towards the north, and where it terminates the water is as salt as out at sea. Yet a good deal of fresh water must be poured into it here at certain seasons. The shore has a barren appearance on the east side; towards the north there is a ridge of mountains, the highest of which, Mount Brown, seems to be about 3000 feet above the level of the sea. The view from this mountain is that of an immense level country covered with wood. At the south side of Spencer's gulf there is a large island called Kangaroo Island, from the great number of kangaroos found on it. The strait between this island and the main land was called Investigator's Strait. Spencer's Gulf is separated from another gulf by a peninsula called Yorke peninsula, about 30 miles broad. This new gulf received the name of St. Vincent's Gulf. It is shorter than Spencer's Gulf, not reaching quite so far north as latitude  $34^{\circ}$ .

After leaving this gulf Captain Flinders met with Captain Baudin in the French ship *Geographe*, who was likewise out on a voyage of discovery. He had sailed through Bass's Straits, and had met with foul weather, during which he separated from his consort. Both here and afterwards in Botany Bay, the French captain and his officers admitted that Captain Flinders had been the discoverer of Spencer's and St. Vincent's Gulfs, Kangaroo Island, &c. But when an account of the French voyage was afterwards published in Paris by M. Peron, all this coast was claimed as the exclusive discovery of the French Captain, and named *Terre Bonaparte*, *Golfe Napoleon*, *Golfe Josephine*, &c. This we have no doubt was by the express orders of the then existing French government; was of a piece with that systematic course of fraud and falsehood which Bonaparte uniformly followed during the whole time of his administration. We have no doubt that the French nation will now give up these unwarrantable and absurd claims; and that in the account of Captain Baudin's voyage, just published

at Paris, a true statement of the real discoveries of the French captain will be given, while those that really belong to Captain Flinders will be assigned to their true author.

After parting with Captain Baudin, the Investigator sailed nearly in a south-easterly direction, along about 50 leagues of coast which had been examined by Captain Baudin. It was a low, barren, sandy shore, without a single river, opening, or landing place of any kind. It terminated at Cape Buffon, in south latitude about  $37^{\circ} 40'$ , and east longitude about  $140^{\circ} 5'$ .

The next portion of coast to Bass's Straits had been explored by Captain James Grant, in the *Lady Nelson*, in 1800. Except Port Philip, a very large bay about 36 miles long and 30 broad, with a narrow opening of not more than two miles wide; and Western Port beside it, which had been examined by Mr. Bass in his whale boat expedition, the remainder of the south coast presented nothing remarkable. Captain Flinders now made the best of his way to Sidney Cove. Here he remained twelve weeks to refit and provision the ship. It was then agreed between him and Governor King, that he should sail north and examine some part of the east coast of New Holland, Torres Straits, and the Gulf of Carpentaria.

He sailed from Port Jackson on the 22d of July, 1802, with the Brig *Lady Nelson*, commanded by Lieutenant Murray, in company. As he had already surveyed a portion of this coast in the *Norfolk*, his examination of it did not properly begin till he had passed Hervey's Bay, in latitude  $25^{\circ}$ , south. In latitude  $23^{\circ} 50'$ , longitude east  $151^{\circ} 20'$ , he discovered a harbour behind an island, called Facing Island, to which he gave the name of Port Curtis. It communicates by a narrow shallow channel with Port Keppel on the north side, discovered by Captain Cook; thus cutting off a considerable portion of land as an island. The country round Port Keppel was the best they had seen since they left Port Jackson. The natives were stout, healthy, and apparently very good natured; for they conducted a master's mate and seaman, who had put themselves entirely in their power, in safety back to the ship.

In latitude  $22^{\circ} 5'$ , a new port was discovered and examined, to which Captain Flinders gave the name of Port Bower. Shoal Water Bay, on the north of this port, but running as far south, and separated from it by a narrow strait, was next examined, and an accurate chart of it made out. This place affords but few temptations for a colony. The land is barren and the water shallow. Pines are found here as well as at Port Bower; but in the latter place they are much more easily got at.

The next object of examination was Broad Sound, which lies a little to the north-west of Shoalwater Bay. Here the tides rise to the height of above 80 feet. The best anchoring place is Upper Head, where a ship may lie secure against all winds; and this is almost the only place in the whole sound where a landing may be easily effected. The country round the sound is tolerably fertile,

and well wooded. Captain Flinders thinks it might be taken advantage of for ship-building.

Our navigator now directed his course for Torres's Straits; but was entangled for 14 days among the extraordinary coral reefs, which extend for 14 degrees of latitude along the east side of New Holland, from south latitude  $22^{\circ} 50'$  to Torres's Straits. At first they are at a considerable distance from the land; but they gradually approach it as we go north, and a little beyond Cape Tribulation they touch it. Captain Flinders made his way at last through an opening in about latitude  $18\frac{1}{4}^{\circ}$ , south, and met with no further interruption on his way to Torres's Straits. Through these straits, previously considered as so formidable, he passed without any accident, in three days, by keeping more to the south than former navigators: and there is every reason to consider them as affording the shortest, and a sufficiently safe passage to the southern ocean.

The survey of the Gulf of Carpentaria occupied Captain Flinders 105 days. Its general form is pretty similar to that represented in the old Dutch charts. It extends in longitude from Endeavour's Straits to Cape Wilberforce  $5\frac{1}{2}^{\circ}$ , and in latitude  $7^{\circ}$ . The east side of it consists of very low flat land without a single opening; but at the southern extremity, and along the west coast, there are various groups of islands, which were carefully surveyed by our navigator, and laid down in his chart. While in this gulf the Investigator was examined, and found so much decayed as not to be reckoned capable of keeping the sea for more than six months. Of these, three were spent in the Gulf of Carpentaria. Captain Flinders was therefore under the necessity of terminating his survey of New Holland here. He sailed to Copang Bay, a Dutch colony in the Island of Timor, in order to obtain a supply of provisions. From this place he sailed round New Holland by the west and south coasts, and arrived in safety at Port Jackson. Here the Investigator was condemned as utterly incapable of repair, and Captain Flinders resolved to return to Britain for another ship, to enable him to continue his survey, and to leave the scientific gentlemen at Port Jackson till his return. The rest of the voyage is little else than a history of disasters.

The command of the Porpoise, his Majesty's armed vessel at that time lying in Port Jackson, was given to Lieutenant Fowler, first of the Investigator, with a crew of 38 men; and Captain Flinders, with such of his officers as chose it, went on board her to be conveyed to England as passengers. Two vessels lying at that time in Port Jackson, the Bridgewater, Extra East Indiaman, commanded by Captain Palmer, and the Cato of London, commanded by Mr. John Park, desired to accompany the Porpoise and were permitted. These vessels sailed from Port Jackson on the 10th of August, 1803, and on the 18th, in the evening, the Porpoise suddenly struck upon a reef and fell over. The Cato and Bridgewater were coming up in such a direction that in a few moments they must have struck against each other, and been both sent to the bottom. The Cato, to prevent this, generously allowed

herself to fall upon the reef. The Bridgewater thus escaped her by a few feet, and got safe out of the reach of the reef. The night was dark. It was uncertain how long the vessels would hold together, and the only chance of safety seemed to be in holding out till morning, when it was fully expected that the Bridgewater would come and take the crews aboard. The cutter was suddenly let down from the Porpoise; but it was supposed to be broken by the violence of the surf. Another boat was let down, and Captain Flinders swam to it, in order to get on board the Bridgewater, and inform Captain Palmer of the situation of the vessels; but the Bridgewater was sailing away, and Captain Flinders found that, with the wind against him, and only two bad oars, it would be impossible to make his way to that vessel. He determined, therefore, to remain near the Porpoise till morning. He found the cutter safe and full of men, without any officers. He told them to remain near him till day-light. Fortunately the Porpoise had fallen over towards the sand bank, so that the sea did not beat over her, and she remained together without being stove in till morning. The case was different with the *Cato*. She had fallen towards the deep water. Her upper works were immediately beaten in and destroyed, and every thing except the men washed overboard.

Next morning a dry sand bank with birds' eggs upon it, indicating that it was never covered by the tide, was perceived at no great distance from the Porpoise. Here they agreed to land, and to take on shore as many stores as possible from the Porpoise. The *Cato's* crew made their way by swimming to the Porpoise, and only three lads were drowned; but most of them were in their shirts, and they had to be supplied with clothes by the officers and men of the Porpoise. On this sand-bank they all landed. Captain Flinders assumed the command as senior officer. Almost the whole of the provisions and stores in the Porpoise were landed, and the whole party brought under the same order and discipline as on board his Majesty's ships. The Bridgewater sailed away, without any attempt to relieve the shipwrecked vessels, or to ascertain whether any of the crew were left alive or not: and Captain Palmer, on his arrival in India, gave information that the two vessels were wrecked, and that all on board had perished. The Bridgewater was herself lost on her homeward voyage to Britain, and Captain Palmer never more heard of. This was, perhaps, fortunate for himself; for his situation, after the true history of the shipwreck was known, would have been most uncomfortable, supposing him possessed of any feeling.

Wreck Reef, as the place of the shipwreck was called, lies in latitude  $22^{\circ} 11' 23''$ , south; and in longitude  $155^{\circ} 18' 50.5''$ , east. It is distant from Port Jackson about 750 miles. Finding themselves abandoned by the Bridgewater, it was agreed in a consultation of officers, that one of them should endeavour to make his way in one of the boats to Port Jackson, in order to procure the means necessary to convey them to a place of safety. Captain

Flinders was selected for this arduous undertaking, and he readily undertook it. Mr. Park of the *Cato* went with him as second in command. They had a crew of 14 men, and provisions for three weeks; so that they were rather too deeply loaded. However, they made their way without any accident, first to the coast of New Holland, and then along that coast to Port Jackson, in 13 days.

Governor King sent the ship *Rolla*, and two schooners, to bring away the unfortunate men of the wreck. One of the schooners was to bring back such as chose it to Port Jackson; the other, of 27 tons burthen, under the command of Captain Flinders, with a crew of ten men, was to proceed to England; while the *Rolla* was to carry the rest of the officers and men to China. Captain Flinders reached Wreck Reef six weeks after he had left it; the stores and provisions of the *Porpoise* were put aboard the *Rolla* or the Port Jackson schooner; the men were all embarked to their various destinations; and Captain Flinders set out on his extraordinary voyage to England.

Sailing through Torres's Straits he arrived at Coepang in Timor, only four days after Captain Palmer, in the *Bridgewater*, had reached Batavia; thus demonstrating the great advantage of sailing through that strait, when compared with the round about way usually followed. From Timor he was obliged to make his way to the Mauritius, because his little vessel could not venture round the Cape of Good Hope without being repaired. He trusted to his French pass, that even if the war should have recommenced, he would be treated in a friendly manner in that island, and be allowed to continue his voyage to Britain. The Governor of the Mauritius was General De Caen, who had been sent out to Pondicherry at the peace of 1801; and, no doubt, expected to make a great figure in India; but the breaking out of the new war in 1802 disappointed his expectations, by depriving the French of all their possessions in India. The *Geographe*, a French vessel, out on a voyage of discovery, with an English pass, had left the Mauritius on the very day that Captain Flinders reached it; and, contrary to the stipulations contracted by obtaining the English pass, had carried dispatches from the Mauritius to France. This induced General De Caen to detain Captain Flinders for some days. At first he pretended that he was an impostor, and treated him with haughtiness and vulgar insolence; but finding that the plea of impostor could not be continued, he next day, without making any previous apology for his former conduct, invited Captain Flinders to dinner. This invitation Captain Flinders declined, in consequence of the Governor's previous treatment of him. De Caen, it would seem, was offended at this display of spirit; and Captain Flinders's letters to the Governor, though very naturally drawn from him by the situation in which he was placed, contributed considerably to heighten the Governor's resentment. Captain Flinders, at the time, does not seem to have been fully aware of the character of

Bonaparte, or the maxims of his government : otherwise, he would not have expected any redress on account of the justice of his cause, or any shame in his adversaries, on account of the cruelty, injustice, or meanness of their conduct. The Cumberland was seized, Captain Flinders and Mr. Aken committed close prisoners in a tavern, and the crew confined in the common prison. He was afterwards sent to the Garden prison, a large house about a mile out of town, attached to which was a piece of ground for walking. Here he remained 20 months. He was then permitted on his parole to reside in Wilhem's plain, in the highest part of the island, with a range of two leagues all round. All the exertions that were made by the Governor General of India, Lord Bentinck, the naval commanders, the men of science in the Mauritius, the President of the Royal Society, were in vain. Even an order from Bonaparte did not produce the desired effect : De Caen probably assigning as a reason that Captain Flinders was too well acquainted with the island to be permitted to depart. At last, in 1810, when an immediate attack upon the island was expected, De Caen suddenly allowed Captain Flinders to depart, without any new order from France ; and even to go to the place from which the attack was expected. Captain Flinders conceives that De Caen wished to get back to France, to share in the glory of his former companions in arms ; and that he set him at liberty, because he thought he might contribute materially to the capture of the island.

## ARTICLE XII.

### *Proceedings of Philosophical Societies.*

#### ROYAL SOCIETY.

ON Thursday the 26th of January, a paper by Mr. Hooke was read, On the Construction of Naval Charts. Ships are navigated within sight of land by means of naval charts ; in the open sea by means of charts of a particular construction, and by observations of the heavenly bodies. The paper was confined to the first of these. Naval charts at present cannot be used upon deck, without the risk of being destroyed. Mr. Hooke proposes to engrave them on stiff paper, and to varnish them well. He shows how the different problems requisite during a navigation along a coast may be solved ; but these solutions could not be sufficiently understood from hearing the paper read, to give any account of them here.

On Thursday the 2d of February, part of a paper by Dr. Watson Philips was read, On the Cause of the Motion of the Heart of Animals. Various opinions on this subject have been entertained by physiologists. Of late, M. Gallois has endeavoured to show that this motion depends entirely upon the spinal marrow, and im-

mediately ceases, when the spinal marrow is removed or destroyed. The motion of the heart, which continues for some time after it is removed from the body, he considers as similar to the motion of any other muscular body from stimuli; and the stimulus in this case is the arterial blood. Dr. Philips related a great number of experiments on rabbits and frogs, which appeared quite inconsistent with M. Gallois's hypothesis. Rabbits were rendered insensible by a blow on the occiput; the spinal marrow and brain were then removed, and the respiration kept up by artificial means. The circulation and motion of the heart continued as usual. When stimuli, as spirit of wine or opium, were applied to the spinal marrow or brain, the rate of the circulation was accelerated. When the hind legs of a frog are kept for two minutes in alcohol, the animal loses the power of motion. In this case it expresses great pain; but if tincture of opium be used instead of simple alcohol, little or no pain is expressed.

On Thursday the 9th of February, Dr. Philips's paper was concluded. He found that the peristaltic motion of the intestines continued after the brain and spinal marrow were removed. When the brain or spinal marrow were suddenly crushed, the effect upon the motion of the heart was much greater than when these organs were removed by cutting, or gradually destroyed by means of a wire. From these experiments Dr. Philips concludes that the action of the heart is independent of the brain and spinal marrow; but that it is capable of being affected by these organs. He conceives, with Haller, that the heart and muscles possess excitability independent of the brain and spinal marrow; that the difference between the voluntary and involuntary muscles depends upon the stimuli; and that animals possess three different sets of organs, the muscular, the nervous, and the sensorial, independent of each other, but capable of influencing each other.

On Thursday the 16th of February, a paper was read from Mr. Clift, describing experiments to ascertain the influence of the spinal marrow on the action of the heart in fishes.

At the same meeting a letter from Dr. Brewster was read, describing a new property possessed by the second surface of transparent bodies.

#### LINNÆAN SOCIETY.

On Tuesday the 7th of February, specimens exactly similar to Bovey coal, but brought from Constantinople, were exhibited to the Society by Mr. Sowerby. A communication by Mr. Sowerby was also read, On a Species of Fossil *Terrebratula*.

At the same meeting, two communications from Dr. Mitchell of New York were read. The first gave an account of a singular species of pleuronectes found in the New York rivers. Dr. Mitchell is inclined to consider it as a variety of the white-bellied pleuronectes; though the differences are considerable. Among others, the belly is nearly as dark as the back. The second paper con-

tained a description of a number of species of fish observed by Dr. Mitchell, and which are caught on the coast or in the rivers.

On Tuesday the 16th of February, a letter was read from Sir J. E. Smith to Mr. Macleugh, in which he shews, from a manuscript of the late Dr. Sibthorpe, that the *lignum rhodium* of Pococke, though not that of the ancients, is the *liquidamber styraciflua*.

At the same meeting, the conclusion was read of M. Vieillot's *Novi Systematis Ornithologici Prolusio*.

#### GEOLOGICAL SOCIETY.

*Dec. 16, 1814.*—Specimens from Maestricht from the Rev. E. Honey were presented, and a notice relative to the mountain of St. Pierre, near Maestricht, by the same, was read, and the thanks of the Society were voted for the same.

The mountain of St. Pierre is a hill about 150 feet high, which commences within a mile of Maestricht, and extends about three leagues in the direction of Liege. On the side next to the Meuse, it forms nearly vertical cliffs, and thus affords excellent sections of the strata, which are almost horizontal, having only a very slight dip to the north.

The lower beds are decidedly chalk; alternating at every two or three feet, with beds of flint nodules: the fossils of this chalk, though less abundant, appear to exhibit the same species as those which occur in the chalk of England. Above these are beds resembling the former in colour, but harder, and gritty to the touch.

Upon these lie a series of beds of calcareous free-stone, of which the mass of the hill is composed, and in which the extensive subterranean quarries are situated. This stone, in the quarry, is yellowish, and so soft as to be readily cut with a knife; but, by exposure to the air, it becomes both whiter and harder. Interposed between these beds are thin ones, composed chiefly of fragments of madreporites and shells. Beds of flint also occur here, as in the chalk; but the distance between each bed gradually increases, so that those at the top of the series are separated by an interval of eight or ten feet. The fossils of the free-stone are very numerous; the most common are madreporites, fungites, belemnites, nummulites, echinites, ostreites, and pectinites.

The top of the hill is covered by a bed of gravel, in some places of considerable thickness; containing rolled pebbles of flint, of quartz, of grey-wacke; with veins of quartz, and of red sandstone.

The whole of this series of beds, with the exception of the gravel, is considered by Mr. Honey to belong to the chalk formation.

The reading of Dr. Macculloch's paper on Glen tilt was begun.

*Jan. 6, 1815.*—The reading of Dr. Macculloch's paper on Glen tilt was continued.

#### ROYAL GEOLOGICAL SOCIETY OF CORNWALL.

Since our last report of this Society, we have to communicate

that his Royal Highness the Prince Regent has become its Patron; and, as this is the first instance in Great Britain in which the attention of the throne has been directed to the promotion of mineralogical science, it is to be fairly anticipated that it may lead our government to the establishment of a School of Mines; the great advantage of such institutions having been strikingly evinced in other nations.

It was resolved that a deputation should present an address of thanks to his Royal Highness, for the great honour conferred upon them; and that it should consist of the Vice Patrons, Lord De Dunstanville, and the Earl of Yarmouth; the President, Davies Giddy, Esq. M. P.; and its founder, Dr. Ayrton Paris.

Apartments have been provided at Penzance, which contain a collection of minerals already highly interesting: among the later additions we may notice *rutilate*, lately discovered in the slate quarries at Tintagel; a *grey copper ore* from Crennis mine, the composition of which resembles the *ful-erz*, with the exception of lead, (on the authority of the Rev. William Gregor.) *Wood tin* from Trethurgy Moor, near St. Austel, in a matrix of shorl and quartz. *The triple sulphuret of antimony, lead, and copper*, which has reappeared at the Antimony Mine, near Port Isaac, after a lapse of twenty years.—*Sulphate of barytes*, now found at Huel Unity, for the first time in Cornwall.—A large quantity of *stream gold*, presented by Sir Christopher Hawkins, with an interesting account of its discovery, in which he states that it was found in streaming for tin in a moor in the parish of Ladock; and offers some information, which he trusts may direct future adventurers to a successful undertaking.—Many other communications have been also read before the Society, an analysis of which we shall offer to our readers in the next number of the *Annals*.—We understand that the Society are preparing a volume of Transactions, which are shortly to appear.

### ARTICLE XIII.

#### SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

##### I. Lectures.

Mr. T. J. Pettigrew, F.L.S. will commence his Spring Course of Lectures on Anatomy and Physiology on Friday, the 10th of March, at half-past eight o'clock in the evening precisely, at his house, No. 3, Bolt-court, Fleet-street, where particulars may be obtained.

Dr. Clarke and Mr. Clarke will commence their next Course of Lectures on Midwifery and the Diseases of Women and Children on Monday, March 20. The Lectures are read at Mr. Clarke's . 10, Saville-row, Burlington Gardens, every morning from

a quarter past ten to a quarter past eleven, for the convenience of students attending the hospitals.

## II. French Agriculture.

The following account is given of the present agriculture of France, by Mr. Morris Birkbeck, in his Notes on a Journey through France, in July, August, and September, 1814, page 109.

"In the agriculture of France there is a great sameness. The arable land, which comprises almost the whole surface of the country, the vineyards and a few tracts of mountains excepted, may be divided into five classes, according to its fertility, without regard to the nature of the soil. The first bears a crop every year, as in Auvergne, in the neighbourhood of Thoulouse, in some parts of Normandy, &c. This description is highly cultivated, and on a principle well adapted to soil and circumstances. The second somewhat inferior in quality, but good land, is also judiciously cultivated, with the intervention of a fallow once in six years, as about Dieppe and Rotten. The third land of middling quality, which embraces a very large part of the kingdom, is managed on the old plan of fallow, wheat, oats. The fourth, poor land, which also covers a large space, is fallow and wheat alternately. The fifth, poor land, is cultivated in the round of fallow, rye, rest, without grass seeds. The first and second classes include what there is of variety and spirit in French husbandry. In the south, Indian corn alternating with wheat, exhibits management as good as the beans and wheat of the best English farmer: and the varied outline observable in the north, affords many proofs of a spirited and judicious culture. It is the three last which betray its weakness; if they comprise half the cultivated surface, which I believe is not over-rating their extent, half of that portion being fallow, it appears that one fourth of the whole country is lying in a state entirely unproductive; a few weeds, mostly thistles, excepted. A very few half starved sheep are kept to pick over the constantly recurring barren fallows, often accompanied by three or four long legged hogs. On the borders, and out of the way corners, you may see a cow or two, with an attendant; but there appears so little for any of these animals to eat, that you wonder how even they are supported. The prairies artificielles, (the artificial grasses, as we less properly call them,) of which so much is said by the amateurs, are like specks of green on a desert. Clover and lucern are cultivated with great success, on the two first classes of land; but very rarely indeed on the others. Thus there is probably as much really waste land in France as in England, and it is of an expensive kind; whereas our wastes support much more stock than theirs, without any expense whatever."

By Mr. Birkbeck's account, the labouring classes in the country parts of France are in much better circumstances, and receive much better wages than the labouring people in England. Most of them are proprietors of eight or ten acres of land, having been

enabled to make the purchase during the revolution. He considers their situation, their information and even their morals, as greatly improved since the revolution. Prices in France are about one half of what they are in England.

### III. *Dr. Cross's Opinions respecting the Cerebellum, &c.*

(To Dr. Thomson.)

MY DEAR SIR,

In the last number of your *Annals* I observed a paper On the Use of the Cerebellum, &c. containing some supposed new observations on the structure of the spinal mass of nerves, and on the function of its parts. Although I have no wish to detract from the originality of Dr. Cross's statements, yet I feel myself bound in justice to inform you that the same facts, or facts that lead to similar conclusions, are published in the following works:—*Lettres de Hufeland a Portal*, 1807. *Anatomie du Systeme Nerveux en general*, &c. par Gall et Spurzheim.

By inserting a notice of the contents of this note, you will much oblige, your obedient servant,

*British Museum, Feb. 3, 1815.*

W. E. LEACH.

### IV. *On the Petrifications in Plymouth Lime-stone.*

(To Dr. Thomson.)

MY DEAR SIR,

In the last number of your *Journal*, p. 150, Mr. Hannah is said to have discovered madreporites and shells in the lime-stone of Plymouth. I am induced, for two reasons, to trouble you with a very few lines on the subject. In the first place, madreporites were first observed in the Plymouth lime-stone about eight years ago (and a notice of this circumstance is given in vol. ii. p. 465, of *Annals of Philosophy*); but it is to Dr. Lockyer, of Plymouth, that we are indebted for the discovery of the exact bed in the lime-stone of that district in which the madreporites are found. The same Gentleman (Dr. Lockyer) also first observed turbinated univalves in loose blocks of lime-stone imbedded in earth on Stonehouse Hill, but shells have nowhere been observed in the lime-stone of Plymouth in situ. In the second place, the Rev. Mr. Hannah some time since showed me a specimen of lime-stone of a fine blood-red colour full of madreporites: at the same time he informed me that he found it at a place called the Devil's Point, Stonehouse; and as I had never observed this variety of rock near Plymouth, I lost no time in examining the point on the same day; and as the tide was out, and the place not very extensive, I may with confidence assert that no bed or vein of lime-stone similar to that shown me by Mr. Hannah occurs at or near the Devil's Point; but as vessels often throw out ballast near that spot, I have no doubt that the fragments he found came there in that manner.

*I should feel it necessary to apologize for troubling you, but for*

the statement respecting the discovery of the univalve in situ, which will I trust afford a sufficient excuse.

I am, dear Sir, your obedient servant,

British Museum, Feb. 3, 1815.

W. E. LEACH.

#### V. *The Caucasus.*

Engelhart and Parrot, during their travels in the Caucasus, ascertained that some of the peaks of that vast Alpine country are equal in height to Mont Blanc, in Switzerland.

#### VI. *Heliotrope.*

Dr. Macculloch, of Woolwich, has discovered heliotrope in the Hill of Kinnoul, near Perth. This is the second time that this rare mineral has been observed in Scotland.

#### VII. *Quantity of Paper at present made in the United States of America.*

	Tons.	Reams.	
For Newspapers* . . . .	500 . . . .	50,000 . . .	\$150,000
Books . . . . .	630 . . . .	70,000 . . .	245,000
Writing . . . . .	650 . . . .	111,000 . . .	333,000
Wrapping . . . . .	800 . . . .	100,000 . . .	83,000

#### VIII. *Burning Gas in the Appennines.*

(To Dr. Thomson.)

SIR,

In a preceding number of your Journal you have given an account of a "jet of burning gas," which you say was *discovered* by Sir H. Davy in the Appennines, and found by him to consist of carbureted hydrogen. In addition to this, you make some inquiries respecting the nature of the country, and the probability of the existence of coal among the Appennines.

Knowing the fact to have been confidentially communicated in a private letter written by Sir H. to this country, I confess I felt some surprise at seeing it appear in a public journal; nor was the defective manner in which the statement was made in your *Annals of Philosophy* calculated to diminish that ungrateful sensation.

The mentioning of this gas occurs in Sir H. Davy's letter as one of the many philosophical *observations* which that indefatigable inquirer has been constantly making during his travels through Italy, and from which the world will, I am confident, derive hereafter, whenever he shall think it proper to *make them public*, very great and useful information. As to the word *discovery*, it never once occurs in speaking of this subject; for Sir H. was perfectly aware that the phenomenon, known to all classes of persons travelling on the road from Florence to Bologna, had been particularly noticed

\* The number of newspapers printed annually in the United States is estimated at twenty-two and an half millions.

by Spallanzani, who had perceived the smell of hydrogen in the gas; then by your own countryman, Faber, in 1772; and since by Lalande and others. Nor has Sir Humphrey in his letter ever mentioned the height of the column of the gas, as stated by you, since this is greater or less according to the state of the atmosphere, which exerts also much influence on its combustion, it being more vivid at the approach of rainy weather.

But you were quoting from memory, and under circumstances unfavourable for publication. You may, therefore, probably, be anxious for some corrections which my knowledge of the country enables me to submit to your judgment.

The jet of gas alluded to is seen on the top of Monte di Fo, half a mile on the right of Pietramala, coming from Florence. The column occupies in circumference a surface of from ten to twelve feet. The ground around it is covered with large and small masses of primitive rocks, or rather of fragments of quartz and *mica argentine* held together by an argillaceous cement, and of limestone. Very feeble signs of vegetation are perceptible, either on this or the surrounding mountains. Monte Fo forms part of Mount Radicoso, or of the highest of the Appennine Ridge, being 883 metres, or 2901 English feet, above the level of the Mediterranean sea. About half a league from this gas-volcano, and from *Pietramala*, there is a well of cold water, called *Acqua buja*, from which there is a constant evolution of what I myself found to be carburated hydrogen, which takes fire at the approach of a lighted taper, and burns with a brilliant yellow flame. The Italian naturalists had hitherto considered these emissions of subterraneous gas, by them called *fumarole*, to be *sulphureted hydrogen*, the formation of which they derived from a decomposition of pyrites, that are found in great quantities at a considerable depth in the Appennine country. They fancied they discovered the sulphur, which the decomposition of the gas set at liberty, incrustating the objects surrounding the spot from whence arose the gas; and I confess having myself been of the same opinion during my last visit to the Appennines in the course of last summer. My examination, however, was very superficial, and too hasty to be correct. That of Sir Humphrey Davy is, of course, of quite a different character.

The Appennines consist chiefly of secondary rocks, amongst which lime-stone containing remains of marine animals is predominant. As they, however, approach the Alps, whence they took their origin, they participate more and more of their nature, and present for a long successive tract of ground a mixture of primitive rocks of various species, such as serpentine, argillaceous schist, grunstein, and saccharoid lime-stone. As to the existence of coals in the Appennines, Soldani, in 1780, in his Ornithographic Essay on the Nautilific Formations of Tuscany, mentions a considerable stratum of coal being discovered near Fiesole, a place at the distance of about 40 miles due south from *Pietramala*. Subsequent examinations, however, instituted for the purpose, and directed by very

able mineralogists, have not been followed by any result, from which one might be led to conjecture the presence of coal in any part of the Appennines, except some traces of it, which the lowest parts of the sub-appennine country offer, though of very little moment.

I have the honour to be,

Sir, your humble servant,

Brompton, Jan. 22, 1815.

A. B. GRANVILLE, M. D.

IX. *Answer to the Queries respecting Shell-Fish in a Moss near Elgin.*

(To Dr. Thomson.)

SIR,

On perusing the last number of your *Annals*, I observed a note signed C. T. requesting information respecting live shell-fish which are said to be found at the depth of three or four feet in the solid body of a moss near Elgin, in Murrayshire.

Though extremely sceptical of the existence of this circumstance, I addressed a letter to the Rev. Wm. Leslie, Minister of St. Andrews, near Elgin, a Gentleman well known for the accuracy of his observation, and zeal in investigating whatever is interesting, requesting he would inform me if there was any foundation for such a report. Mr. L. has kindly favoured me with an account of the different mosses in the vicinity of Elgin, with whatever appeared worthy of notice respecting them. He assures me there is not the least measure of reality, or any kind of foundation for the report alluded to, as no trace of shell-fish, either dead or alive, were found in any of the mosses near Elgin, nor to his knowledge in any other mosses in the adjacent country.

I was some time ago informed by Mr. Hughes, the Gentleman who superintended the operations in draining the lake of Spynie, in the vicinity of Elgin, that a few live fresh-water muscles, and some belemnites, were found in the bed of the lake. The existence of live shell-fish and petrified tangles (as the belemnites were termed) in fresh water was considered by many who saw them as a very extraordinary circumstance; and it is not unlikely but a continuing increase of exaggeration in the detail of this circumstance may have sunk the live shell-fish three or four feet into the solid bed of the lake.

I am, Sir, your most obedient servant,

Inverness, Jan. 21, 1815.

J. J. NICOL.

X. *Notice of a remarkable Meteor which appeared on the 2d of December, 1814.*

(To Lake Howard, Esq.)

SIR,

George Yard, Lombard-street, Dec. 14, 1814.

At your request I now give you the best account I am able to afford of the late luminous meteor.

On Friday night, the 2d of Dec. at about 20 min. before 11, I was walking in an open part of the village of Peckham, about four miles S.S.E. of London. The night was cloudy and dark, the lower part of the atmosphere clear and calm, a *very slight* wind blowing from the E. Suddenly I was surrounded by a great light. I remember that at the instant I shrunk downward and stooped forward; as I was apprehensive of some danger behind me, I instantly ran a few paces. I turned about in a few seconds to the N. E.; for I was certain the light came from that part of the heavens (as it brilliantly illuminated some houses to the S. W. of me); and I think at a considerable height from the horizon. But I saw nothing to cause this light. It did not give me the idea of the force and intensity of lightning; its brilliancy was not so instantaneous and fierce; but it was a softer and paler kind of light, and lasted perhaps three seconds. I could discover no noise, though immediately I expected an explosion.

The strength of the light was nearly equal to that of common day-light; all near objects were distinctly visible. The light very much resembled that of the luminous balls thrown from a sky-rocket when it finally explodes. It was not as vivid and blue as that of a strong flash of lightning at night. None of the persons I met that night thought it to be lightning, though none of them saw any thing but the light. Unless it had been very high, if it was in the direction I have supposed, it would be concealed from them by a high wall and some houses.

A relation of mine, who resides at the northern extremity of Tottenham, saw the light of it as he lay in bed, through a window facing the west. He describes it to have been as light as day.

I am, Sir, respectfully yours,

JOHN WALLIS.

### XI. *Weather at Calcutta.*

During the winter months at Calcutta there is always a fog every evening and morning. In the evening it rises high enough to cover the ground floors of the houses; but the upper stories are still visible. On this account the ground floors are considered as unhealthy, and are never inhabited by Europeans; but employed either as warehouses, or as lodgings for black servants. In the morning the fog is still more intense, and rises higher, so as to conceal the sun from view an hour and a half after his rising. When the sun sets through the fog he assumes a beautiful vermilion colour. The fog itself acquires the same tint. It is reflected from the water of the river; so that all nature appears painted of the finest red imaginable.

### XII. *On the Mode of conveying Gas for Lighting the Streets.*

(To Dr. Thomson.)

SIR,

In answer to your Correspondence respecting the manner in which

the Gas Light Companies force the gas through the pipes, I beg to inform him that, from its very compressible nature, and the number of collateral branches from the main to the lamps, forming so many outlets, that it is sent through the pipes with a very small pressure: I believe of not more than equal to a column of water of one inch in height. Should you consider the above worth inserting, I shall send you a paper on the structure of the cells of wasps and bees, which I consider so different as to require a particular explanation.

I remain, Sir, yours truly,

R. W.

### XIII. *Mr. Accum's Treatise on Gas Light.*

Mr. Accum has in the press a Treatise on Gas Light, exhibiting a summary description of the apparatus and machinery best calculated for illuminating houses, streets, and public edifices, with carbureted hydrogen, or coal gas; together with remarks on the utility, safety, and general nature of this new branch of civil economy.

The treatise will be illustrated with geometrical and perspective designs, exhibiting the larger gas light apparatus now successfully employed for lighting the streets and houses of this metropolis, as well as the smaller apparatus used by certain manufacturers and private individuals, together with other objects connected with this new art of illumination.

### XIV. *Metallization of Charcoal.*

Mr. Dobereiner, Professor of Chemistry at the University of Jena, in Saxony, informs Mr. Accum that he has discovered charcoal to be a metallic substance. The following statement has been received from Mr. Dobereiner by Mr. Accum:—

“ I hope soon to be able to communicate to you the successful metallization of charcoal, which I have reason to believe to be a metallic substance; in cast-iron and in steel the metal which it contains is present in a metallic state, and may be separated from both of them by the united action of phosphorus and an alkali.”

Farther particulars concerning this subject Professor Dobereiner promises to communicate in his next.

### XV. *On the Octohedral Form of Iodine.*

(To Dr. Thomson.)

DEAR SIR,

I regret that I was not aware of your intention to notice my observations on the crystals of iodine, as I would have undertaken to define with more precision the primary form from which all its other modifications may be readily inferred.

You have stated, (*Annals* for Jan. p. 12,) that it assumes an octohedral form; which, though true, is liable to be misinterpreted; since the crystal may be supposed equilateral and equiangular, unless its peculiar deviation from the regular octohedron be specified.

The axes of the octohedral crystal of iodine instead of being equal, are to each other in the proportion of the numbers 2, 3, and 4. so nearly that, in a body so volatile, it is scarcely possible to detect an error in this estimate by the reflective goniometer, because the surfaces change by evaporation too rapidly for any precise measurement of their mutual inclination.

The form which most frequently appears in a section of this octohedron parallel to the plane of its greatest and least axes, presenting to view a rhombic plate bevelled at each of its edges by two narrow planes, which are inclined to each other at an angle of about  $120\frac{1}{2}^{\circ}$ .

From the frequent occurrence of this rhombic plate, (of which the acute angle is about  $53^{\circ}$ ;) some crystallographers may be disposed to regard this surface as the terminal face of a rhombic prism, of which the diagonals are 2 and 4, and its height 3; and it is evident that all the modifications of the octohedron may with equal facility be derived from this prism.

I remain, Dear Sir, with great regard,  
Your obliged and obedient Servant,

FEB. 22, 1815.

WM. H. WOLLASTON.

P. S. In the same Number of your *Annals*, p. 75, you have noticed the discovery of chromate of iron in America, crystallized in regular octohedrons found in serpentine.

Permit me to add, that this rare mineral, which I believe has not hitherto been observed in Britain, occurs also in the Serpentine of Portsoy, having the same crystalline form, and tinging the adjacent rock of the usual green colour of chrome.

## ARTICLE XIV.

### New Patents.

LÆGER DIDOT, Paddington, Middlesex; for certain improvements in the method or means of illuminating houses or places, by the combination of tallow or other inflammable materials. Nov. 10, 1814.

WILLIAM BENECKE, Deptford, Kent; for the improved method of manufacturing verdigris, of the same quality as that known in commerce by the name of French verdigris. Nov. 12, 1814.

HORACE HALL, Golden-square, London; for an improved method of preparing and spinning hemp, flax, and other substances. Communicated by a foreigner residing abroad. Nov. 17, 1814.

EDWARD MASSEY, Coventry; for his chronometers and pocket watches. Nov. 17, 1814.

ROBERT BARLOW, Francis-street, Southwark, surgeon; for a machine or instrument called the *hydrostatic self-blowing machine*. Nov. 22, 1814.

## ARTICLE XV.

## METEOROLOGICAL TABLE.

1815.	Wind.	BAROMETER.			THERMOMETER.			Evap.	Rain.
		Max.	Min.	Med.	Max.	Min.	Med.		
1st Mo.									
Jan. 2	N E	30.45	30.42	30.435	35	32	33.5		
3	N E	30.42	30.16	30.290	33	30	31.5		
4	N	30.16	30.06	30.110	33	30	31.5		
5	N E	30.06	30.05	30.055	31	30	30.5		
6	N	30.05	29.95	30.000	34	25	29.5		
7	N W	29.95	29.48	29.715	35	24	29.5		
8	W	29.99	29.48	29.735	34	25	29.5		
9	Var.	30.02	29.76	29.890	42	26	34.0		
10	N W	29.76	29.52	29.640	44	35	39.5	.13	
11	N W	29.88	29.52	29.700	40	32	36.0		
12	N W	30.15	29.88	30.015	40	24	32.0		
13	S W	30.15	29.75	29.950	40	28	34.0		.28
14	N	30.17	29.75	29.960	43	32	37.5		
15	N E	30.30	30.17	30.235	36	27	31.5		
16	Var.	30.25	30.20	30.225	37	30	33.5		
17	N W	30.25	30.13	30.190	41	32	36.5	.19	
18	N	30.13	29.96	30.055	36	27	31.5		
19	E	29.96	29.68	29.820	34	22	28.0		
20	N E	29.80	29.77	29.785	28	26	27.0		
21	N E	29.80	29.75	29.775	35	29	32.0		
22	N	29.85	29.80	29.825	35	28	31.5		
23	Var.	29.85	29.73	29.790	37	17	27.0		
24	Var.	29.61	29.56	29.585	29	20	24.5		
25	N	29.56	29.41	29.485	30	22	26.0		
26	S E	29.41	28.96	29.185	89	23	31.0		
27	E	28.96	28.88	28.920	39	30	34.5		
28	S W	29.03	28.96	28.990	42	36	39.0		.57
29	Var.	29.24	29.03	29.135	43	34	38.5		
30	S E	29.29	29.22	29.255	41	35	38.0		
31	S	29.36	29.34	29.350	45	38	41.5	.17	.22
		30.45	28.88	29.770	45	17	32.66	.49	1.07

the observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash signifies, that the result is included in the next following observation.

## RESULTS.

*First Month, 1815.* — 2. Slight hoar frost: misty: cloudy. 3. Breeze fresh, and drier air: slightly clouded. 4. The same. 5. The same: a little snow. 6. The same. 7. a. m. A bank of *Cirrostratus* in the S. E.: during the forenoon these clouds passed over in flocks, and becoming denser at night, there fell a little rain or sleet. 8. A fine day, with *Cirrus*: bright star-light night. 9. Clear morning, but a *Cirrostratus* over the marshes, and *Cirrus* above in lines from N. E. to S. W.: at mid-day a little snow: windy evening. 10. *Cirrostratus* and *Cumulostratus*: *max.* of temp. for the day this morning. 11. Brisk wind: *Nimbi* to the S., succeeded by *Cumulus* and *Cirrus*: a little snow after dark. 13. a. m. Barom. falling: hoar frost: red lowering *Cirrus* and *Cirrostratus* clouds, in lines from N. to S.: these indications were followed by rain after two p. m. 15. Cloudy: smart breeze: S. E. in the night. 16. a. m. The sky overcast with *Cirrostratus*: this modification continued through the day, with a dry air. 17. Cloudy: windy night. 18—23. Snow fell at intervals during these six days, often in regular and beautiful crystallizations. 24. a. m. Much rime on the trees and shrubs: misty from *Cirrostratus*: temp.  $18^{\circ}$  about nine: wind easterly in the night. 25. Somewhat misty: overcast: snow. 26. Cloudy, a. m.: snow, p. m. in flakes of all sizes, varying from the simple union of six prisms in a minute star to broad feathery flakes of the most regular compound structure: it was nearly calm during this time; so that the crystals escaped the derangement consequent on being driven about in their descent. 27. Snow and sleet, p. m.: a thaw, interrupted by a little frost, in the evening: *max.* of temp. in the night. 28. Wind and rain in the night. 30. a. m. Misty: the trees dripping: rain.

## REMARKS.

Winds Northerly till near the close of the period.

Barometer: Greatest height..... $30\cdot45$  inches;  
 Least..... $28\cdot88$   
 Mean of the period..... $29\cdot77$

Thermometer: Greatest height..... $45^{\circ}$   
 Least..... $17$   
 Mean of the period..... $32\cdot66$

Evaporation,  $0\cdot49$  inch.

Snow and Rain,  $1\cdot07$  inch.

TOTTENHAM,

L. HOWARD.

*Second Month, 24, 1815.*

ANNALS  
OF  
PHILOSOPHY.

APRIL, 1815.

ARTICLE I.

*A Biographical Account of Sir Benjamin Thompson, Knt. Count Rumford.* By Thomas Thomson, M.D. F.R.S.

SIR BENJAMIN THOMPSON was born in the year 1752, in the little town of Rumford, in New England. His parents, who were in middling circumstances, gave him the best education the place could afford. He married, early in life, the school-master's daughter of the place; and I have been told, though I cannot vouch for the accuracy of the information, that he himself for some time discharged the duties of school-master. This is by no means inconsistent with the rank which he held, as Major of the Militia of this district; as in America the military officers did not constitute a separate profession, but were selected out of the most respectable inhabitants of the country, who still retained their old situations, and continued their old pursuits.

When the American revolutionary war commenced, Mr. Thompson embraced the side of the mother country, and was of considerable service, from his local knowledge of the country. He soon came over to London, with proposals to raise a regiment in America for the service of his Majesty; and such was his address and insinuating manners, that he acquired the confidence and friendship of Lord George Germaine, at that time at the head of the Colonial Department, who gave him, it is said, a situation in his office. He was afterwards sent over to New York to raise the proposed regiment, which he accomplished. In consequence of this, when the peace was concluded in 1783, he became entitled to half-pay. In 1784 he was knighted by his present Majesty.

He had been elected a Member of the Royal Society in the year

1779; and he began to distinguish himself as an experimental philosopher in 1781. In 1784 he made a tour on the Continent, and became acquainted with the present King of Bavaria at Strassburgh, who was at that time Prince of Deux Ponts. He insinuated himself so successfully into the good graces of this Prince, that he recommended him in the most powerful manner to his relation and predecessor the then Elector Palatine, who invited him into his service on the most honourable terms. He accepted the offers that were made him, and was employed by the Elector in new modelling the army, and in introducing various changes and reforms into different departments of government. He received a pension from the King of Bavaria amounting to about 900*l.* a-year, which constituted, I believe, almost the whole of his income. I have been told, though I do not believe the statement, that the Elector Palatine had applied to the British Government for a proper person to organize his army, and that Sir Benjamin Thompson was recommended by Ministry for that purpose. Be that as it may, he seems to have enjoyed the full confidence of the Elector Palatine; and he remained in Bavaria till 1799, when he returned to Great Britain; still, however, retaining his Bavarian pension.

During this long interval he had distinguished himself by various papers published in the *Philosophical Transactions*, on the Force of Gunpowder and on Heat, all of them written in the most pleasing and fascinating style, and containing much curious and important information. On his return to London in 1799 he began to make known those speculations which seem to have occupied a good deal of his attention during his residence in Germany. They regarded chiefly improvements in the construction of our fire-places, and in the mode of preparing food for the poor. His alterations in our fire-places constituting a real improvement; and Rumford grates, as they were called (for by this time Sir Benjamin Thompson had been created a Baron of the German Empire), very soon became almost universal in Great Britain. These improvements gave him a degree of popularity very uncommon among literary men in this country; so that for some time Count Rumford constituted the general subject of conversation.

1799 and 1800 were two years of very bad crops, which succeeded one another; the one from too much rain, the other from too long a continuance of dry weather. It was then that the prices rose to their present pitch; and they have never since fallen to what was formerly considered a reasonable rate. These high prices occasioned subscriptions in different parts for the relief of the poor. The Count's popularity enabled him to suggest a plan which he had put in execution in Germany, and to procure its adoption. Large quantities of soup were made, and distributed either by means of tickets, or sold at a very cheap rate. I had an opportunity of seeing this practised for two successive winters in Edinburgh upon a very considerable scale. But though the plan appeared good in an abstract point of view, it was not found upon trial to answer so well

as had been expected. Many of the greatest objects of charity were deterred by a false shame from making application for the soup, because they considered such an application as a declaration of their poverty before the whole town, and as sinking them a step lower in the scale of society. Those who came forward with the greatest effrontery were sufficiently poor indeed; but too frequently their characters were not of the best stamp, and not a few of them by a vicious course of life had lost that regard for character, and that desire of the good opinion of others, which constitute so important a part of the feelings of the common people in those countries where they have not been vitiated by improper institutions and erroneous laws. Though those who had the office of preparing the soup discharged their duty with great fidelity and honour, yet the poor people were not satisfied, but complained loudly against the quality of the soup, and the honesty of the purveyors.

These two years of scarcity seem to have directed the Count's attention to the art of cooking. Combining his notions of economy, and his opinions respecting heat, he contrived a new cooking apparatus by means of steam, which he assured the public was greatly superior in every respect to the old mode. Such was his popularity at that time, that numbers of people adopted his ideas, and fitted up their kitchens according to his models; but I have not heard that his scheme was found to answer in a single instance. I remember going in 1802 to see the Count's own kitchen, which was fitted up according to his own plan, and was at Brompton, or somewhere about Knightsbridge. I was very much surprised to observe that not one of the utensils had ever been put to use. Hence it was likely that his notions of cooking were rather theoretical than practical.

He had begun, soon after his arrival in London in 1799, to publish a series of essays in succession on different subjects; but all connected with his own favourite pursuits. These essays amount in all to 18 or 19, constituting two octavo volumes. They were exceedingly popular at their first appearance, and very generally read; and they contain many valuable facts, put together in a pleasing and entertaining manner.

It was at this time that he had influence enough to procure the formation of the Royal Institution as a school for science in the metropolis of Great Britain, and a place where models of every kind were to be collected and exhibited. This Institution has flourished ever since, and has given birth to various others upon a similar plan, both in London and in other towns of Great Britain and Ireland.

The uncommon popularity which the Count enjoyed for some years seems to have produced a bad effect upon his disposition, or perhaps rather induced him to display without reserve those dispositions which he had hitherto been at some pains to conceal. Pomposity, and a species of literary arrogance quite unsuitable to the nature of experimental philosophy, for some years characterized his

writings, and injured their value. But in some of the last essays with which he favoured the world we find much valuable and curious information, respecting the heat evolved by different combustibles while burning, a subject of great interest, which he prosecuted for many years, and at last elucidated with considerable success.

I pass over his quarrel with the managers of the Royal Institution, about the nature of which I am not fully informed, though I suppose it was an attempt on the part of the Count to retain in his own hands the entire management of that Institution. Be that as it may, the result of the dispute induced him to leave London, to which he never again returned. He settled at Paris, and some years after married the widow of M. Lavoisier, who had retained part of her unfortunate husband's property; but their tempers were not found to be congenial. Hence after some time they parted. He lived in a house at Auteuil, in the neighbourhood of Paris; and during the two or three last years of his life, his daughter, who had been brought up in America, but who came over to join him in France, lived with him in the same house. He died on Sunday, August the 21st, 1814, in the 62d year of his age. His writings, so far as I am acquainted with them, are the following:—

1. New Experiments on Gunpowder. *Phil. Trans.* for 1781. P. 229.—This is an elaborate paper. The experiments appear to have been made with great care. His method was similar to that contrived by Robins, and universally known. I can only give a general idea of some of the points which he established. He found, as Robins had done, that when the powder was rammed into the piece the effect was greater than when it lay loose, and therefore recommends the use of the ramrod in charging a piece. The force of the charge increased as the piece acquired heat by firing. This is so well known in the navy that, after firing two or three times, it is customary to diminish the quantity of powder used. Our author found that the barrel became much hotter when the piece was only charged with powder than when a ball was employed. He conceives the heat to be produced by the vibration of the barrel, and supposes that this vibration will be greatest when there is no ball, because in that case the action on the barrel is only momentary. Here we have the first notice of our author's peculiar notions respecting heat, which he retained during the whole of life. It appears from his experiments that the relative velocities of the bullets, supposing every thing else equal, are in the subduplicate ratio of the weights of the charges nearly. This is conformable to preceding experience. The position of the vent produces very little effect on the charge. He points out a method of proving the relative goodness of powder with great accuracy. But the method had been already introduced by Dr. Hutton, and was in use at *Woolwich*. It consists in having a standard powder, the velocity *with which* a particular charge of which drives a bullet of a given

size and weight is known. According as the powder under examination drives the bullet with more or less velocity than this powder, it is above or below the standard. From Robins's theory it follows, that when bullets of the same size, but of different weights, are discharged from the same piece by the same quantity of powder, their velocity is reciprocally as the square roots of their weights. But our author shows that there are circumstances which prevent this theory from holding good in all cases. The explosive force of *aurum fulminans* he found equivalent to about 307 atmospheres, or about  $\frac{1}{4}$  of that of gunpowder.\* The specific gravity of gunpowder he found 1.745. When well shaken, its weight, compared to the same bulk of water, was 0.937 to 1.000, and when lying loose as 0.836 to 1.000. He attempted to increase the force of gunpowder by mixing it with carbonate of potash, sal-ammoniac, and brass filings; but the effect was the reverse of his expectation. The swimming bladders of small fish filled with water or alcohol, and put into the middle of the charge, likewise diminished the effect very considerably.

2. New Experiments on Heat. Phil. Trans. 1786. P. 273.—

A thermometer surrounded with different mediums was plunged into boiling water, or into melting ice, and the times that elapsed during the heating or cooling were considered as indicating the conducting power of the different mediums inversely. The following table exhibits the conducting power of the different mediums tried according to these experiments:—

Mercury .....	1000
Moist air .....	330
Water .....	342
Common air, density 1 .....	80 $\frac{1}{2}$
Rarified air, density $\frac{1}{4}$ .....	80 $\frac{1}{2}$
Rarified air, density $\frac{1}{7}$ .....	78
The Torricellian vacuum .....	55

But before the conducting power of these substances can be concluded from these experiments, it would be necessary to take into view their specific heats, and likewise the radiation of heat through different elastic mediums.

3. Experiments on the Production of Dephlogisticated Air from Water with various Substances. Phil. Trans. 1787. P. 84.—

Dr. Ingenhousz had discovered that when the leaves of plants are put under water, and exposed to the rays of the sun, a quantity of oxygen gas is evolved; and Dr. Priestley had observed that when water became green it always yielded more of this gas than common water. On these experiments a theory had been founded that vegetables decompose water, retaining the hydrogen, and giving out the oxygen, and that by this process the oxygen taken from common

\* I conceive its force does not exceed  $\frac{1}{4}$ th of that of gunpowder.—T.

air by animals and by combustion is restored. The present experiments show that raw silk, eider down, and various similar bodies, may be substituted for the leaves of plants without diminishing the evolution of oxygen gas. The light of lamps was found to produce the same effect as solar light; but our author did not obtain gas by heat when light was excluded. He confirmed Dr. Priestley's observation, that green-coloured water produces much oxygen gas, but the green matter, according to him, is not of a vegetable nature, but consists of a congeries of animalcules. This subject of the evolution of oxygen gas by exposing various substances under water to the action of the solar rays is not yet cleared up. When our author wrote, chemical analysis was not far enough advanced to enable experimenters to determine exactly the nature of the gases evolved, and modern chemists have not paid any attention to the subject.

4. Experiments to determine the positive and relative Quantities of Moisture absorbed from the Atmosphere by various Substances under similar Circumstances. Phil. Trans. 1787. P. 240.—The following table exhibits the result of these experiments :—

Substances.	Weight after being dried 24 hours in a hot room.	Weight after being exposed 48 hours in a cold uninhabited room.	Weight after being exposed 72 hours in a damp cellar.
Sheep's wool .....	1000	1084	1163
Beaver's fur .....	1000	1072	1125
The fur of a Russian hare .....	1000	1065	1115
Eider down .....	1000	1067	1112
Silk { Raw, single thread .....	1000	1067	1107
{ Ravellings of white taffety .....	1000	1054	1103
Linen { Fine lint .....	1000	1046	1102
{ Ravellings of fine linen .....	1000	1044	1092
Cotton wool .....	1000	1043	1099
Silver wire, very fine, gilt, and flattened, being the ravellings of gold lace.... }	1000	1000	1000

5. Experiments on Heat. Phil. Trans. 1792. P. 48.—The object of these experiments is to determine the conducting power of various bodies. He surrounded the bulb of a thermometer with various bodies inclosing the bodies and bulb in a glass ball. The thermometer was raised nearly to a boiling heat, and then plunged into ice-cold water. The time of cooling a certain number of degrees was noted, and the conducting power considered as inversely as this time. The following table shows the principal results :—

Air .....	576"
Hare's fur .....	1315
Eider down .....	1305
Beaver's fur .....	1296
Raw silk .....	1284
Sheep's wool .....	1118
Cotton wool .....	1046
Fine lint .....	1032

The finer the fibres were the worse a conductor of heat was the substance. He ascribes this to the attraction between the fine fibres and air, which prevents the air from moving out of its place, and thus carrying off the heat. In these experiments, likewise, the radiating power of different bodies is overlooked.

6. A Method of Measuring the comparative Intensities of the Light emitted by Luminous Bodies. *Phil. Trans.* 1794. P. 67.— The method is to make the luminous bodies compared shine on a sheet of paper in the middle of which is placed a small cylindrical body. This cylinder will project two shadows, one illuminated by one of the luminous bodies, the other by the other. The distance of one of the lights is to be varied till the two shadows are of equal intensity. The light emitted by each body is as the square of its distance from the cylinder. This apparatus our author called a Photometer. It is nearly similar to the method long before employed by Bouguer, of which Count Rumford was probably not aware, as he takes no notice of it. The facts ascertained by Count Rumford by means of this apparatus were the following:—No perceptible quantity of the light is absorbed during its passage through air. When light passes through fine mirror glass about  $\frac{1}{4}$ th is absorbed. About  $\frac{1}{3}$  is lost when light is reflected from a good plane glass mirror. A good Argand's lamp gives as much light as eight wax candles  $\frac{1}{2}$  inch in diameter. The quantity of oil consumed in an Argand's lamp to that consumed in a common lamp to produce the same light is 15 per cent. less. The light of a candle fluctuates much more than that of a lamp. A tallow candle newly snuffed gave out a quantity of light = 100; in 29 minutes the light was reduced to 16. On being snuffed, it recovers its original intensity. The following table exhibits the weights of different substances, which must be consumed in order to produce the same quantity of light:—

	Weight.
Bees' wax. A good wax candle kept well snuffed, and burning with a clear bright flame .....	100
Tallow. A good tallow candle kept well snuffed, and burning with a bright flame .....	101
The same tallow candle, burning very dim for want of snuffing .....	229
Olive oil burnt in an Argand's lamp .....	110

	Weight.
Olive oil burnt in a common lamp, with a clear bright flame without smoke .....	129
Rape oil. Burnt in the same manner .....	125
Linseed oil. Likewise burnt in the same manner .....	120

But it is necessary to observe that these experiments were afterwards repeated by M. Hassenfratz, who obtained a very different result. He found that more oil must be consumed to produce the same quantity of light in an Argand's lamp than in a common lamp.

7. An Account of some Experiments on Coloured Shadows. Phil. Trans. 1794. P. 107.—It is well known that when two shadows are cast upon paper, one illuminated by the light of day or of the moon, the other by the light of a candle, the first appears blue, the second yellow. Count Rumford observed this by accident, and he was induced to make a set of experiments on the subject. He found that the two colours could be produced by two candles by interposing a pane of yellow or blue glass before one of them. When two rays of light from different parts of the heavens on a windy day were made to illuminate two shadows, a most fascinating succession of colours took place, all perfectly harmonious, and exhibiting every possible variety. He found that the blue colour was merely an optical deception, being always produced when the other shadow was yellow. But when he surveyed the blue shadow alone through a long tube which excluded from sight the yellow shadow and all surrounding objects, no blue colour was perceptible.

8. Experiments to determine the Force of fired Gunpowder. Phil. Trans. 1797. P. 222.—This is perhaps the most curious and interesting of all Count Rumford's papers. The experiments are very ingenious, and appear to have been well conducted; though the conclusions which he deduces from them are by no means satisfactory. Robins estimated the force of gunpowder at 1000 atmospheres; Daniel Bernouilli, at 10,000 atmospheres; and Dr. Hutton, at 2000. Count Rumford endeavours to prove in this paper that the force is equivalent to 50,000 atmospheres. But his mode of reasoning is not correct. Twelve grains of gunpowder were found to move a weight of 8800 lbs. out of its place. There is no difficulty in calculating the quantity of elastic fluid formed during the combustion of a given quantity of gunpowder, supposing the whole of it to be consumed. 100 grains of gunpowder contain usually about 67·3 grains of nitre. This quantity of nitre contains 35·8 grains of nitric acid, equivalent to 78 cubic inches of oxygen gas and 32 cubic inches of azotic gas, making a total of 110 cubic inches. Now as gunpowder is rather lighter than the same bulk of water, we shall not err much if we suppose 100 grs. of gunpowder to occupy the bulk of  $\frac{1}{4}$  of a cubic inch; so that the gas in the powder is equivalent to 330 atmospheres. This gas is

chiefly converted into carbonic acid gas, which does not alter the bulk of the oxygen. We do not know the heat generated by the combustion; but it cannot be less than  $1000^{\circ}$ . Such a temperature would just triple the elasticity of the generated gas, and therefore render it equal to 990 atmospheres; so that, according to this supposition, the calculation of Robins is exact. But probably the heat may considerably exceed  $1000^{\circ}$ ; and there is reason to conclude from Count Rumford's experiments that the sulphuret of potash which remains after the explosion of the gunpowder is at first in a gaseous state. All these causes must render the elasticity considerably greater than 1000 atmospheres. Probably Daniel Bernoulli's supposition is not very far from the truth.

The diminution of elasticity which took place when the elastic fluid generated from powder was prevented from escaping, and the formation of the hard stony substance, which astonished Count Rumford so much, is easily explained. The dissipation of the heat would speedily reduce the elasticity to one-third. Potash is capable of absorbing nine-tenths of its weight of carbonic acid. Therefore a considerable portion of the carbonic acid (the principal gas generated) would be absorbed, which would diminish the elasticity still farther. The stony body was a mixture of bicarbonate of potash and sulphuret of potash, the last of which would speedily absorb moisture from the atmosphere, and generate sulphureted hydrogen.—Count Rumford's notion that the elasticity of the elastic fluid from gunpowder is partly owing to steam is certainly erroneous. Nitre contains no water of crystallization. The charcoal that answers best for gunpowder is that which absorbs the least moisture. The drier the gunpowder is made, the stronger it is found to be. Hence it follows that moisture, instead of increasing, very much diminishes, the strength of gunpowder.

9. An Inquiry concerning the Source of the Heat which is excited by Friction. *Phil. Trans.* 1798. P. 80.—This is also a very curious paper. He found that by the friction of a steel borer against gun metal, pressing against it with a force of 10,000 lbs., while the gun metal turned round 32 times in a minute, 18½ lbs. avoirdupois of water were made to boil in two hours and a half. The heat produced was as great as would have been given out by nine large wax candles burning with a clear flame all the time. He showed that this heat was not owing to any change in the specific heat of the metal, nor was it derived from the air. As no source could be pointed out, he draws as a conclusion, that heat is not a substance, but mere motion. But such a conclusion is going rather further than the experiments warrant. There is nothing absurd in supposing that friction has the property of drawing heat continually from the surrounding bodies, just as it does electricity, though it is not in our power to explain how it produces this effect.

10. On the Chemical Properties that have been attributed to Light. *Phil. Trans.* 1798. P. 449.—When certain substances are

exposed to the rays of the sun they undergo remarkable changes. Thus the oxides of gold and silver, if in contact with combustible bodies, are reduced; chlorine and hydrogen gas explode, and form muriatic acid; water holding chlorine in solution emits oxygen gas, &c. Count Rumford suspected that these effects were occasioned solely by the heat evolved by the absorption of the light. The experiments related in this paper were instituted in order to determine the point. Though they cannot be considered as quite satisfactory, yet it seems established by the subsequent experiments of chemists, particularly of Gay-Lussac and Thenard, that the opinion entertained by Count Rumford on this subject is correct.

11. An Inquiry concerning the Weight ascribed to Heat. Phil. Trans. 1799. P. 179.—From an experiment of Dr. Fordyce, it was concluded that bodies become heavier the more they are cooled, and of consequence that heat diminishes their weight. But Count Rumford found, on repeating the experiment, that the supposed increase of weight was a deception, arising from vapour condensing on the surface of the glass vessel in which the experiment was made. Lavoisier had previously ascertained the same thing.

It does not seem necessary to give a particular account of the remainder of Count Rumford's writings. His two volumes of essays are of a very miscellaneous nature, and the most important of the essays are republications of those papers which have been already noticed. The seventh essay, in which the Count endeavoured to prove that fluids are nonconductors of heat, has been sufficiently refuted by the more decisive experiments of subsequent chemists. Indeed the Count himself, though abundantly obstinate, appears at last to have given up his opinion. The essays on the treatment of the poor, on cooking, on chimnies, and on the management of fuel, are not very susceptible of abridgment. His paper published in the Philosophical Transactions for 1804, entitled, An Inquiry concerning the Nature of Heat and the Modes of its Communication, gives us a number of curious facts respecting the effect of surface on the heating and cooling of bodies. But the publication of Mr. Leslie's book on heat, in which this subject is treated of at much greater length, and much more completely, have deprived this essay of most of its interest. It is not necessary to notice the papers published by the Count in Nicholson's Journal for 1806. An outline of his last paper, On the Quantity of Heat evolved during the Combustion of different Bodies, was given in the first paper in the third volume of the *Annals of Philosophy*, to which I beg leave to refer the reader.

## ARTICLE II.

On Dew. By Dr. Wells.

(To Dr. Thomson.)

SIR,

MY Essay on Dew has lately been honoured in the Quarterly Review with a Criticism by Dr. T. Young, the depth and variety of whose knowledge entitle him, perhaps, to be considered as the most learned man in this country. I mention his name thus openly, because I am confident, that he has too much of the spirit of an English gentleman ever to desire to conceal his being the author of any publication, in which he discusses the merits of a literary work of another private person. With respect to the criticism of my work, indeed, he clearly shows, that he is the writer of it, by the manner, in which he speaks of his own works at its close.

I am necessarily much pleased with the general commendation, which has been bestowed upon my Essay by one of his high rank in literature. As several of his observations, however, respecting it do not appear to me entirely just, I beg leave to make a reply to these through the medium of your Journal.

I. Dr. Young has called my theory a simple and *obvious* consequence of principles deduced from the discoveries, concerning heat, by Mr. Leslie, and other observers. On this point I shall offer a few remarks.

1. The Inquiry of Count Rumford, and the Essay of Mr. Leslie, were both published in 1804, and in these works are to be found all the new facts relating to heat, which I have taken from others in forming my theory of dew. Whether Count R. ever afterwards treated of atmospherical appearances is unknown to me; but Mr. L. published, nine years after his Essay, a work on heat and moisture, in which, agreeably to the opinion of Aristotle, the production of dew is attributed to the condensation, by the cold of the night, of watery vapour diffused through a considerable portion of the atmosphere. Now, when the great ingenuity of Mr. L. is considered, if the theory of dew, which I have proposed, be an *obvious* consequence of his own discoveries, it would assuredly have occurred to him, in that long space of time, since he has shown, that the subject of dew had in the meanwhile occupied his attention.

2. Your own various publications demonstrate, both that you are well acquainted with the modern discoveries respecting heat, and that you have attended closely to atmospherical appearances; yet I remember, that you asked me, at an accidental meeting, shortly before the publication of my Essay, what my opinion was on the formation of dew, giving as a reason, that you were yourself ignorant of its cause.

3. Even Dr. Young, though his Lectures on Natural Philosophy prove him to have been well skilled in the new doctrine of heat, has advanced in that work an opinion upon the cause of dew, in the most ordinary form of its occurrence, which has not the least connection with any modern discovery, as I shall more particularly mention hereafter.

I might proceed in this way considerably further; but what has been said is, I think, sufficient to establish, that, when regard is paid to the imperfection of the human understanding, my theory of dew is not an obvious consequence of the recent doctrine of heat. My explanation, indeed, of the *immediate* cause of dew is altogether independent of that doctrine, being grounded on the simple fact, that bodies always become colder than the neighbouring air before they are dewed, and was consequently open to the discovery of every person since the invention of thermometers. It is true, that the next step in my theory could not have been taken, without the assistance of the late discoveries of others, and this has been amply acknowledged in my Essay.

II. "Dr. Wells," says Dr. Y., "appears, in his historical account of the doctrines relating to the nature and causes of dew, to have undertaken to afford us complete information respecting the sentiments, not only of Aristotle and Theophrastus, but also of the "most distinguished" philosophers of modern times: some of the works, however, of the persons whom he mentions, and some of the latest, have most unaccountably escaped his attention." In this sentence there are two small mistakes, which, from my respect for the author, I must suppose unintentional, though they give point to his statement. The first relates to the engagement into which he regards me to have entered concerning the opinions of preceding writers on dew; for I never gave the slightest hint, that could lead him to the conclusion which he has made. I knew that my account of opinions on the cause of dew was incomplete; and well it might be, since the whole of it, with the accompanying refutations, does not occupy more than three pages of my Essay. But my health, at the time of its being drawn up, was in such a state, that I scarcely hoped that I should ever finish my work, and my notes were so written, that no person besides myself could make use of them. I composed therefore in haste, and had neither leisure nor strength to search public libraries for all the works, which I wished to consult. I certainly thought, however, that what I had collected contained every thing of much importance, which had been said upon my subject. The second mistake consists in his applying the words "most distinguished," which were used by me concerning the authors, who had given opinions on the formation of ice in India, to those who had treated of dew.

I pass now to more important matters. Dr. Y. in support of what he has said respecting some very late and important works, connected with my subject, having most unaccountably escaped my attention, gives a long extract from the "*Recherches sur la Cha-*

leur" of Mr. Prevost, published in 1792. This work was, indeed, unknown to me when I composed my Essay; but Dr. Y. I presume was ignorant, when he wrote his Criticism, that the very passages, which he has cited, are contained in another and much later work on heat by Mr. P. unaccompanied with any intimation, that they were copied from a preceding publication. Mr. P.'s later work, which was printed in 1809, was first seen by me in 1812, two years before my Essay came out. What appeared to me the most worthy of attention in the passages cited by Dr. Y. was spoken of in one of my notes, p. 79. I mentioned there, very distinctly, that Mr. P. had already accounted for the effect of clouds, in diminishing the cold of the air at night, by making this to depend upon their preventing the escape of its heat by radiation to the heavens; but only impliedly, that he had accounted, in this way also, for a similar effect produced by them upon the temperature of bodies on the surface of the earth, as I said only, that he did not seem to have known, that they have a much greater effect upon the temperature of such bodies, than upon that of the air. My full meaning was, that Mr. P. did not seem to know, that the degree of cold, which is prevented by clouds from appearing on the surface of the earth, is much greater, than that which they prevent from appearing in the air; or in other words, that he was ignorant, that, bodies on the surface of the earth become much colder than the air in a clear night, this being one of the principal facts, on which my theory of dew is built. That I had no desire to conceal any thing which Mr. P. had said upon this subject is shown, by my referring to the latest work, in which he has mentioned the effect of clouds upon the heat of the earth and atmosphere at night, and by my referring, likewise, in three different parts of my Essay, (p. 68, 74, and 118,) to Count Rumford as supposing, that the earth is cooled by radiation at night; since it cannot be thought, that, although unacquainted with both of those authors, I should withhold the knowledge of the possessions of one, and yet repeatedly speak of similar possessions of the other.

I have said in the preceding paragraph, that Mr. P. did not seem to know, that the earth ever becomes colder, at its surface, than the air by radiation. My reasons are, 1st, That he has not mentioned this fact: 2dly, That he has said what is equivalent to a denial of it in his late work, *Du Calorique Rayonnant*, p. 249: and 3dly, That it is apparently in opposition to an observation of his friend Mr. Pictet, from whom he seems to have derived all the facts which he has related in this disquisition on the effect of clouds; for that philosopher found, that, although, in clear and calm nights, the heat of the air decreased from the height of 75 feet above the ground to within four lines of it, yet a thermometer, lying upon the ground, and having its bulb slightly covered with earth, "*precisement enterrée*," was higher than all those which were suspended in the air above it.—*Pictet sur le Feu*, p. 180.

Considering the purpose to which Dr. Y. has applied his quotation from Mr. P., I shall venture to examine this a little closely.

1. What has hitherto been called by me Mr. P.'s *explanation* of the effect of clouds, at night, upon the temperature of the surface of the earth and of the lower atmosphere, is in reality only a *conjecture*; for, 1st, It is denominated by himself an "Essai d'Explication:" 2dly, It is supported by no experiment: 3dly, It takes as established, that air can radiate heat, whereas he says in p. 24 of his last work, "On peut supposer, que les molécules de l'air rayonnent."

2. Mr. P. mentions, that clouds send back to the earth a *little* more heat than transparent air can do; which is equivalent to affirming, that clouds alter the temperature of bodies upon the surface of the earth at night only a little. Nothing can show more strongly Mr. P.'s want of practical knowledge upon this subject; since I have given in my Essay, p. 32, an instance of the temperature of grass having risen  $15^{\circ}$  in less than 45 minutes, on the sky becoming cloudy.

3. It is remarked by Mr. P. that the operation of clouds, in keeping the surface of the earth warm at night, is exactly similar, "exactement comparable," to that of cloathing on the human body; and Dr. Y. assents to this observation. Now it appears to me, that very dissimilar things are here confounded together. Cloaths keep us warm by being bad *conductors* of heat; but clouds warm the earth by *radiating* heat to it. Hence, the effect of clouds is *immediate*, but cloaths require to be applied some time, before they completely answer their purpose. Cloathing must touch the skin to produce its full effect, whereas clouds produce theirs at the greatest distance. The thicker our cloathing is, the warmer it renders our bodies; but, if a cloud be sufficiently dense to prevent the heat, that is radiated into it, from passing through its interstices, the earth is kept as warm by it, as far as the principle of radiation is concerned, as it would be by one many times deeper. This is shown by a piece of cambric preventing the occurrence of cold on the earth's surface, from radiation, as effectually as a thick blanket.

4. Annexed to the disquisition of Mr. P., are two meteorological facts communicated to him by Mr. Pictet. One is, that upon a night in January, 1777, a thermometer, suspended in the open air, rose nearly  $3\frac{1}{4}^{\circ}$  of Fahr. in the space of an hour, the weather having in the mean time become cloudy. This was observed by himself; but it does not appear, that he afterwards made any similar observations. The second is given on the authority of husbandmen, who are said constantly to find that, though other circumstances are favourable for the production of dew, none, or almost none, appears, if the sky be cloudy; and that hoar frosts, which are so frequently injurious in spring and autumn, do not occur in those seasons, during cloudy weather. These facts, if

considered in themselves, are certainly very important, but they are not new, though they appear to have been thought so by Dr. Young, both by their being brought in support of his position, that I had been anticipated in several of the opinions contained in my Essay, which I had regarded as original, and by the use, which he has made of the *Italic* character, in presenting Mr. Pictet's words to his readers. With respect to the first fact, I referred in p. 78 of my Essay to Mr. Wilson's being acquainted with it, and as his observations concerning it appeared in 1786 in the *Edin. Phil. Trans.* his claim to the discovery must, by the common rule in such cases, be held superior to that of Mr. P., whose communication upon it was not published till 1792. Besides, Mr. W. formed his conclusion from several observations, whereas Mr. P. has given only a single one. But the same fact, though in forms somewhat different, was known to two authors anterior to both Mr. W. and Mr. P. For Lord Bacon says, *Nat. Hist.* § 866, "Star-like nights, yea, and bright moonshinè nights, are colder than cloudy nights;" and Professor A. Wilson, of Glasgow, has mentioned, *Phil. Trans.* 1771, that on a winter night, during which the atmosphere was several times misty and clear alternately, he observed a thermometer, suspended in the air, always to rise from a half to a whole degree, whenever the former state began, and to fall as much, as soon as the weather became serene. In regard to the facts related by Mr. P. on the authority of husbandmen, I shall only say, that Aristotle expressly mentions, that neither dew nor hoar frost occurs except the night be clear, and that the same observation has been made by many other authors prior to Mr. P.

5. Mr. Prevost says at the end of his disquisition, that all the facts mentioned by Mr. Pictet may be accounted for on the principles previously established by him; but, as I have proved, I think, that he did not know that the surface of the earth ever becomes colder than the atmosphere, by radiation, any explanation he could offer of those facts must necessarily be imperfect.

Having now, I think, shown, that nothing is contained in the extract given by Dr. Y. from Mr. Prevost that required being taken notice of in my Essay, besides what he has said on the effect of clouds on the temperature of the earth and air at night, I shall mention, in justice to this philosopher, that the *Essai d'Explication*, just discussed, has risen considerably in my estimation, since I knew, that it had been given to the world so long ago as in 1792; for as I had before supposed, that he had been assisted in forming it by the works of Count Rumford and Mr. Leslie on heat, which were published in 1804. His opinion, therefore, on the effect of clouds in keeping the earth, and lower atmosphere, warm at night, appears to me now not only to be entirely original, but to exhibit a degree of knowledge on the subject of radiant heat, which I did not conceive had been possessed by any person at the time of its publication.

III. I mentioned in a note in the 105th page of my Essay, that

I had once intended to offer an explanation of some very curious observations on dew by a French author, Mr. B. Prevost, but that, fearing to prove very tedious, I had afterwards given up the design. I intimated, however, that my explanation would have been, in great measure, derived from the doctrine of radiant heat. On this note Dr. Y. has remarked, that if I had been as solicitous to attend to the labours of my contemporaries, as I had been laudably anxious (the expressions are his) to recur to those of my predecessors, I might have said, not that the experiments of Mr. P. *might* be easily explained, but that they actually *had* been explained in a similar manner by one of my own countrymen, that is, by himself.

If Dr. Y. ascribes to me here a designed inattention to what he has written upon Mr. P.'s experiments, he is very much in error; but, if he means only, that, from want of sufficient diligence, I had never read the explanation given of them in his Lectures, he is, I believe, perfectly correct. For I do not recollect my having seen that explanation before I met with it in his Criticism, and I have no note of it among my papers, though these contain a considerable number of references to his 51st lecture, and one to the 60th. The cause of my not having examined an intermediate one, which contains the explanation spoken of, I take to be this, that having gone to a public library, with the view of consulting his work, while in haste, for the reason formerly mentioned, to finish my Essay, and not finding in the Index a single reference under the word "Dew," to any of his lectures, I searched no further.

But, admitting that I had been acquainted with Dr. Y.'s explanation of Mr. P.'s experiments, I should not have been prevented by that circumstance from offering one myself, if, from other reasons, I had been inclined to do this; since he treats of only a *part* of those experiments, and leaves unanswered several important questions relating to this part; whereas I should have treated of the *whole* of them, and have attempted to answer *all* the questions to which they give rise. I proceed now to justify the opinion which I have thus ventured to give respecting the inadequacy of Dr. Y.'s explanation. In the prosecution of this undertaking, I shall, in consequence of the indefinite manner in which the explanation is expressed, endeavour to show from collateral circumstances what it cannot be, rather than prove in a direct way what it is not.

The experiments, considered by Dr. Y., comprise some of the most important facts relating to the formation of dew; he, therefore, that can account for them fully, must, in my opinion, possess the true theory of that appearance. But that this was not the case with Dr. Y., when he published his lectures, is proved by the following passage in the very lecture, which contains the explanation in question. "The dew, which is deposited upon vegetables, is partly derived, in the evening, from the vapours ascending from the heated earth, [whence is the other part?]-and towards the morning from the moisture descending from the air above, as if

*begins to cool.* Sometimes, however, in warmer weather, the dew begins to *descend* in the evening." For, what connexion with the doctrine of radiation has this theory of the dew, which is deposited by the atmosphere, and in what does it differ from that which was invented by Aristotle? In like manner it may be said, that if Dr. Y. had entertained a just notion of the cause of dew, he would not have conjectured, that the remarkable cold observed by Mr. Wilson on snow, in conjunction with hoar frost, a substance admitted by Dr. Y. to be frozen dew, might arise from the contact of air occasioning the snow to melt more readily; ii. 395; nor have supposed, that, when the surface of a metal remains dry, in the neighbourhood of a piece of glass which is covered with moisture, this arises from the different attractions for moisture in those bodies while of the same temperature; i. 708; since both of these facts must now, I think, if he considers them fully, appear to him to owe their explanation to the doctrine of radiation of heat. Having offered this argument in the first place, as being of easy comprehension, I pass to others of a different kind, which I am afraid I shall not be able, for various reasons, to render very perspicuous. To facilitate reference, I shall place at the bottom of this page all that he has transcribed in his Criticism from his Lectures concerning Mr. P.'s experiments.\*

The account given by Dr. Y. of such of Mr. P.'s experiments as he means to consider, is, to my apprehension, involved in much obscurity, part of which would have been prevented by his mentioning, that the experiments were made upon a glazed window-frame, pertaining to a room the air in which was warmer than that without. But, notwithstanding that these circumstances were known to me, I mistook one of his facts on the first reading of his statement, and was not undeceived, until I consulted Mr. P. In Mr. P.'s apparatus a thin rectangular plate of metal is attached to the inner or outer side, or the warm or cold side, (to use Dr. Y.'s expression,) as occasion may require, of a pane of glass of the window, in such a manner, as to be surrounded by a margin of bare glass. When, therefore, Dr. Y. spoke of moisture being deposited on the glass *near* the metal, I naturally concluded, as I remembered the construction just described, that the portion of glass meant was, the naked margin on the same side with the metal. But on looking into Mr. P. I found, that Dr. Y.'s meaning must be, that the moisture was deposited on the glassy surface immediately *opposite* to

\* "It has been observed, that a piece of metal, placed on glass, usually protects also the opposite side of the glass from the deposition of dew; and Mr. Benedict Prevost has shown, that, in general, whenever the metal is placed on the warmer side of the glass, the humidity is deposited more copiously, either on itself, or on the glass near it, [as in the case of the shutter]; that when it is on the colder side, it neither receives the humidity, nor permits its deposition on the glass; but that the addition of a second piece of glass over the metal destroys the effect, and a second piece of metal restores it. It appears, that, from its properties with respect to radiant heat, the metallic surface produces these effects by preventing ready communication either of heat, or of cold to the glass."

the metal. If this interpretation be admitted, his statement, as far as it reaches, seems to me quite correct, except where it is said, that, when the metal is on the warmer, or inner, side, the humidity is deposited more copiously *either* on the metal, *or* on the glassy surface opposite to it; since, according to Mr. P., it may appear on both at the same time.

My remarks on Dr. Y.'s explanation will chiefly refer to what takes place on the colder or outer side of the window. This learned author has said, that, when the metal is placed upon the warmer or inner side of the glass, the humidity is deposited *more* copiously on its outer surface, but has not mentioned where the *less* humidity occurs. He has compared, therefore, a thing that is known with something that is unknown, as far as his account of facts is concerned. This deficiency is to be supplied from Mr. P., who informs us, that the *less* humidity occurs on glass similarly exposed with the former, but having no metal attached to its inner surface. The appearance, consequently, on the bare glass, from being influenced by fewer circumstances, and being that with which the other is compared, is to be regarded as the standard fact. But surely we must be able to explain a standard fact, before we can fully account for any deflection from it. No sufficient reason, therefore, could, in my opinion, have possibly been offered by Dr. Y. for the greater quantity of moisture on the outer surface of glass, to the inside of which a metal was affixed, while he was ignorant, why moisture appeared in *any* quantity on bare glass similarly placed with the former, which I must conclude he was, from the reason given by him for the formation of dew upon vegetables, the relations of these substances and of glass to that fluid being the same, or nearly the same.

But it is manifest to me, for another reason, that Dr. Y. could not have given an adequate explanation of the facts related by him from Mr. P., which is, that he was unacquainted with certain conditions necessary for the appearance of those, which occur on the outside of the window, for they are not described by Mr. P., and Dr. Y. seems never to have made any experiments himself upon this subject. The conditions are, 1. The exposure of the window to a considerable extent of the heavens: 2. Stillness of the air: 3. Clearness of the sky.

Again:—The heat of the air in the room is a given condition; but whence is the radiant cold, to use an expression rendered necessary by Dr. Y.'s mode of accounting for the appearances, which enables the outer surface of the glass to condense the watery vapour of the atmosphere? It cannot be furnished by the neighbouring air, since this body must be unable to give more than what is possessed by itself, and unless the glass becomes colder than the air, the latter will not deposit moisture on the former. If it be now said, that the outside of the window-glass becomes colder than the air by receiving cold, which is radiated to it by the heavens, to employ still an expression authorised by Dr. Y., it will be replied,

that this source of cold cannot be resorted to for the removal of the present difficulty, since, I think, I have shown, that Dr. Y. was ignorant of the necessity of a clear sky for the success of any of Mr. P.'s experiments, and unless the sky be clear, the outer surface of the window-glass will not be rendered colder than the atmosphere by radiation.

To conclude the consideration of Dr. Y.'s claim to having justly accounted for the facts related by Mr. P., I shall remark, that although his explanation could be made to apply to the whole of them, and all their attending circumstances, still, as it was never verified by experiments, that would admit the existence of no cause of the appearances observed, but that which is assigned by him, it can strictly be regarded in no other light than in that of a conjecture.\* The conjecture, however, was, I believe, original; it was most happy too, since, if admitted to be just, it completely accounted for several important circumstances in Mr. P.'s experiments. If then its learned and ingenious author had established its truth by facts clearly seen by himself, and had afterwards pursued the subject of dew through its various ramifications, by means of the clue which would have thus been obtained, he must soon have acquired a knowledge of the theory, which has lately been submitted by myself to the consideration of the learned, and which he, as a member of that body, has pronounced to be just. But, I must, on the other hand be permitted to say, that, if Dr. Y., forgetting that Newton became a glass-grinder in the service of science, will neglect to employ, for the increase of natural knowledge, the slow and laborious method of observation and experiment, and will frequently exhibit his speculations in a manner, unsuited to the capacities of ordinary men, he ought not to think it strange, that opinions, advanced by him on difficult points of philosophy, are not, agreeably to his own remark at the end of the Criticism, received as truths beyond doubt, and are often not understood.

I am, Sir,

Your most obedient humble servant,

London, Feb. 15, 1815.

WILLIAM CHARLES WELLS.

\* In p. 475 of the second volume of Dr. Y.'s work on Natural Philosophy is the following passage:—"Most of the facts [related by Mr. P.] may perhaps be explained by Mr. Leslie's discoveries." Hence I naturally concluded, that the author himself at the time of his publishing that work, regarded his explanation, not only as a conjecture, but as a conjecture inapplicable to the whole of the facts observed by Mr. P. I have very lately learned, however, that Dr. Y.'s first volume was printed *after* the second. What is said, therefore, by him, respecting the cause of those facts, in the passage quoted in his Criticism from the first volume, must be held to supersede what is cited by me in the beginning of this note from the second.

## ARTICLE III.

*Experiments to determine the Definite Proportions in which the Elements of Organic Nature are combined.* By Jacob Berzelius, M.D. F.R.S. Professor of Chemistry at Stockholm.

III. *Analysis of the Ternary Oxides.*

(Concluded from p. 184.)

*Substances not Acid.*—I mentioned before that the ternary oxides composed of carbon, hydrogen, and oxygen, have a strong tendency to combine with saline bases; and that in these combinations they act the part of acids. They possess the acid characters, however, in very different degrees. Many of them possess the property of combining at the same time with the strong acids, as is the case with tannin, and almost all the extractive and colouring matters. These oxides then are placed near the middle of the electro-chemical chain, and their affinities diminish in proportion as they approach the *point of indifference* of that chain. The only ternary oxide which contains nitricum, is a strong alkali; and the quaternary oxides, which contain nitricum, have likewise properties opposite to those of vegetable substances. They lie on the other side of the point of indifference of the electro-chemical chain; their tendency to unite with acids being more distinct than to combine with bases.

The ternary oxides already analyzed are all of a decided electro-negative character, so that chemistry gives them the name of acids; if we except tannin, the acid properties of which are however very distinct.

It is known that the resins, fat oils, gums, extracts, &c., form insoluble compounds with various metallic oxides. These combinations are generally stated in such a manner in our Manuals of Chemistry, that a reader, whose attention is not specially directed towards these objects, considers these combinations as the only ones of which these organic bodies are capable: and, in fact, they are the only ones which are recognized by some striking property. These insoluble combinations of ternary oxides with binary oxides belonging to the class of salifiable bases, makes it probable that there exists a general affinity between these ternary oxides and bases. It is easy to convince ourselves of the truth of this suspicion by experiment.

Gum is precipitated by subacetate of lead, and sugar has the property of rendering lime more soluble in water. These facts were ascertained without the consequence being drawn from them, that these combinations owe their existence to a general affinity between saline bases and vegetable bodies, in consequence of which gum ought to have an affinity with lime and sugar for oxide of lead.

If we pour sugar into a solution of subacetate of lead, no precipitate takes place. But we should deceive ourselves, were we to conclude from this, that sugar does not combine with oxide of lead, or that it does not form an insoluble compound with that oxide. A solution of sugar not only dissolves oxide of lead; but by a long digestion it combines with an excess of that oxide, and forms an insoluble, light, white, and bulky compound. This compound dissolves in acetate of lead, subacetate of lead is formed, and the sugar is disengaged. I shall have occasion to speak more of this combination below. If we pour some drops of caustic ammonia into a solution of sugar of milk, we do not observe any change. The ammonia acts as a re-agent, just as if the sugar of milk were not present. We should deceive ourselves were we to conclude from this that sugar of milk and ammonia have no affinity for each other. We have only to macerate at the temperature of  $122^{\circ}$  a solution of sugar of milk with oxide of lead, and then to drop a little ammonia into the filtered liquid. This liquid, which is a combination of sugar of milk and oxide of lead, is decomposed, and an insoluble compound of sugar of milk with an excess of oxide of lead is precipitated. This precipitation is owing to the affinity of ammonia for sugar of milk, which it divides with the oxide of lead.

It is, in general, very difficult to obtain neutral combinations with these substances; at least unless this can be done when they are in solution in water, which has the same action on them as on the metallic oxides placed round the point of indifference of the electro-chemical chain; as the oxide of bismuth, antimony, tellurium, &c. We know that it is impossible to obtain a neutral muriate of these oxides, by treating them with liquid muriatic acid; we obtain only an insoluble submuriate and a soluble muriate with an enormous excess of acid. In the same manner when we digest a solution of common sugar, or sugar of milk, with oxide of lead, we obtain only an insoluble compound with an excess of oxide of lead, and a soluble combination with an enormous excess of sugar. If you pour ammonia into a solution of sugar, nothing indicates that the substances combine, and the ammonia evaporates from that solution as easily as from pure water. But if you expose sugar in powder to the action of ammoniacal gas, the sugar absorbs the gas, and forms with it a compound. Here the same thing takes place as when you expose oxide of antimony to the action of muriatic acid gas. These observations, I conceive, not only prove that the ternary vegetable oxides have a general tendency to combine with salifiable bases; but they point out also the reasons why this general tendency has hitherto remained unnoticed.

To be able to speak of these combinations it will be necessary to give them names, and I thought it would be agreeable to the principles of the chemical nomenclature to name, for example, a combination of sugar with oxide of lead, according to the different degrees of saturation, *saccharate*, *sub-saccharate*, *super saccharate*

of lead. In the same manner I shall use the terms *gummates*, *amylates*, *saccolactates*, to denote the combinations of gum, starch, sugar of milk with saline bases.

There is still a circumstance relative to the combination of the ternary oxides with the saline bases, which deserves to be mentioned here. An excess of these bases, especially the stronger ones, decomposes a great part of the ternary oxides in different ways, usually producing a great quantity of carbonic acid. The ternary oxides least exposed to this decomposition are these: 1. whose atoms of oxygen are equal in number to those of some one of the other elements, or surpass them in number: and 2. when the ratio of the hydrogen to the oxygen is less than in water. Such is the case with most of the acids analyzed. If the atoms of carbon and hydrogen surpass in number those of oxygen to a certain amount, the ternary oxide has a great tendency to undergo decomposition from the action of the salifiable bases. Such, for example, is the case with gallic acid, tannin; and, to a certain extent, with acetic acid. When, on the other hand, the atoms of each of the combustible elements surpass four or five times those of oxygen, the ternary oxide is more permanent: so that those oxides are best preserved which have either a great excess of combustible atoms or of oxygen. Of consequence, benzoic acid and the fat oils are but insensibly altered by the action of alkaline bodies.

This is the reason why, when the ternary oxides are gradually decomposed, either by the influence of air and water, or of acids, their atoms arrange themselves in such a manner as to form new products, in which, on one side, the oxygen is in excess, and, on the other, the atoms of the combustible elements greatly surpass those of the oxygen. Thus sugar is converted into carbonic acid and alcohol by fermentation; and gallic acid, by the influence of an alkali, yields on one side water and carbonic acid, and, on the other, an extractive matter abounding in carbon, to which it owes its dark colour. Perhaps it would be useful to class the ternary oxides into: 1. Oxides not easily decomposed in consequence of their excess of oxygen; such as oxalic, citric, tartaric acids, &c. 2. Oxides easily decomposed in consequence of the equilibrium of their elements, which is easily destroyed by a small force; such as tannin, gallic acid, sugar of milk, common sugar, &c. 3. Oxides not easily decomposed in consequence of the excess of their combustible atoms; such as benzoic acid, resins, fat oils, &c.

Let us now turn our attention more particularly to the ternary oxides destitute of acid properties.

#### 10. Common Sugar.

I digested a solution of sugar with oxide of lead. At first the oxide dissolved, but after the digestion had been continued for some time, it was converted into a light white powder, by which the whole liquid was rendered opaque. I separated the white powder

on a filter, washed it with boiling water, and dried it in a vacuum. This substance is a new combination of sugar and oxide of lead. It is quite insoluble in water, light, white, and destitute of taste. The acids, even carbonic acid gas, separate the sugar from it. When heated to a certain point it takes fire, and continues to burn of its own accord, leaving as a residuum oxide of lead mixed with metallic lead. It appears to contain no combined water. I have not been able to procure this substance quite free from carbonate of lead; but it is easy to determine how much of it is present by dissolving it in acetate of lead, which does not act upon the carbonate. By this means I found that in the subsaccharate of lead employed in my experiments, there was  $1\frac{1}{4}$  per cent. of carbonate of lead; for 10 parts dissolved in acetate of lead left 0.15 of carbonate of lead undissolved.

Two parts of the subsaccharate of lead, when burnt, left 1.1728 of oxide of lead; but we must subtract 0.03 for carbonate of lead, that is, 0.025 of oxide of lead, and 0.005 of carbonic acid. There remains 1.1478 for the oxide of lead, and 0.8222 for the sugar; so that the subsaccharate is composed of

Sugar.....	41.74	....	100
Oxide of lead .....	58.26	....	139.6
	<hr/>		
	100.00		

I repeated this analysis several times, and the results varied between 138 and 140 of oxide combined with 100 of sugar. The reason of this variation seems to be the difficulty of discovering when the oxide of lead is entirely penetrated with sugar. When any of it remains uncombined, it is obvious that the analysis will give an excess of base. The oxygen of 139.6 of oxide of lead is 9.98.

If we digest the above-mentioned subsaccharate in a solution of sugar, a part of it dissolves, and forms a clear liquid, with a slightly yellow colour, which contains lead. But the quantity of this metal is very small when compared with that of the sugar, of which the solution appears to contain an excess in the form of super-saccharate. When evaporated it leaves behind it a syrupy mass, which does not crystallize, and which attracts humidity from the atmosphere.

The crystalline form of sugar does not lead us to suspect that it contains water. I reduced it to a fine powder, and dried it in a vacuum. The loss of weight was only 0.1 per cent. I took ten parts of this sugar, and mixed them with 40 parts of yellow oxide of lead, reduced to a fine powder, and heated to redness afterwards. I digested this mixture in water, in the heat of a water-bath, till the oxide of lead had absorbed all the sugar. I then put it into a vacuum and dried it. The loss of weight was 0.53. I then heated it to  $212^{\circ}$  in a vacuum; but it sustained no farther loss of weight. This loss must have been water combined with the sugar; for, on dissolving the saccharate in nitric acid, not a single

bubble of carbonic acid gas made its escape. This shows that the sugar had not undergone decomposition. From this experiment it appears that sugar in its ordinary state is a compound of

Sugar .....	94.7	100
Water .....	5.3	5.6
	<hr/>	100.0

But 5.6 of water contain 4.941 of oxygen, which is exactly the half of that found in the oxide of lead with which the sugar is combined in the sub-saccharate.

I took five parts of sugar dried in a vacuum, and put them into a small glass exactly weighed, the mouth of which was covered with paper. I put this glass in a jar over mercury, and then filled the jar with ammoniacal gas. The gas was slowly absorbed; the sugar contracted in bulk, and its surface acquired so strong a crystalline lustre as to appear humid. The saturated combination was a dense, coherent, flexible mass, which might be cut with a knife. It exhaled the odour of ammonia. The absorption of ammoniacal gas continued for four days; but I left the sugar in the gas 24 hours after it was saturated. It had gained 0.26 of its weight. This combination, which I consider as a neutral saccharate of ammonia, is composed of

Sugar .....	90.000	100
Ammonia .....	4.930	5.49
Water of combination..	5.070	5.60
	<hr/>	100.00

But 5.49 ammonia contain 2.5 of oxygen, which is half the quantity that the water contains, and  $\frac{1}{4}$ th of the oxygen in the oxide of lead.

0.4 of sugar dried in a vacuum, gave by combustion from 0.237 to 0.24 of water; and from 0.607 to 0.61 of carbonic acid gas. Hence it follows, that sugar is composed of

Hydrogen .....	7.05
Carbon .....	41.48
Oxygen .....	51.47
	<hr/>
	100.00

But we have seen that in these 100 parts of sugar there are 5.3 of water; containing 4.67725 oxygen. Now  $4.67725 \times 11 = 51.44975$ . Hence it follows, that the 94.7 parts of pure sugar contained ten times that quantity, or 46.7725, and consequently, that sugar contains ten times as much oxygen as the water, five times as much as the oxide of lead, and 20 times as much as the ammonia with which it was combined in the compounds mentioned above,

0·9675 of subsaccharate of lead, equivalent to 0·4 of sugar, produced by combustion from 0·230 to 0·231 of water; and from 0·650 to 0·652 of carbonic acid gas. This gives from 6·756 to 6·785 per cent. of hydrogen, and from 44·2 to 44·336 of carbon. According to this analysis sugar is composed of

Hydrogen .....	6·785
Carbon .....	44·200
Oxygen .....	49·015
	<hr/>
	100·000

The difference between these two analyses is to be ascribed to the presence of water in the first analysis. It cannot be ascribed to the presence of carbonic acid in the subsaccharate employed; for this would occasion a diminution in the weight of the carbon found, as well as an augmentation of that of the oxygen, since sugar contains much more carbon and much less oxygen than carbonic acid.

But we have seen that sugar contains five times as much oxygen as the oxide of lead with which it is combined, and ten times as much as the water. Hence it ought to contain either five or ten volumes of oxygen. When we compare the weights of its elements with one another, we find that it is composed of 100 + 12 C + 21 H. This gives per cent.

Hydrogen .....	6·802
Carbon .....	44·115
Oxygen .....	49·083
	<hr/>
	100·000

A result which comes very near the actual analysis. Some readers will probably think it likely that the 21 H ought to be either 20 or 24, in order to be double the number of atoms either of oxygen or carbon; but it is evident, that the difference in the quantities of water which ought to be produced by the combustion of sugar, according to either the one or the other of these suppositions, is so considerable, that it would make a very sensible difference in the result of the analysis. And we shall see afterwards, that such a proportion between the oxygen and hydrogen characterizes the substances which do not yield mucous acid when acted upon by nitric acid.

To obviate the objection that the saccharate analyzed might contain some other body besides sugar, I decomposed a certain quantity of it by mixing it with water, through which I passed a current of carbonic acid gas; I filtered the liquid, and concentrated it. It yielded pure white crystals, which possessed all the properties of common sugar.

I likewise burnt a part of it, and obtained from 0·4 of sugar, dried in a vacuum in a hot sand bath; 0·2846 of water, and 0·628 of carbonic acid, which makes in 100 parts

Hydrogen .....	6.891
Carbon .....	42.704
Oxygen .....	50.405
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	100.000

Now it is evident that this is  $21\text{ O} + 24\text{ C} + 44\text{ H}$ ; that is to say, that it contains  $1\text{ O} + 2\text{ H}$  more than pure sugar, and as much less than the sugar which I have before analyzed; so that this last sugar contains only half the water of combination of the other. As this was the last of all the experiments described in this memoir; and as it was made just at the time when other circumstances obliged me to desist, I have not had an opportunity of repeating it. It would have been interesting to discover by what accident one half of the water of combination had been removed.

Thenard and Gay-Lussac found sugar composed of

Hydrogen .....	6.90
Carbon .....	42.47
Oxygen .....	50.63
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	100.00

This coincides with my last analysis.

11. *Sugar of milk.*—The sugar of milk employed in the following experiments was purified by repeated crystallizations. It was then reduced to powder, and dried at  $212^{\circ}$  in a vacuum. Gay-Lussac and Thenard found sugar of milk composed of

Hydrogen .....	7.341
Carbon .....	38.825
Oxygen .....	53.834
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	100.000

I burnt 0.4 of sugar of milk, which produced 0.244 water, and 0.5805 carbonic acid. From these data sugar of milk is composed of

Hydrogen .....	7.167
Carbon .....	39.474
Oxygen .....	53.359
	<hr/>
	100.000

These numbers correspond almost exactly with the supposition that sugar of milk is composed of one volume of oxygen, one volume of carbon, and two volumes of hydrogen. According to that supposition it is composed per cent, of

Hydrogen .....	7.019
Carbon .....	39.819
Oxygen .....	53.162
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	100.000

We can hardly doubt, therefore, that crystallized sugar of milk is composed of  $O + C + 2 H$ . But it would be a mistake to suppose that pure sugar of milk has such a composition. It contains combined water as well as common sugar. If it be fused at a temperature not sufficiently high to decompose it, a great deal of water is disengaged, which is not formed by any alteration in the sugar, as it remains colourless and gives a colourless solution. To succeed in this experiment it must be made on a very small scale. Common sugar does not give out any water till it begins to become brown.

To determine the quantity of water in sugar of milk, I made an experiment similar to that made with common sugar. I took three parts of sugar of milk and mixed them with 20 parts of oxide of lead, and digested the mixture with a little water, till the whole sugar was absorbed by the oxide of lead, and the mass was reduced to the consistence of a thick paste. This mass, dried in a vacuum at the ordinary temperature of the atmosphere, lost 0.34 of its weight. When heated to  $212^{\circ}$  in a vacuum, it sustained an additional loss of 0.04, making in all 0.38, equivalent to 12.333 per cent. These 12 $\frac{1}{3}$  contain 10.8 of oxygen, which is the fifth part of all the oxygen contained in the crystallized sugar of milk. The small deviation is too inconsiderable to be owing to any thing else than an error in the experiment. Hence it follows, that sugar of milk is combined with a quantity of water, which contains one fourth as much oxygen as itself. Experiment in that case ought to have given 12.04 of water, instead of 12.333.

To determine the capacity of saturation of sugar of milk I digested a mixture of it and oxide of lead in water, in a corked phial; the oxide had been obtained by burning oxalate of lead in an open vessel, because the oxide thus prepared is in the state of a very fine powder. On attempting to assist the solution of the oxide by heat, I found that the temperature must not be raised higher than  $140^{\circ}$ , because at a higher temperature the sugar of milk is decomposed by the action of the oxide of lead; the liquid becomes brown, and assumes an empyreumatic odour, though it has not been raised to the boiling temperature. Bouillon Lagrange, and Vogel, have already observed this property of sugar of milk to be decomposed by the more powerful bases.

A strong solution of sugar of milk, digested during eight hours with oxide of lead, gave a colourless milky solution, of a taste a little sweet, alkaline, and astringent. This solution contained a small quantity of a white, light, and mucous matter, which was easily separated from the oxide of lead undissolved, by agitating the vessel. I poured the solution, holding the mucous matter suspended upon a filter; and as the liquid passed very slowly, I placed it under a glass jar, into which I had introduced a quantity of moist hydrate of lime, in order to absorb the carbonic acid gas of the air, and prevent the liquid from being affected by it.

The mucous matter remaining on the filter had some resemblance to oxide of tin. When dried in a vacuum it was semi-transparent, and greyish coloured. When heated to  $212^{\circ}$  in a vacuum, it gave out water, and became yellow. This yellow matter when exposed to the direct rays of the sun, is decomposed in a few minutes. It becomes at first green, and then grey. Decomposed by combustion it gave 63.529 per cent. of oxide of lead. Hence it was composed of

Sugar of milk.....	36.471	.....	100.00
Oxide of lead.....	63.529	.....	174.15

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100.000

These 174.15 of oxide of lead contain 12.45 of oxygen; but we have seen that sugar of milk contains  $12\frac{1}{2}$  water, or 100 sugar of milk are combined with 14 water, in which there are 12.245 of oxygen. It is therefore clear that in the saccolactate of lead analyzed, the sugar of milk ought to contain four times as much oxygen as the oxide of lead.

A part of the filtered liquid mixed with caustic ammonia, deposited an abundant precipitate, possessing all the characters of the above-described saccolate, except the mucous aspect and the pelliculidity after desiccation. It became equally yellow when the water of combination was driven off. Analyzed by combustion it gave 64.14 per cent. of oxide of lead. Treated by neutral acetate of lead it gave a little carbonate of lead, to the presence of which it probably owes its difference from the saccolate above described.

Another part of the liquid evaporated to dryness gave a gummy and transparent mass, which did not attract moisture from the atmosphere. When dried at  $212^{\circ}$  in a vacuum, it swells up, becomes frothy, and acquires a yellow colour. Analyzed by combustion it left 18.123 per cent. of oxide of lead. I consider this substance as a super-saccolate of lead. It is composed of

Sugar of milk.....	81.877	.....	100
Oxide of lead.....	18.123	.....	22.1

Thus it appears that in the super-saccolate the sugar of milk is combined with  $\frac{1}{4}$  of the quantity of base as in the neutral saccolate.

For  $\frac{174.15}{8} = 21.77$ . The error in the result is owing to the impossibility of drying the gummy combination without exposing it to a temperature at which it begins to undergo decomposition to which its yellow colour is owing.

Oxide of lead digested with sugar of milk appears at first not in the least altered. It is, however, converted into a subsaccolate of lead. When well washed, and dried in a vacuum in the temperature of  $212^{\circ}$ , it forms a yellow powder. When analyzed by combustion it leaves 87.2 per cent of oxide of lead. Hence it is composed of

Sugar of milk .....	12.8	.....	100
Oxide of lead .....	87.2	.....	681
			<hr/>
			100.0

But  $174.15 \times 4 = 696.6$ ; hence it follows, that in the subsaccolate the sugar of milk is combined with four times as much base as in the neutral saccolate.

As it is easier to obtain this last combination with excess of oxide in a state of purity than the others, I consider the result of its analysis as nearer the truth than that of the others. It is clear that the sugar of milk and oxide of lead in it ought to contain equal quantities of oxygen. 681 of oxide of lead contain 48.65; and we shall see immediately, from other data, that the sugar of milk ought to contain exactly the same quantity.

I exposed five parts of pulverized sugar of milk to the action of ammoniacal gas; their action on each other was very slow, especially at the commencement. The absorption continued for 15 days. The sugar of milk had then diminished a little in bulk, and had assumed a yellowish colour, owing to some slight alteration in its composition. It now weighed 5.62, and exhaled a strong smell of ammonia. I left it under a glass jar, not shut, and weighed it at the end of every half hour. It diminished in weight with a great deal of rapidity, so that after two hours it weighed only 5.33. After this I found that it scarcely lost any more weight; for in 18 hours the loss did not exceed 0.05. From this we see that sugar of milk forms at least two saccolates of ammonia, in which the proportions of ammonia are as 1 : 2. Now we have seen that 100 parts of crystallized sugar of milk contain  $12\frac{1}{2}$  per cent. of combined water, and that the same quantity of sugar of milk absorbs 12.4 of ammonia. The oxygen of this last is 6.7, and that of the first 10.9, or very nearly twice as much; so that we have here found the same ratio between the ammonia and the water of combination as in common sugar. And though the sugar of milk absorbed a little more ammonia than it ought to have done according to calculation, this seems to have been owing to a commencement of decomposition in it. In fact, when saccolate of ammonia was dissolved in water, and muriatic acid mixed with the solution, there was a disengagement of some carbonic acid, which sufficiently explains the excess of ammonia absorbed. I ought, besides, to make the general observation, that the tendency of sugar of milk to be decomposed by the bases, which appears to exceed that of tannin and gallic acid, prevents any of my experiments on its capacity of saturation from being decisive; but as they correspond sufficiently with the probable composition of sugar of milk, I consider them as probably very near the truth.

Supposing crystallized sugar of milk to be  $O + C + 2H$ , and that it contains a quantity of water the oxygen in which is  $\frac{1}{4}$  of the whole found in the substance, there ought to remain for pure

sugar of milk  $40 + 5 \text{ C} + 10 \text{ H}$ , or, according to the composition of saccolate of ammonia,  $8 \text{ O} + 10 \text{ C} + 16 \text{ H}$ , which constitute per cent.

Hydrogen .....	6.385
Carbon .....	45.267
Oxygen .....	48.348

100.000

Sugar of milk then contains more carbon and less oxygen than common sugar. This explains why, when we burn the compound of these two bodies with oxide of lead, the first reduces much more lead than the other. We understand also why sugar of milk is more easily carbonized than common sugar.

12. *Gum Arabic*.—I mixed caustic ammonia with a filtered and boiling solution of gum arabic. I then mixed it with a solution of subnitrate of lead, likewise boiling hot. I took care not to add enough to precipitate the whole of the gum. I decanted off the liquid from the precipitate, which I washed repeatedly with boiling water. I then subjected it to pressure, and dried it in a vacuum, finally exposing it to the heat of  $212^{\circ}$ .

When analyzed by combustion, the gummate of lead left for residue 38.25 per cent. of oxide of lead. It was therefore composed of

Gum .....	61.75	100.000
Oxide of lead .....	38.25	62.105

100.00

The oxygen of these 62.105 of oxide of lead is 4.44. To obtain a subgummate of lead I digested a solution of gum with oxide of lead. A subgummate was formed, very heavy and coherent; but which could not be entirely separated from the gummy liquid. Of course I was unable to analyze it.

0.648 of gummate of lead, equivalent to 0.4 of gum, produced by combustion 0.2297 of water, and 0.617 of carbonic acid, equivalent to 6.75 per cent. of hydrogen, and 41.956 of carbon.

0.4 of gum arabic, in the state of a fine powder and dried in a vacuum at the temperature of  $212^{\circ}$ , produced by combustion 0.231 water and 0.6196 of carbonic acid, equivalent to 6.788 per cent. of hydrogen and 41.906 of carbon. Hence it appears, that dried gum arabic contains no water; for it is probable that gummate of lead cannot retain water with greater force than the other combinations having oxide of lead for their base. Gum arabic is then composed as follows:

Hydrogen .....	6.788
Carbon .....	41.906
Oxygen .....	51.306

100.000

But we see that gum arabic ought to contain 12 times as much water as the base by which it is saturated: for  $4.44 \times 12 = 53.28$ . This small difference ought to be ascribed to a small error in the analysis, or in the preparation of gummate of lead; which error is in the present case increased 12 fold in the calculation. But if gum arabic contain 12 atoms of oxygen it must contain 13 of carbon and 24 of hydrogen, which make in 100 parts

Hydrogen .....	6.792
Carbon .....	41.752
Oxygen .....	51.456
	<hr/>
	100.000

This agrees very well with the result of direct experiments. Gum arabic then is  $12\text{ O} + 13\text{ C} + 24\text{ H}$ .

The analysis of Gay-Lussac and Thenard gave 6.93 hydrogen, 42.23 carbon, and 50.84 oxygen.

I ought to observe, that in my experiments, gum, whether pure or combined with oxide of lead, produced traces of nitric acid; so that what in the analysis I considered as pure hydrogen, contains in fact a trace of the radicle of nitric acid; which, as is known, has the same capacity for oxygen that hydrogen has.

I am, however, far from pretending that gum arabic contains nitricum among its elements. It is more probable, that the formation of the nitric acid is owing to the presence of a small quantity of vegetable albumen in the gum. Probably the excess of  $\frac{1}{3}$  of a per cent. of carbon obtained in the analysis, is owing to the same cause.

Gum arabic, when incinerated, leaves a small quantity of ashes, the weight of which is very variable; and, of course, it cannot be considered as an essential constituent of the gum. So much the less, as both Vauquelin and Cruikshanks found that gum contained vegetable salts with a base of lime. To see whether gum contained any secondary element, I endeavoured to separate it from gummate of lead. I found that this could not be accomplished either by means of sulphuric acid, or sulphureted hydrogen; for the disengaged gum causes the insoluble precipitates to pass through the filter, and it prevents them from being ever deposited. I therefore dissolved gummate of lead in acetic acid, and into the solution I poured alcohol, which precipitated the gum. I washed the gum very well in alcohol. This gum, being dried and burnt in a glass capsule, left oxide of lead, retained probably in the state of a super-gummate of lead. The ashes were easily soluble in acetic acid. The liquid being evaporated to dryness, and burnt, left sulphate of lime, with traces of sulphate of potash, weighing together  $\frac{1}{3}$  of a per cent. of the gum. But sulphuric acid constituting more than one half of these ashes, it appears that the small quantity of the radicles of the bases cannot be considered as of any value in the composition of gum arabic.

Thenard and Gay-Lussac obtained 2·4 per cent. of the gum in ashes, of which they kept an account in the exposition of the result of their analysis of gum arabic. These ashes consisted chiefly of carbonate of lime, the carbonic acid of which constituted nearly one per cent of the gum. No attention was paid to this; so that what they considered as 100 gum, was in reality 101. Such an error is too considerable to be neglected.

Gum in its ordinary state contains water; but this water is a hygroscopic substance, whose quantity varies according to the state of the hygrometer. I have found that gum in powder, dried in a vacuum at the temperature of 212, lost about 17 per cent. of water. From this we may conclude, that gum ought to contain a quantity of water, the oxygen in which is  $\frac{1}{4}$  that in the gum. I have not found that gum when dried slowly forms other combinations with water.

13. *Potatoe Starch*.—I took some potatoe starch, mixed it with a little water, and poured it by small portions at a time into boiling water, till I had obtained a solution of the requisite degree of consistency. I passed it through a linen cloth, and then mixed it with a boiling solution of subnitrate of lead, which I added in excess, and with which I digested the precipitate for a long time, that no part of the starch might remain uncombined. I then washed the precipitate by boiling it repeatedly in water, till that liquid ceased to take up any more subnitrate of lead. I then subjected it to pressure, and dried it in a vacuum in the temperature of 212°. This amylate, when analyzed by combustion, left 28 per cent. of oxide of lead. Hence it is composed of

Starch.....	72	.....	100
Oxide of lead .....	28	.....	38·89
	<hr/>		
	100		

These 38·89 parts of oxide of lead contain 2·78 of oxygen.

I endeavoured, likewise, to form an amylate containing more base. I found that such a compound exists, though I was not possessed of any method of separating it in a state of purity. We form it by mixing ammonia with a boiling solution of starch, and precipitating the mixture by a boiling solution of subnitrate of lead. But we cannot (unless by pure accident) find the point at which no neutral amylate is formed, or no subnitrate of the last degree. A subamylate, which I endeavoured to obtain as near the point of supersaturation as possible, without being mixed with insoluble subnitrate, gave by combustion half its weight of oxide of lead; so that it seems to contain the starch combined with three times as much base as in the neutral amylate.

0·4 of starch, dried in a vacuum, gave by combustion 0·2405 water, and 0·6395 carbonic acid, equivalent to 7·064 per cent. of hydrogen, and 43·481 of carbon.

0·555 of amylate of lead, equivalent to 0·4 of starch, produced

by combustion 0.239 water, and 0.643 carbonic acid, which gives us per cent. 7.023 hydrogen, and 43.724 carbon. It appears then that starch dried in a vacuum does not retain any water. Potatoe starch then is composed of

Hydrogen .....	7.064
Carbon .....	43.481
Oxygen .....	49.455
	<hr/>
	100.000

But we find that in amylate of lead the starch contains 18 times as much oxygen as the base, and in the subamylate six times as much. For  $2.78 \times 18 = 50.04$ , which differs very little from 49.455. Starch then contains either six or 18 atoms of oxygen. It is therefore composed of  $6\text{ O} + 7\text{ C} + 13\text{ H}$ , or of  $18\text{ O} + 21\text{ C} + 39\text{ H}$ . According to this statement, its composition should be

Hydrogen .....	7.090
Carbon .....	43.327
Oxygen .....	49.583

This agrees well with the analytical result.

Thenard and Gay-Lussac found wheat starch composed of 6.77 hydrogen, 43.55 carbon, and 49.68 oxygen. 20 parts of starch, when incinerated in a platinum crucible, furnish 0.046 of ashes, consisting entirely of the earthy phosphates. Of course they appear to have been only mechanically mixed with the starch; but if they be chemically combined, it is difficult to determine the relation of their elementary atoms to those of the primary elements.

Before finishing this first set of experiments on the chemical proportions in organic nature, I shall make some observations relative to the substances which have come under our review.

As to the number of elementary atoms of which an organic atom is composed, it does not seem necessary that the most simple ratio between the elements should express that number. Thus it does not seem necessary that an atom of starch ought to contain  $6\text{ O} + 7\text{ C} + 13\text{ H}$ , or 26 elementary atoms; it is just as possible that it contains three times that number, and that it is upon such a composition that the proportion between the starch and oxide of lead in the neutral amylate depends. In the same way, we may say, that gallic acid is composed of  $3\text{ O} + 6\text{ C} + 6\text{ H}$ , and not of  $\text{O} + 2\text{ C} + 2\text{ H}$ . The great number of elementary atoms combined in a compound atom does not constitute any objection to this conclusion; for if the atom of starch contains in fact 78 elementary atoms, we may, on the other side, point out in organic nature compound atoms of the second and third order, which contain a still greater number of elements. Thus the atom of crystallized alum contains one atom of potassium, two of aluminium, eight of sulphur, 80 of oxygen, and 96 of hydrogen, constituting altogether

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187 elementary atoms: or, even supposing potash to contain only one atom of oxygen, the atom of crystallized alum will still contain 94 elementary atoms.

The organic atoms must be much larger than those of the first order in organic nature: hence the reason why their capacity of saturation is in general so small.

It is well known that sugar of milk and gum arabic form *mucous acid* when treated with nitric acid, while sugar and starch do not furnish any of this acid. In the two first the hydrogen has to the oxygen the same ratio as in water. The two last contain hydrogen in excess.

Sugar of milk is composed of.....  $8\text{ O} + 10\text{ C} + 16\text{ H}$

Mucous acid contains.....  $8\text{ O} + 6\text{ C} + 10\text{ H}$

If from the first you take  $4\text{ C} + 6\text{ H}$ , you obtain the second. In the same manner, by taking from two atoms of gum arabic  $4\text{ C} + 9\text{ H}$ , you will leave three atoms of mucous acid. As to sugar and starch, if you represent the first as containing 20 atoms of oxygen, and the last as containing 18 atoms, we shall find it impossible to form mucous acid without taking away some atoms of oxygen.

We may then form the idea that the organic atoms have a certain mechanical structure, which enables us to deprive some of them of certain elementary atoms without altering the whole very much. In this way we remove certain atoms of sugar of milk, and there remains mucous acid. If we continue to remove other atoms, this acid is likewise destroyed, and the whole reduced to the common products of the decomposition of the ternary oxides, namely, oxalic and malic acids. The construction of the atom of common sugar is such that, as soon as you take away the first elementary atom, it separates (so to speak) into oxalic and malic acids. It is only by such a structure that we can explain the different products from the action of nitric acid on the ternary oxides, composed of the same elements, and in proportions (stated in per cents.) but little different from each other.

I am persuaded that an attempt to study the probabilities of the construction of organic atoms, nearly in the same way as Dr. Wollaston endeavoured to represent the composition of boracite, by means of differently coloured balls—I am persuaded, I say, that such a study would be of the greatest importance, and might be even capable of correcting analysis; as analysis might probably indicate a number of elementary atoms, incapable of forming any regular figure whatever.

If we were acquainted with the composition of alcohol, and of several varieties of sugar, the theory of fermentation would follow as a consequence from this knowledge. It is natural to suppose that fermentation is a very simple operation, and that it connects *merely* in the formation of carbonic acid gas, from the carbon of the sugar uniting partly with the oxygen of the sugar, and partly

with that of the water, the hydrogen of which combines with the rest of the sugar, and forms alcohol.

In my memoir On the Cause of Chemical Proportions, I mentioned organic substances as of a composition difficult to be explained by the views of the corpuscular theory. We see that in proportion as we study organic nature these difficulties disappear; and in the present state of our knowledge the corpuscular theory is the only one which puts it in our power to explain the composition of organic atoms in a satisfactory manner. I shall probably discuss the subject more at large hereafter; when furnished with a greater number of experiments I shall be able to speak about it with more confidence.

#### ARTICLE IV.

*An Essay on the Shapes, Dimensions, and Positions of the Spaces, in the Earth which are called Rents, and the Arrangement of the Matter in them.* By Mr. John B. Longmire.

(Continued from p. 92.)

##### *The Arrangement of the Matter in Bended-Tabular Rents.*

THE most of bended-tabular rents contain matter which is similar to that of the formation in which they exist; but some of them contain matter very different to it. Hence in treating of the matter in these rents, a very natural arrangement is to divide them into such as contain matter which proceeded from that contiguous to them, and such as have been filled with matter that entered them at the earth's surface. Again, as the matter in all bended-tabular rents which contain matter similar to that of the formation in which they exist is arranged in the shape of tabular masses accompanied with crystals; and sometimes with variously shaped masses confusedly mixed together; and as all the rents which contain tabular masses contain *earthy* tabular masses, but only some of them *metallic* tabular masses, I divide them into those which contain *earthy*, and into those which contain *earthy* and *metallic* tabular masses.

##### FIRST DIVISION.

*The Arrangement of that Matter in Bended-Tabular Rents which has proceeded from the Matter on the Sides of these Rents.*

##### 1. *Of the Matter in Rents containing only Earthy-Tabular Masses.*

In bended-tabular rents filled with earthy matter only, but which has proceeded from the matter of the formation in which these rents exist, two sorts of earthy-tabular masses are found. The

masses of one sort vary in denomination as do the strata ; but those of the other are generally of one denomination throughout a rent, and their arrangement is very different from that of the first sort. These masses I divide, for reasons which will afterwards appear, into the first-formed and second-formed tabular masses.

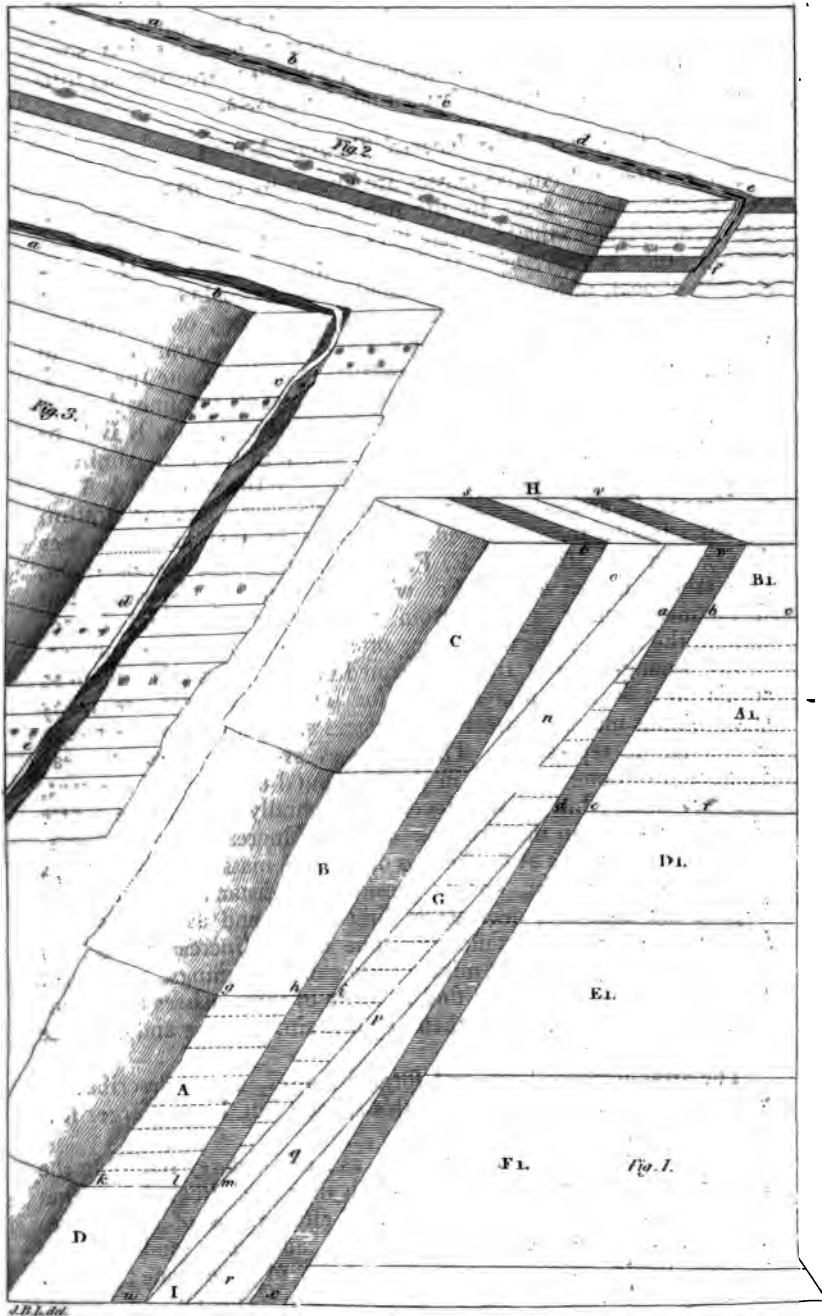
*Of the First-Formed Tabular Masses.*

The first-formed tabular masses are known by the names of vein-stones, ribs, walls, &c. In the parts of these rents where the first-formed tabular masses exist very regularly arranged, the following order is observed. If A A 1, fig. 1, Plate XXXII., be a stratum of coal, in the rent H I a tabular mass of coal G is so situated that its under side *m d* lies between two lines *d, m*, one parallel to and opposite the line which joins the surfaces of separation *b'e* to the under side *e f*, and the other parallel to and opposite the line *l*, and its upper side *a i* is similarly disposed towards the lines *b, h*, as is its under side towards the lines *e, l*. If the stratum B B 1 be slate-clay, it has a tabular mass of slate-clay *n* in the rent, similarly situated with respect to it, as the mass G is to the stratum A A 1 ; the stratum C, and every other stratum in the rent above the stratum A, has a tabular mass arranged in the same manner as the masses G O ; and the strata D, E, F, and all the strata below them, have tabular masses *h, q, r*, &c. which are similarly disposed. But all the masses differ as much from one another in external characters, as the strata from which they proceed differ from one another.

The arrangement of the first-formed tabular masses is seldom so regular as it is here represented : for sometimes there are many small tabular masses between the surfaces of separation of the strata, as *f e*, fig. 2, represents. These masses are sometimes at great distances from one another, in the horizontal direction of the rent, and very small ; but as we proceed horizontally towards the centre of the masses, as in the direction *a b*, they increase in dimensions, and at the centre, as at *b*, there is one large mass, which perhaps fills the whole height, and a considerable distance horizontally. Beyond this mass they decrease in dimensions, and as the distance from it increases, the distance from one another increases also ; till at last they become what miners aptly call "grimings." Beyond this place, as beyond *c*, we find no more of these masses for a considerable distance *c d* ; they then begin again, increase and decrease in magnitude as before.

The arrangement of the first-formed masses, as just described, is very perfect in all small rents of this shape ; but in the larger it is seldom perfect.

The first-formed tabular masses, as I said before, resemble the matter contiguous to the rents in which they are situated ; so that in the coal formation they consist of alternations of coal, slate-clay, white sand-stone, &c. ; in the red, and white sand-stone, of red and white sand-stone ; in lime-stone, of lime-stone ; and in granitic, gneis, sienite, &c. of these rocks respectively.





When we see these tabular masses lying in a rent of the coal formation, between the surfaces of separation of the strata on the sides of that rent whose matter is the same as theirs; when in addition we recollect, as before stated, that these masses in granitic rents are granite, &c. we are irresistibly led to the conclusion that these masses are a part of the matter which existed in the formations before these rents took place; because had they proceeded either from above or below, they could not have assumed their present "*highly inclined positions*;" nor is it probable that they would be similar to the matter near the rents; and it is still less probable that where the matter of a formation consists of strata of different denominations, these masses would lie close to strata whose denominations are similar to theirs. These remarks are sufficiently conclusive: but there is one fact still which gives them greater weight, and that is, that parts of the coal from which these masses are supposed to proceed, are wanting to such distances from these rents, that the area of the parts wanted is about equal to that of the large masses in the rents. Therefore I think the position, that the matter thus arranged in rents proceeds from the matter which is contiguous to them, is fully proved.

The process by which these masses have acquired their present situations and positions may have been similar to that which is described as follows. After the formation of the separated surfaces of the stratum in which the lowest extremity of a rent is situated, the parts of that stratum which were still in a fluid state passed naturally into the rent, or were forced into it by the incumbent weight as the rent gradually increased in width. Several parts of the strata close to the rent would be fluid, therefore near the whole of it was filled opposite these parts; in other parts only small portions of the strata were fluid, of course only small parts entered the rent; and in other parts the strata were sufficiently solid to resist the weight of the incumbent matter, so that from these parts no matter was forced into the rent. It is easy to conceive that such a modification in the arrangement of the masses as that already described would take place in this manner. From the angular position of the masses in a bended-tabular rent, and from the other phenomena already described respecting them, I deduce the formation of the rent in the stratum opposite its lowest extremity so long before its commencement in the second stratum as to allow the matter which entered the rent from the lowest stratum to be so much consolidated, that it would retain its position as the rent continued to widen, and would support the fluid matter which rested on it when the formation of the rent in the second stratum from the bottom commenced; and the commencement of the rent's formation in every stratum, reckoning upwards, before its commencement in that stratum which rests on it, such a length of time as I have mentioned to take place between the commencement of the rent's formation in the lowest stratum, and of its commencement in that stratum which lies on it.

The foregoing description of the first-formed tabular masses, and their mode of arrangement, is of a general application: but many modifying circumstances are to be taken into the question in giving an account of all the variations in their arrangement; but a description of them would be too long for this essay.

*Of the Second-Formed Earthy-Tabular Masses.*

The second-formed tabular masses are known by the names of sticking, dowk, flookans, &c. These masses always resemble the softer parts of the matter contiguous to the bended-tabular rents; and consist chiefly of that part of it which is the easiest suspended mechanically in water. They, therefore, generally contain a large portion of clay. In the coal formation they resemble slate-clay, alum-slate, &c.; but in general they are much softer, and sometimes have the consistence of common clay. I have never met with them in lime-stone but in Derbyshire, and there they have proceeded from the slate-clay which lies on the lime-stone. In granite, gneis, and sienite, they consist of soft, greyish, greenish, or yellowish-white clayey matter, just as the colour of the felspar of these rocks may vary. In serpentine they sometimes constitute the substance called soap-rock. In Cornwall they are called flookans; and Price describes a flookan, in his *Mineralogia Cornubiensis*, "as a tenacious and glutinous earth or clay, that sometimes runs withoutside some veins, immediately between either wall of the lode and the lode itself, and more frequently adhering to the hanging or superior wall, and sometimes mixed in and throughout the lode itself."

The second-formed tabular masses are generally situated between the upper side of the rent and the first-formed tabular masses, and sometimes between these masses and the lower side of the rent. They lie parallel and close to the sides, and never stretch across the rent, like the first-formed tabular masses, except when they change from one side to another, which is very seldom. The unshaded parts *a, b, c, d, e*, fig. 3, represent these masses. They do not continue the whole length of a rent, but are frequently interrupted by the first-formed masses, and by the closing of the sides of a rent. Hence the spaces in the rents which contain these masses are of tabular shapes, which are of various figures and dimensions. These spaces are generally connected together by channels, which are sometimes very small. But although the greatest part of the matter is arranged in tabular masses which lie near the sides of these rents, yet the rest has adapted itself to every inequality in the surfaces of the first-formed tabular masses, and to those in the sides of the rents; and has also completely filled every cavity in the first-formed masses, and in the matter contiguous to the rents; to which they could gain access.

As the nature and position of the second-formed are so different to those of the first-formed tabular masses, these masses could not both acquire their situations at the same time; but as the former

have adapted themselves to the inequalities of the latter, those must have been in a fluid state when these were solid. Hence the first obtained their situations after the second sort of masses. With the help of these observations, it is easy to account for the forming cause of the second-formed tabular masses. As the bended tabular rent continued to increase in height and width, fluid matter entered it, and the first-formed tabular masses were produced in the manner already described; but after a certain length of time, no more matter was separated from that contiguous to the rent; yet by the contraction of this matter, the rent continued to widen, and the first-formed masses contracted also, so that hollow spaces were formed in it. These spaces could not lie without some interruptions as to their existence opposite the whole of the superficial area of a rent; because some of the first-formed masses must lie against the sides of the rent, to retain their situations, and to support others which did not rest against the sides. The spaces so formed were gradually and slowly filled up in the *manner of a clayey sediment*, and thus produced the second-formed tabular masses.

2. *Of the Matter in Rents containing Earthy and Metallic-Tabular Masses.*

The arrangement of the earthy-tabular masses in rents which contain metallic-tabular masses is the same in every respect as in rents filled only with earthy-tabular masses, with the addition of being associated with the metallic masses which are found sometimes on the sides and middle of the rent, and which are subject to the same variation of thickness, and want of continuity, as the earthy-tabular masses. There is one circumstance in the arrangement of metallic matter which is peculiar to formations consisting of alternating strata of different denominations. The metallic masses are not found in rents opposite strata of every denomination, but only opposite one or two. Thus, in the mining district round Alston, in Cumberland, the strata consist of white sand-stone, lime-stone, coal, slate-clay, &c.; but the lead ore is only found in lime-stone and sand-stone, and never opposite the rest; except when the first-formed masses of lime-stone or sand-stone are situated in a rent, a little lower than the strata from which they proceed; then they sometimes have masses of lead ore adhering to them; and when the lead ore is found in small masses mixed throughout similar masses of lime-stone, sand-stone, and clay-slate, it is sometimes lying opposite the clay-slate strata; but here it is evident, from the lime-stone and sand-stone masses which accompany it, that it has fallen from opposite the lime-stone and sand-stone strata. The same peculiarity takes place in the Derbyshire mines. There the lead ore is found only opposite the lime-stone, and not opposite the amygdaloid, or the slate-clay strata, except when accompanied with lime-stone masses.

As there are no metallic strata opposite most of the bended-

tabular rents from which the metallic matter could proceed, and as the arrangement of this matter in these rents is such as to show most distinctly that it has not been deposited from a solvent which might once cover the earth, nor has been forced into its present situation from below in a fluid state ; so its existence in these rents embarrasses very much the speculator on the source of the matter in them. But as the metallic masses are associated with the earthy, which I have shown to proceed from the contiguous matter, I would be very much inclined to suppose that the metallic matter has the same source. But although the mechanical part of the process admits of proof, yet the chemical part is difficult to reconcile with the present principles of chemistry. Still, however, from my knowledge of the mutual arrangements of the earthy and metallic-tabular masses, I will venture to hazard the conjecture, that some profound chemist will discover much nearer relations between the internal properties of metallic and earthy matter than we are at present aware of.

#### SECOND DIVISION.

##### *The Arrangement of that Matter in Bended-Tabular Rents which entered them at the Earth's Surface.*

Although the matter in the most of bended-tabular rents is similar to, and has proceeded from, that in which they are situated, yet into some of them it has entered from the earth's surface, in either a fluid or a solid state.

The matter which entered these rents from above in a fluid state is green-stone, basalt, porphyry-slate, &c. Many of the rents so filled contain throughout matter of one denomination ; but a few of them of two or three denominations. Rents filled from above with the matter of green-stone, basalt, &c. are frequently found in the coal, red, and white sand-stone formations, and are abundant in many parts of Scotland, in these and some other formations.

When the rents so filled were formed, the matter in which they are situated was so near its present state of solidity that no matter from that source could be forced into them by the incumbent weight. They, therefore, remained empty, till fluid substances, by a part of which they are filled, travelled over them to take their situations in lower parts of the same formations, or in others which are on lower levels ; and in doing so, matter entered, and then filled them to the earth's surface.

Bended-tabular rents, filled with matter which entered them in a solid state, contain clay, sand, gravel, and stones, either separate or mixed together. A few of them are found in almost every formation of coal. Such rents were formed at the same time as those containing green-stone, &c. ; but as these fluid substances did not travel over them, they remained empty till filled by the process which produced the alluvial matter, by a part of which they are filled.

### Of the Straight-Tabular Rent.

This rent is straight in both directions. Hence its name. Its position is always at right angles to that of the strata. Hence it is generally either perpendicular, or angular with a great ratio of angularity, or approaching nearly to a perpendicular line. There is no alteration in the position of the strata contiguous to it, as any stratum on one side is opposite its corresponding part on the other. This is a circumstance that at first sight distinguishes it from the bended-tabular rent. It is filled with matter that entered it from above; such as the matter of green-stone, basalt, &c. or clay, stones, sand, and gravel.

This rent was formed when the earth's matter was so near its present state of solidity, that the *inequality* in its contraction was so very small as not to be discernible. Hence we find the strata on the same level on both sides of it.

The cylindrical and ovalar rents I will not at present describe, as they are not very important phenomena in a geognostic point of view. They are very well described in William's Mineral Kingdom, under the names of pipe and flat veins.

In my next communication I will describe the *junctions* of tabular rents one with another.

#### Errata in Mr. Longmire's first Communication on Rents.

Read as follows the sentence which begins on the 42d line of the 84th page:—  
“And Professor Jameson mentions two near Freyberg, in Germany; one of which is more than four miles in its *horizontal direction*, and the other more than ten miles in this direction.”

The letter *w* in fig. 1, Plate XXIX., ought to have been placed close to the highest extremity of the rent, *v*, *u*, *h*.

In fig. 3 of the same plate some letters are misplaced: these, however, will be best corrected by repeating the figure in a future plate.

## ARTICLE V.

*Experiments upon Green Uran Mica, with a view to its Chemical Analysis.* By the Rev. W. Gregor.

ANY general account of the external character and appearance of a mineral that is so well known would be superfluous. A *scientific* description, that can be useful or interesting, must be left to the experienced crystallographer.

The subject of the following observations was raised in the copper-mine called Gunnis Lake, in the eastern extremity of the county of Cornwall.

The crystalline laminæ are, in general, very free from extraneous substances. Small fragments of quartz, however, and a light ochrey

matter, easily separable by washing, are sometimes found adhering in small quantities to them.

The purest pieces were carefully selected for experiment.

The specific gravity of this fossil, taken at the temp. 52 of Fahr. was 3.3.

A portion of it, reduced to a fine powder, was exposed, in a small glass retort, to an open fire. A dew soon collected on the neck of the retort, and passed into the receiver. It was pure water.

100 grains, that had been exposed to a low red heat for ten minutes in a platinum crucible, weighed whilst still warm = 84.6. The mineral, therefore, had lost 15.4 per cent. Another portion exposed to a stronger heat for one hour was diminished, in respect of weight, very nearly in the same proportion.

The mineral, after ignition had lost its brilliancy, and had assumed the appearance of fine brass filings. 100 grains, which had not been ignited, in the state of a fine powder, were put into a matrass, and covered with pure nitric acid, moderately diluted with water. The acid produced no effervescence, and whilst it was unaided by heat, it seemed to act very feebly upon the mineral for many hours. The vessel was then placed in a digesting heat. Yet the solution proceeded very slowly, and was frequently interrupted by the deposition of a green crust, which from time to time covered the bottom of the vessel, and protected the mass from the action of the acid. Boiling water produced little or no change on this crust. It became necessary, therefore, repeatedly to add fresh portions of nitric acid in order to dissolve it. At last, by means of an excess of acid, a solution of the mineral was effected, except a very small quantity of residuary matter, which after ignition = 0.1 gr. It was a mixture of quartz and oxide of iron; and as it was extraneous, I dissolved an equal quantity (0.1 gr.) of pure uran mica in nitric acid, and added the solution to the former one.

The solution was light green. I now poured into it ammonia in excess, which separated a yellowish-white matter. The blue supernatant fluid was carefully poured off from the subsided precipitate, and fresh portions of ammonia were repeatedly supplied, and the matrass containing it was from time to time shaken, and at last exposed to a digesting heat.

When the last effusions of ammonia, aided by the heat of the sand-bath, showed not the slightest indications of copper, but were poured off absolutely colourless,\* the undissolved matter was mixed with distilled water, and thrown upon a filter, and sufficientlyedulcorated. What was thus separated by the filter was gradually

\* When in a former analysis I operated upon the same quantity of uran mica which had been ignited, I found that the last portions of ammonia, when they had ceased to be tinged blue, became of an opal hue, and that a considerable quantity of distilled water passed through the filter before it became perfectly transparent. In this case nearly two per cent. of uran oxide were suspended by the ammonia.

dried, collected, and ignited. It was weighed whilst *still warm*, and it amounted to 74.9 grains. It was of a light yellow colour (A). This I had reason to consider as oxide of uranium; but subsequent experiments proved that this amount must be subjected to some slight corrections.

II. The ammoniacal solution of copper, with the water of edulcoration, was evaporated gradually to dryness;\* and with a view of ascertaining whether it contained any of the oxide of uranium, I redissolved the mass, which remained, in nitric acid, and treated the solution, as before, with ammonia. A very small quantity of yellowish-white matter remained undissolved; which after ignition = 0.2. It was oxide of uranium, and must therefore be added to 74.9.

The ammoniacal solution of copper was now evaporated to dryness, again dissolved in nitric acid, and boiled with liquid potash, which precipitated the copper in the state of oxide, which after ignition = 7.65.

III. The 74.9 gr. of supposed oxide of uranium were moistened with distilled water, and some *test sulphuric acid* was gradually poured upon the mass, and the vessel containing it was placed in warm sand. The mass soon became dark green, and resembled in consistency, as well as in intensity of colour, the pigment called *sap-green*, when suspended in water. What the acid had not dissolved appeared gelatinous; and when warm water was poured upon it, and it was stirred with a glass rod, it separated into lumps somewhat resembling spermaceti. They were encompassed by a greenish fluid, and required repeated effusions of sulphuric acid for their solution. A greenish-white matter resisted the acid. It was carefully collected, dried, and ignited; it = 0.6, treated with carbonate of soda, in a platinum crucible, and subsequently with nitric acid, &c. It proved to consist chiefly of oxide of uranium, with a very small portion of oxide of lead, too small, indeed, to admit of being accurately weighed.† Upon dropping ammonia into the solution of the oxide of uranium, which I had effected by means of sulphuric acid, I was surprised to find that it was tinged blue, indicating thereby the presence of copper. The same process with ammonia, before detailed, was repeated, except that at last the copper was dissolved in sulphuric acid, and separated in a metallic state by a cylinder of zinc. The copper thus obtained, after having been edulcorated, and shaken with very dilute muriatic acid, and carefully dried, weighed 0.5, which, with the accession of oxygen in the ratio of 25 per cent., is equivalent to 0.62 of oxide. The 74.9 grains of supposed uran oxide must therefore be diminished by the subtraction of 0.62 of oxide of copper = 74.28, which,

\* In a former experiment upon 100 gr. of this mineral I evaporated the cupreous ammoniac to dryness, and exposed the dry mass to a red heat, but I could not detect the slightest trace either of potash or soda.

† In my former analysis I detected a small quantity of oxide of lead also.

with the accession of 0.2 of uran oxide, that was mixed with the copper in the ammoniacal solution, will constitute the whole amount of uran oxide 74.48. The 7.65 grs. of copper must be increased by 0.62 = 8.27.

After the last separation of copper, I could not by any methods discover any substance in the residuum but oxide of uranium. The relative amount of the several ingredients of uran mica stated, at one place of decimals, will be as follows :—

Oxide of uranium, with a trace of oxide of lead..	74.4
Oxide of copper .....	8.2
Water .....	15.4
Loss .....	2
	<hr/> 100

This statement agrees so very nearly with the result of a former analysis of the same quantity of uran mica, that I do not think it necessary to set down the average mean of the two operations.

The copper exists in this fossil in a much larger proportion than it was conceived to be.

I should not, probably, lay much stress upon the minute quantity of oxide of lead which I found in this fossil, if I had not recollected that I had some years ago detected the same oxide as more decidedly constituting an ingredient of the *yellow uran mica*.

The ease with which this fossil parts with its water in a low red heat is a circumstance that will probably prevent its being denominated a hydrate of uranium and copper.

*Creed, Feb. 27, 1815.*

W. GREGOR.

## ARTICLE VI.

*Register of the Weather in Plymouth for the last Six Months of 1814.* By James Fox, jun. Esq. With a Plate.

### JULY.

Date.	Wind.	Rain.	Observations.
1814.			
July 1	Var.	0.01	Light rain, morn; fair day.
2	Ditto	—	Fair.
3	Ditto	—	Ditto.
4	Ditto	—	Cloudy morn; fair day.
5	Ditto	—	Fair.
6	Ditto	—	Ditto.
7	W S W	} 0.15	Cloudy; light rain.
8	S S W		Light rain, morn; cloudy day.
9	Ditto		Cloudy.
10	S		Fair.

# THERMOMETER

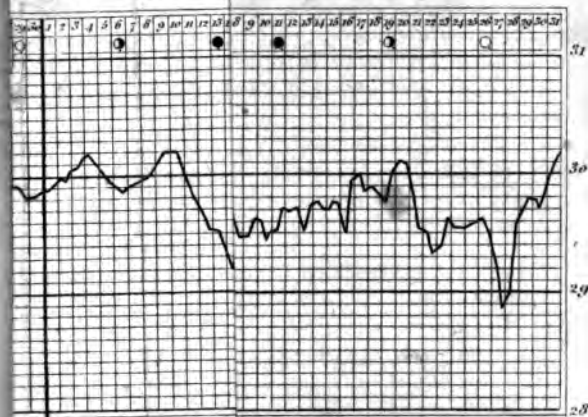
2 feet above the level of the

Page 234.

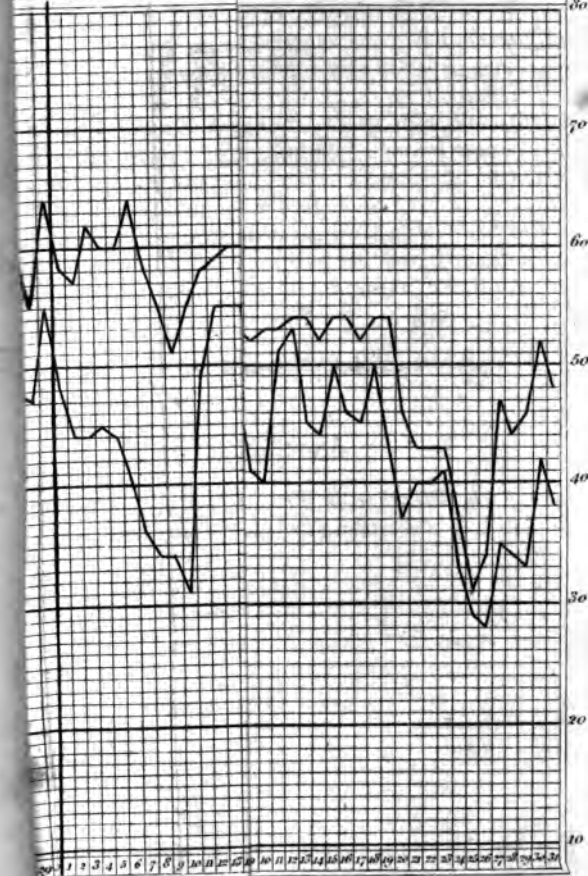
THERMOMETER.

O

December



THERMOMETER.





Date.	Wind.	Rain.	Observations.
814.			Fair.
dy 11	W to NW		Ditto.
12	Var.		Ditto day; cloudy at night.
13	NW		Cloudy day; light rain at night.
14	Ditto	1.02	Cloudy morn; heavy rain, afternoon.
15	SW to SE		Cloudy morn; fair eve.
16	NW		Fair morn; misty eve.
17	SSW		Cloudy and fair; ditto, eve.
18	NW		Fair morn; heavy showers, eve.
19	S to W	0.36	Fair.
20	SSW to NW		Fair morn; misty eve.
21	WSW	0.01	Fair.
22	Ditto		Ditto.
23	S to E		Ditto; high wind.
24	E		Cloudy and fair; sultry.
25	S		Ditto; light rain, morn.
26	S to E	0.43	Ditto, ditto.
27	ENE to E		Heavy rain, morn; cloudy eve.
28	ESE		High wind; shower, morn; fair noon;
29	SW to WNW	0.05	misty eve.
30	NNW		Cloudy and fair.
31	S to NE		Fair day; cloudy at night.
		2.03	Inches rain.

Barometer: Greatest height.....	30.22 inches	Wind. E
Lowest .....	29.60	NW
Mean .....	29.917	
Thermometer: Greatest height.....	76°	E
Lowest .....	47	Var.
Mean .....	63.31	

## AUGUST.

Aug. 1	WNW to W		Cloudy and fair.
2	S		Fair.
3	WNW		Misty morn; fair day.
4	S	0.19	Fog, morn; fair day; light rain, night.
5	WSW		Showers, and high wind.
6	WNW		High wind; heavy ditto morn; cloudy
7	SW	0.76	and fair day.
8	WNW		Ditto; cloudy morn; showers, eve.
9	Ditto		Ditto; showery day.
10	NW	0.12	Ditto; cloudy morn; showers, eve.
11	SW		Showers, morn; cloudy eve.
12	WSW		Cloudy and fair.
13	SW to NW		Misty morn; fair day.
14	WSW to NW	0.20	Light rain, morn; cloudy and fair.
15	WSW	0.21	Ditto, morn; fair afternoon.
16	NW		Showers.
17	WSW	0.20	Cloudy and fair.
18	WNW		Misty; light rain.
19	NW		Ditto, morn; cloudy and fair eve.
20	SW to S	0.09	Cloudy morn; showers, noon; fair eve.
21	S to WNW	0.36	Showery day; high wind, eve.
		0.05	Fog, morn; cloudy and light rain.

Date.	Wind.	Rain.	Observations.
1814.			
Aug. 22	ENE to SE	0·01	Cloudy, and a shower.
23	S to SW	0·49	Heavy rain, morn; fair afternoon.
24	S	0·06	Shower, morn; ditto, ditto.
25	WNW	0·09	Cloudy, and showers.
26	NNW		Ditto, and fair.
27	Ditto		Ditto, ditto.
28	Var.		Ditto, ditto.
29	Ditto		Fair.
30	Ditto	0·01	{ Cloudy morn; fair afternoon; shower eve.
31	Ditto		Fog, morn; fair day.
		2·84	Inches rain.

Barometer: Greatest height.....	30·29 inches	Wind.
Lowest .....	29·64	Var.
Mean .....	29·993	S
Thermometer: Greatest height.....	78°	W
Lowest .....	46	SW
Mean .....	61·8	

## SEPTEMBER.

Sept. 1	E		Fair.
2	ENE		Ditto.
3	Ditto		Ditto.
4	Ditto		Ditto.
5	Var.		Fog, morn; fair day.
6	WNW		Ditto, ditto; cloudy at night.
7	NW	0·10	Showers.
8	ENE to S		Cloudy and fair.
9	Var.		Fair.
10	Ditto		{ Ditto; very dark clouds in the E. and S. at night.
11	ENE		Fair.
12	Var.		Misty morn; fair afternoon.
13	NE		Fair.
14	E		Ditto.
15	E		Ditto; high wind.
16	E		Ditto.
17	E		Ditto.
18	S to ENE		Ditto, morn; cloudy eve.
19	SSE		Fair.
20	S		{ Cloudy, and a shower.
21	SSW to WSW	0·10	{ Fair morn; showers, afternoon.
22	WSW		Cloudy and fair.
23	SW to SSW	0·23	Heavy showers.
24	SSE to NW	0·92	Heavy rain, morn; showery day.
25	SSW		Fair day; cloudy at night.
26	SSE	0·57	{ A gale of wind, and heavy rain, morn hail and thunder storm at night.
27	SW to SE	0·05	A shower, morn; fair day.
28	Var.	0·02	Misty morn; fair day.
29	ENE		High wind; cloudy.
30	Ditto		Ditto, ditto; ditto.
		1·99	Inch rain.

		<i>Wind.</i>
Barometer: Greatest height.....	30.32 inches	E
Lowest .....	29.53	N W
Mean .....	30.02	
Thermometer: Greatest height .....	74°	S S E
Lowest .....	41	N E
Mean .....	59.2	

OCTOBER.

date.	Wind.	Rain.	Observations.
814.			
Oct. 1	E N E		Very high wind; cloudy and fair.
2	Ditto		High wind; ditto.
3	Ditto		Ditto; fair.
4	E		Fair.
5	Ditto		Fog, morn; fair day.
6	E to N N W		Cloudy and fair.
7	N W to N E		Fog, morn; fair day.
8	Var.		Fair.
9	N E		Ditto.
10	E to S		Hoar frost, fog, and fair morn; cloudy afternoon.
11	S S W		High wind; cloudy.
12	S to E S E	0.21	Ditto; showery.
13	S E		Ditto; showers at night.
14	E S E to W N W	0.37	Ditto; showery day.
15	W to S	0.40	Fair morn; high wind, and showers, afternoon.
16	W N W		Fair morn; cloudy afternoon.
17	S	0.28	Showery morn; heavy rain at night.
18	S S W to Var.	0.38	High wind, and very heavy showers.
19	N W	0.28	Ditto, ditto, and hail ditto.
20	W N W		Ditto; fair morn; cloudy eve.
21	Var.		Fog, morn; fair day; cloudy at night.
22	W N W		Fog, ditto; showery eve.
23	N W	0.31	High wind; showery.
24	E N E to S E	0.42	Misty day; a storm, and heavy rain, at night.
25	W N W	0.48	Heavy showers, with hail.
26	E N E		Fog, and fair morn; cloudy eve.
27	Var.		Ditto, ditto, ditto.
28	Ditto	0.30	Showery day.
29	Ditto		Ditto.
30	E S E	0.32	Ditto.
31	E	0.22	Ditto; high wind.
		3.97	Inches rain.

		<i>Wind.</i>
Barometer: Greatest height.....	30.21 inches	E
Lowest .....	29.07	N W
Mean .....	29.771	
Thermometer: Greatest height .....	64°	Var.
Lowest.....	31	E
Mean .....	49.854	

## NOVEMBER.

Date.	Wind.	Rain.	Observations.
1814.			
Nov. 1	E N E		High wind; cloudy.
2	Ditto		Ditto, ditto.
3	Ditto		Ditto, ditto.
4	Ditto		Ditto; cloudy and fair.
5	Var.		Cloudy.
6	N W to N		Hoar frost; fog, morn; fair day.
7	E N E to N W	0.18	{ Cloudy; showers at night.
8	N W		
9	N W		
10	N N E to E		Cloudy.
11	W to S W	0.05	Hoar frost; fair.
12	W N W	0.17	{ Ditto; misty morn; storm at night.
13	W N W		
14	Ditto	0.05	{ High wind, and showers.
15	S W		
16	W N W	0.35	Showers.
17	W S W		Cloudy day; light rain, eve.
18	S S W to N W	0.31	{ Misty day.
19	N W		
20	N W to N N E		
21	N W to N E	0.18	{ Fair morning; showery afternoon.
22	N E to S S E		
23	S E to E N E		
24	S to S W	1.02	{ Fog, ditto; ditto.
25	S W to W		
26	N W	0.17	Cloudy and fair day; fair at night.
27	E to N W	0.14	Hoar frost; fog, morn; fair day.
28	Var.	0.37	Ditto; high wind and showers.
29	W to W N W		Cloudy.
30	N W	0.16	Ditto, morn; heavy rain, afternoon.
			Misty morn; showers, afternoon.
			High wind, and showers.
			Ditto, ditto.
			Thick weather, morn; heavy rain, afternoon.
			Ditto, and high wind.
			Showers.
		3.15	Inches rain.

Barometer: Greatest height.....		30.36 inches	Wind.
Lowest .....		29.23	E
Mean .....		29.771	N W
Thermometer: Greatest height.....		54°	S S W
Lowest .....		24	N E
Mean .....		43.3	

## DECEMBER.

Dec. 1	N W	0.05	Fair morn; showery afternoon.
2	E N E		Hoar frost; fog, morn; cloudy day.
3	E N E to S S E		{ Ditto, ditto; cloudy and fair day; high wind at night.
4	W N W to N W	0.67	
5	N W	0.08	Heavy showers.
6	E N E to S		Showers.
7	N W to W	0.61	Hoar frost; fair morn; cloudy afternoon.
8	S to S W	0.40	High wind, and heavy showers.
			Ditto; a wet day.

Date.	Wind.	Rain.	Observations.
1814.			
Dec. 9	W N W to E N E	0.51	Showers.
10	S E to W	0.51	A wet day; high wind.
11	W to S W	0.31	Ditto.
12	S S W to S W	0.35	Ditto, and violent storm.
13	S W to W S W	0.42	A storm; showery.
14	S W	0.25	Ditto, ditto.
15	S W	0.25	Ditto, ditto.
16	S W	0.28	A violent ditto, ditto.
17	S S W to S W	0.28	A storm, ditto.
18	S W to S S W		A high wind; thick weather.
19	S W to N W		Showers.
20	E	0.41	Thick weather, and showers, morn; cloudy afternoon.
21	E S E		A storm, showers, and sleet.
22	Ditto to N E	1.06	Heavy rain; fair at night.
23	E		Showers, morn; cloudy day.
24	E N E		Cloudy, with light rain.
25	N N E	0.17	Hoar frost; cloudy day.
26	N N W to E S E		Ditto, ditto; showers of sleet.
27	Var. at S.	0.60	Heavy showers.
28	Ditto at N		Cloudy day.
29	Ditto	0.32	Steady rain, morn; misty eve.
30	S to N W	0.41	Showers, morn; fair eve.
31	N W to S E		Cloudy and fair day.
		6.70	Inches rain.

Barometer: Greatest height .....	30.19 inches	Wind.
Lowest .....	28.86	S E
Mean .....	29.688	Var.
Thermometer: Greatest height .....	54°	S W
Lowest .....	28	N N W
Mean .....	43.677	

## ARTICLE VII.

*Magnetical and Astronomical Observations at Hackney Wick,*  
By Col. Beaufoy.

Latitude, 51° 32' 40.3" North. Longitude West in Time 0<sup>h</sup> 1<sup>m</sup> 10<sup>s</sup>.

March 14, Immersion of  $\xi$  Ceti. . 7<sup>h</sup> 9' 15.1" Mean Time at Hackney Wick,  
 Mar. 14, Immersion of Jupiter's { 9 50 36.6 Ditto at Hackney Wick.  
 2d Satellite . . . . . { 9 50 43.4 Ditto at Greenwich.

*Magnetical Observations.*

1815.

Month.	Morning Observ.					Noon Observ.					Evening Observ.	
	Hour.	Variation.				Hour.	Variation.				Hour.	Variation.
Feb. 18	8h 25'	24°	15'	02"		1h 35'	24°	22'	09"			
Ditto 19	8 25	24	14	14		1 35	24	22	42			
Ditto 20	8 25	24	15	03		1 35	24	21	24			
Ditto 21	—	—	—	—		1 25	24	22	25			
Ditto 22	8 30	24	14	56		1 25	24	20	01			
Ditto 23	8 25	24	14	32		—	—	—	—			
Ditto 24	8 45	24	14	16		1 20	24	20	43			
Ditto 25	8 40	24	13	35		1 30	24	20	29			
Ditto 26	8 45	24	15	32		1 25	24	22	01			
Ditto 27	8 35	24	14	51		1 20	24	23	10			
Ditto 28	8 30	24	15	38		1 30	24	23	28			

1815.

Mean of Observations in Feb.	Morning	at	8h 37'.....	Variation	24° 15' 11"	West.
	Noon	at	1 31.....	Ditto	24 21 51	
	Evening	at	—.....	Ditto	— — —	Not obs.
Ditto in Jan.	Morning	at	8 47.....	Ditto	24 16 46	West.
	Noon	at	1 36.....	Ditto	24 20 12	
	Evening	at	—.....	Ditto	— — —	Not obs.
1814. Ditto in Dec.	Morning	at	8 44.....	Ditto	24 18 02	West.
	Noon	at	1 30.....	Ditto	24 20 36	
	Evening	at	—.....	Ditto	— — —	Not obs.
Ditto in Nov.	Morning	at	8 41.....	Ditto	24 16 20	West.
	Noon	at	1 40.....	Ditto	24 20 37	
	Evening	at	—.....	Ditto	— — —	Not obs.
Ditto in Oct.	Morning	at	8 39.....	Ditto	24 14 06	West.
	Noon	at	1 42.....	Ditto	24 21 45	
	Evening	at	—.....	Ditto	— — —	Not obs.
Ditto in Sept.	Morning	at	8 32.....	Ditto	24 14 33	West.
	Noon	at	1 39.....	Ditto	24 23 17	
	Evening	at	6 19.....	Ditto	24 16 50	
Ditto in Aug.	Morning	at	8 30.....	Ditto	24 14 13	West.
	Noon	at	1 39.....	Ditto	24 23 48	
	Evening	at	6 57.....	Ditto	24 16 31	
Ditto in July.	Morning	at	8 41.....	Ditto	24 13 29	West.
	Noon	at	1 42.....	Ditto	24 23 44	
	Evening	at	6 58.....	Ditto	24 17 00	
Ditto in June.	Morning	at	8 44.....	Ditto	24 13 10	West.
	Noon	at	1 39.....	Ditto	24 22 48	
	Evening	at	6 52.....	Ditto	24 16 29	
Ditto in May.	Morning	at	8 45.....	Ditto	24 13 12	West.
	Noon	at	1 44.....	Ditto	24 22 13	
	Evening	at	6 38.....	Ditto	24 16 14	
Ditto in April.	Morning	at	8 45.....	Ditto	24 12 53	West.
	Noon	at	1 48.....	Ditto	24 23 53	
	Evening	at	6 29.....	Ditto	24 15 30	
Ditto in March.	Morning	at	8 53.....	Ditto	24 14 29	West.
	Noon	at	1 52.....	Ditto	24 23 08	
	Evening	at	6 11.....	Ditto	24 15 33	
Ditto in Feb.	Morning	at	8 47.....	Ditto	24 14 50	West.
	Noon	at	1 52.....	Ditto	24 20 58	
	Evening	at	—.....	Ditto	— — —	Not obs.

Mean of	Morning	at	8 <sup>h</sup> 59' ....	Variation	24° 15' 05"	} West.
Observations	Noon	at	1 53 ....	Ditto	24 19 03	
in Jan.	Evening	at	— — — —	Ditto	— — —	
	Morning	at	8 53 ....	Ditto	24 17 39	} West.
1813.	Noon	at	1 51 ....	Ditto	24 20 30	
Ditto in Dec.	Evening	at	— — — —	Ditto	— — —	
	Morning	at	8 42 ....	Ditto	24 17 17	} West.
Ditto in Nov.	Noon	at	1 54 ....	Ditto	24 20 24	
	Evening	at	— — — —	Ditto	— — —	
	Morning	at	8 45 ....	Ditto	24 15 41	} West.
Ditto in Oct.	Noon	at	1 59 ....	Ditto	24 22 53	
	Evening	at	— — — —	Ditto	— — —	
	Morning	at	8 53 ....	Ditto	24 15 46	} West.
Ditto in Sept.	Noon	at	2 02 ....	Ditto	24 22 32	
	Evening	at	6 03 ....	Ditto	24 16 04	
	Morning	at	8 44 ....	Ditto	24 15 55	} West.
Ditto in Aug.	Noon	at	2 02 ....	Ditto	24 23 32	
	Evening	at	7 05 ....	Ditto	24 16 08	
	Morning	at	8 37 ....	Ditto	24 14 32	} West.
Ditto in July.	Noon	at	1 50 ....	Ditto	24 23 04	
	Evening	at	7 08 ....	Ditto	24 16 43	
	Morning	at	8 30 ....	Ditto	24 12 55	} West.
Ditto in June.	Noon	at	1 33 ....	Ditto	24 22 17	
	Evening	at	7 04 ....	Ditto	24 16 04	
	Morning	at	8 32 ....	Ditto	24 12 02	} West.
Ditto in May.	Noon	at	1 37 ....	Ditto	24 20 54	
	Evening	at	6 14 ....	Ditto	24 13 47	
	Morning	at	8 31 ....	Ditto	24 09 18	} West.
Ditto in April.	Noon	at	0 59 ....	Ditto	24 21 12	
	Evening	at	5 46 ....	Ditto	24 15 25	

*Magnetical Observations continued.*

1815.

Month.	Morning Observ.			Noon Observ.			Evening Observ.	
	Hour.	Variation.		Hour.	Variation.		Hour.	Variation.
March 1	8 <sup>h</sup> 20'	24° 15' 35"		1 <sup>h</sup> 30'	24° 23' 32"		Not observed.	Not observed.
Ditto 2	8 35	24 14 45		1 45	24 20 08			
Ditto 3	—	—		1 40	24 22 06			
Ditto 4	8 35	24 14 43		1 50	24 22 20			
Ditto 5	8 35	24 13 17		1 20	24 25 32			
Ditto 6	8 35	24 16 51		1 35	24 24 41			
Ditto 7	8 40	24 14 47		1 25	24 22 26			
Ditto 8	8 25	24 18 25		1 15	24 23 06			
Ditto 9	8 35	24 15 43		1 15	24 24 17			
Ditto 10	8 25	24 15 51		1 30	24 21 55			
Ditto 11	8 45	24 18 32		1 30	24 22 48			
Ditto 12	8 40	24 14 22		1 20	24 20 35			
Ditto 13	8 35	24 13 33		1 25	24 22 28			
Ditto 14	—	—		1 25	24 25 16			
Ditto 15	—	—		1 25	24 25 52			
Ditto 16	8 40	24 14 12		1 25	24 25 52			
Ditto 17	8 30	24 16 47		1 30	24 23 01			

Feb. 20.—The wind blew very hard and squally from the west, with hard showers; and the needle vibrated at intervals 28' 30"; and, what is remarkable, the needle, comparatively speaking, has been steady to the present day, March 17.

Rain fallen { Between noon of the 1st Feb. } 0.830 inches.  
 { Between noon of the 1st Mar. }

## Comparison of the Variations in the Years 1813, 1814, and 1815.

		1813.	1814 and 1815.	Difference.
April	Morning .....	24° 09' 18"	24° 13' 53"	+ 3' 35"
	Noon .....	24 21 12	24 23 53	+ 2 41
	Evening .....	24 15 25	24 15 30	+ 0 05
May	Morning .....	24 12 02	24 13 12	+ 1 10
	Noon .....	24 20 54	24 22 13	+ 1 19
	Evening .....	24 13 47	24 16 14	+ 2 27
June	Morning .....	24 12 35	24 13 10	+ 0 35
	Noon .....	24 22 17	24 22 48	+ 0 31
	Evening .....	24 16 04	24 16 29	+ 0 25
July	Morning .....	24 14 32	24 13 29	- 1 03
	Noon .....	24 23 04	24 23 44	+ 0 40
	Evening .....	24 16 43	24 17 00	+ 0 17
Aug.	Morning .....	24 15 55	24 14 13	- 1 42
	Noon .....	24 23 32	24 23 48	+ 0 16
	Evening .....	24 16 08	24 16 31	+ 0 23
Sept.	Morning .....	24 15 46	24 14 33	- 1 13
	Noon .....	24 22 32	24 23 17	+ 0 45
	Evening .....	24 16 04	24 16 50	+ 0 46
Oct.	Morning .....	24 15 41	24 14 08	- 1 33
	Noon .....	24 22 53	24 21 45	- 1 08
	Evening .....	— — —	— — —	— — —
Nov.	Morning .....	24 17 17	24 16 20	- 0 57
	Noon .....	24 20 24	24 20 37	+ 0 13
	Evening .....	— — —	— — —	— — —
Dec.	Morning .....	24 17 32	24 18 02	+ 0 23
	Noon .....	24 20 30	24 20 36	+ 0 06
	Evening .....	— — —	— — —	— — —
Jan.	Morning .....	24 15 05	24 16 46	+ 1 41
	Noon .....	24 19 03	24 20 12	+ 1 09
	Evening .....	— — —	— — —	— — —
Feb.	Morning .....	24 14 50	24 15 11	+ 0 21
	Noon .....	24 20 58	24 21 51	+ 0 53
	Evening .....	— — —	— — —	— — —

## Errata in Comparison of Variation.

May, morning observation, for 12' 49" read 13' 12"; difference for + 0' 41" read + 1' 10".

November, noon observation, difference for - 0' 13" read + 0' 13".

January, morning observation, for 16' 26" read 16' 46".

## ARTICLE VIII.

Reply to Mr. Hume's Answer to Mr. R. Phillips's Animadversions.\*

(To Dr. Thomson.)

SIR,

MR. HUME describes me as unfortunate in having noticed an error he had committed, because I was not aware that it had been

\* *Annals*, p. 116 of the present volume.

previously detected and exposed by you. To be ignorant is unquestionably a misfortune; but he is surely less censurable who is unacquainted with one fact, stated by only one author, than he who, ignorant of many discoveries, related by several writers, denies them the credit of priority, and claims it for himself: the former is my case, the latter Mr. Hume's; when candour has done its utmost in allowing him the plea of ignorance.

To your decision, however, Mr. Hume says that he assented long ago; but so silent has been his admission of what he could no longer deny, that even his friends appear not to have known of this sacrifice of fame to truth: for Mr. Parkes, who long "has been even personally acquainted with" Mr. Hume, has admitted his claim to the discovery alluded to, in the last edition of the *Chemical Catechism*; a work deservedly popular, and with which Mr. Hume cannot be unacquainted.

I am "equally unfortunate," according to Mr. Hume, "in the two experiments quoted from M. Sage; and have drawn inferences diametrically opposite to those of all the chemists who have written on the subject, especially those of France." If my inferences are just, it matters not from whom I differ; and it would have been more to Mr. Hume's advantage, to show my error, than to overwhelm me with a pretended host of opposing authorities; I say pretended, for I confidently challenge him to produce the names of those chemists, who have drawn from M. Sage's experiments inferences diametrically opposite to mine. M. Sage says "Nitrous acid of 32 degrees occasions at first a brisk effervescence with aerated heavy spar; the nitre which results, requiring much water for its solution, precipitates as soon as it is formed." "(se precipite aussitôt qu'il se forme.)" Now the inference which I drew from this experiment, is "that nitrate of barytes is perfectly insoluble in nitrous acid of the usual specific gravity;" to copy the terms in which Mr. Hume has claimed priority of discovery, but omitting indeed the words in which he speaks of an insoluble solution; a thing much beyond my comprehension, and to the discovery of which I suppose he will be allowed an uncontested right.

The other experiment quoted from Sage, is, that "marine acid dissolves aerated heavy spar with effervescence, the salt which results precipitates immediately. The supernatant marine acid does not hold in solution (ne tient point en dissolution) any salt with a base of heavy earth." The fact expressed in the quotation with a precision that no sophistry can evade, and claimed by Mr. Hume as his own discovery, is "that muriate of barytes is virtually insoluble in muriatic acid."

This appears to me the plain statement of the controversy; but Mr. Hume has attempted to elude these deductions by a truly notable expedient; for it is one which, if it succeed in supplying his present purpose, must inevitably deprive him of the priority of the two discoveries remaining out of seven, which he has claimed in his paper on barytes, in 1802. Mr. Hume asserts that in Sage's experiments, "there is neither water of solution nor water of crys-

tallization;" if this is the case, the acids he employed were concentrated acids. With this view of the subject, let us turn to Mr. Hume's paper in the Phil. Mag. for 1802, in which his fourth and sixth discoveries are thus described: "carbonate of barytes may be entirely changed into nitrate by nitrous acid in its concentrated state."—"Carbonates of barytes may be rendered into muriate by concentrated muriatic acid." Mr. Hume further contends, that there is no ground for asserting, that suffering the muriatic and nitric acids to form their respective salts from the decomposition of the carbonate "is precisely the same as adding their salts, or more particularly their solutions, to the respective acids;" he does not, however, attempt to show the difference; the facts of the case are, I assert, simply these: when crystals are added to the acid they remain undissolved; when a solution is substituted for them, it is decomposed, and crystals are deposited on account of their insolubility; and when water has been put to a strong acid, carbonate of barytes is decomposed, an aqueous solution is first formed in, and then decomposed by, the acid; crystals are formed, and remain undissolved; the manipulation in these cases differs, but the same substances are employed, and the same results produced; and these in every case (again to copy Mr. H.'s words) may be correctly described by stating "that such is the avidity of nitrous and muriatic acids for water, that they will attract even the whole of the water of solution from their respective salts."

Mr. Hume now says that when he published his paper on barytes in 1802, "it had been a common practice to ascertain the purity of nitrous acid, and even to purify it, by dropping into it a solution of nitrate of barytes." This statement, as well as some other parts of Mr. Hume's letter to you, Sir, would induce any one, unacquainted with his controversial style, to suppose that his alleged discoveries were attended with directions for avoiding so erroneous a practice. This, however, is entirely an after-thought; for the fact is, that Mr. Hume's paper on barytes does not contain any notice of the errors which the facts he mentions are likely to occasion; and consequently no "instructions" whatever are offered on the subject of purifying nitric acid. The experiments are related both by Mr. Hume, and by Sage who preceded him, without any reference to this subject; and nothing can be learned from the former which the latter does not supply.

According to the purpose to be answered Mr. Hume puts different constructions upon similar phrases. Thus, in the paper on barytes, the quotation "*Tous les acides minéraux décomposent ce sel,*" meaning "carbonate de baryte," must be translated *diluted acids*; otherwise he must abandon a claim already quoted, but not very clearly made out, to the discovery that carbonate of barytes is decomposable by *concentrated* nitric and muriatic acids: whereas, in his letter, he attempts to show that Lagrange was ignorant that *strong nitric acid* decomposes solution of nitrate of barytes; and to prove this case, the words "*acide nitrique,*" which he quotes from *this author*, must be rendered concentrated nitric acid. Thus Mr.

Hume, to substantiate his own discovery, makes acids *weak*, and to prove another's ignorance they become *strong*.

The fate of so large a proportion of Mr. Hume's claims to originality has naturally made him anxious for the remainder. Concerning the "silver test," he evinces painful forebodings, which are ill concealed by the seeming pleasantry with which they are accompanied. In one of his numerous communications respecting "this excellent test," he says, "the nitrate of silver is also extremely sensible in its operation upon arseniate of potash; and it seems most decidedly to distinguish this salt from the above solution, or arsenite of potash; for the colour of this precipitate is much darker and more inclined to red or brick colour." *Med. and Phys. Journal*, vol. xxii. p. 450.

In his letter to you, Sir, Mr. Hume says, "I shall now expect to be told that I have also been forestalled respecting my test for arsenic, that the arseniate of silver, the *brick red* coloured compound, had been prepared by others, &c.;" and that "M. Klaproth had frequently got hold of it, analyzed it, regenerated the same compound by means of nitrate of silver; but was so cruel and unlucky as to disregard the silver as *a test*, always preferring the *acetate of lead*, even to the end of the second volume of his valuable analytical researches." Mr. Hume is the best judge of the foundation on which this expectation is raised; and that it may not be disappointed, I refer him to the following passage from p. 151, vol. ii. of Klaproth's work; "To be more convinced that this precipitate was an arseniated lead, I drenched it with water, and digested it with half its weight of *sulphuric acid*. The liquor separated by filtration contained uncombined arsenic acid. I neutralized it with soda, and treated one part of it with a solution of nitrated silver. This produced a copious precipitate of arseniated silver, which possessed the brick red colour peculiar to it." By Mr. Hume's indiscreet challenge of inquiry, and triumphant defiance of the result of it, he is reduced to a dilemma; from which, if he extricate himself, he will show that his talent for supporting his claims is commensurate to his facility in making them. If, as must be inferred from the above quotation in his letter, he have read Klaproth's essays, how could he prevail upon himself to publish as his own the discovery that nitrate of silver is a test of arsenic acid; when that author expressly asserts that he employed nitrate of silver "to be more convinced" that a precipitate he had obtained was arseniated lead? If on the other hand he have not read them, how could he venture to assert that Klaproth preferred the use of acetate of lead to that of nitrate of silver?

To many persons invective is a "cheap defence," and it costs no one less than Mr. Hume; in the use of it he is voluble, and seems justly conscious that he may employ it without injury to his reputation. Consistently with this style, he insinuates that I am "an hireling," actuated by "flattery, ambition, malevolence, or jealousy;" that I am "peevish," and evince "much malice and little candour."

Having in my present communication rescued another discovery from Mr. Hume's extensive grasp, and restored it to the rightful owner, I expect from him a repetition of the invectives of which he has been so liberal, with such scanty additions as even his well-stored vocabulary can now afford; but having, as I conceive, answered Mr. Hume's arguments, I shall hold myself excused from entering upon a mode of discussion, in which success is inseparable from disgrace.

I remain yours, very respectfully,

29, Poultry, Feb. 22, 1815.

RICHARD PHILLIPS.

## ARTICLE IX.

*A Memoir on Iodine.* By M. Gay-Lussac.

(Continued from p. 214.)

### *Combination of Iodine with Chlorine.*

Dry iodine rapidly absorbs chlorine, producing a heat which rises to  $212^{\circ}$ . The compound has in some parts a fine orange-yellow colour; in others, an orange-red. The yellow parts contain more chlorine than the red; they are likewise more volatile. Though I passed a great deal of chlorine over the iodine, yet the greatest part was not saturated. We shall see immediately by what characters this point can be determined. To the red compound of iodine and chlorine I give the name of *subchloruret of iodine*, though it does not appear to me to have fixed proportions. To the yellow compound I give the name of *chloruret*.

Both of these compounds speedily deliquesce in the air. The solution of the subchloruret is a deeper orange-yellow the more iodine it contains. The solution of the chloruret is colourless when the excess of chlorine is driven off, and then the mutual saturation of the two constituents seems to be complete. Both solutions are very acid, and destroy the colour of the solution of indigo in sulphuric acid. When the solution of the chloruret is saturated with an alkali, it is changed completely into iodate and hydro-chlorate. When too long exposed to the light, it becomes coloured. It dissolves a great quantity of iodine, and then assumes the characters of subchloruret. Heat disengages chlorine from it, and the iodine being then in excess, the liquid assumes the characters of subchloruret. The solution of the subchloruret is volatilized without decomposition. Light does not produce any further alteration upon it. When saturated with an alkali, it gives iodate and hydro-chlorate; but if the alkali be cautiously added, we obtain a precipitate of iodine, which disappears on the addition of more alkali, and then hydriodate and iodate are formed. Thus the subchloruret is

characterized by the precipitation of iodine when an alkali is added, whereas the chloruret gives no such precipitate.

We obtain but little chloruret in a solid state, as I have already remarked; but it may be obtained with facility, and in great quantity, on solution in water. For that purpose nothing more is requisite than to saturate with chlorine a somewhat diluted solution of subchloruret. It is then exposed for some time to the sun till it loses its colour, or it may be put into a large bottle in which the air is continually renewed. By this means we obtain a very acid colourless liquid, having only a slight smell of chlorine, which destroys the colour of solution of indigo in sulphuric acid, though slowly, and gives an abundant precipitate of iodine when ammonia is poured into it. We cannot employ heat to drive off the excess of chlorine unless it be very moderate; for I have just observed that it converts the liquid into a subchloruret. When we wish to saturate a solution of subchloruret with chlorine, the liquid ought to be dilute; because, when concentrated, the process does not succeed. The subchloruret presents itself frequently, and possesses stability, while the other (to make use of the expression) has only an ephemeral existence.

When we pour hydro-chlorate of potash or barytes into a solution of the chloruret or subchloruret, it gives up its base to a portion of iodic acid which we may conceive to be present; but the hydro-chloric acid becoming predominant, prevents a complete decomposition.

We have seen that the solution of chloruret is changed into iodate and hydro-chlorate, when saturated with an alkali. From this fact, and from the characters of the solution, we may suppose it to be a mixture of iodic and hydro-chloric acid. On the other side, as it deprives indigo of its colour, it would seem that the chlorine and iodine in it still preserve their properties entire. We may conceive it likewise to be a peculiar acid, which is decomposed when we saturate it with a base. I adopt the first supposition, because I compose exactly the solution of the chloruret when I mix iodic and hydro-chloric acids together. But I consider their elements as very mobile, and capable of taking a new arrangement according to circumstances. On this supposition the water is decomposed when the chloruret is dissolved in it. Its oxygen combines with the iodine, and its hydrogen with the chlorine. The inverse distribution could not take place, for the iodic and hydro-chloric acids are much more stable than the chloric and hydriodic acids; and it is a general law that, every thing else being equal, the strong compounds are always formed in preference to the weak.

If we take a given quantity of iodine, and act upon by an alkali, it will be divided into two very unequal parts. The smallest portion forms iodate; the greatest, hydriodate. If we wished to convert it entirely into iodate, we must begin by making it a chloruret; and after having dissolved it in water, we saturate it with the alkali

which we wish to convert into iodate. The iodates of barytes, lime, and strontian, being very little soluble in water, will be obtained pure after some washings. The others must be separated from the hydro-chlorates by repeated crystallizations or by alcohol.

### *Of the Hydriodates.*

In general these salts may be prepared by combining hydriodic acid with the bases; but we may obtain those of potash, soda, barytes, strontian, and lime, directly by treating iodine with these bases, employing the methods above described to separate them from the iodates which are formed at the same time. The hydriodates of zinc, iron, and in general of all the metals that decompose water, are obtained by dissolving the iodurets of these metals in water. We may put together the water, the iodine, and the metal, and by the application of heat the hydriodate is quickly formed. I do not propose to treat of all the hydriodates in detail, but merely to give their generic characters and their principal properties.

Sulphurous, hydro-chloric, and hydro-sulphuric acids, produce no change on the hydriodates at the usual temperature of the atmosphere.

Chlorine, nitric acid, and concentrated sulphuric acid, instantly decompose them, and separate the iodine.

With solution of silver they give a white precipitate insoluble in ammonia; with the pernitrate of mercury, a greenish-yellow precipitate; with corrosive sublimate, a precipitate of a fine orange-red, very soluble in an excess of hydriodate; and with nitrate of lead, a precipitate of an orange-yellow colour.

They dissolve iodine, and acquire a deep reddish-brown colour.

### *Hydriodate of Potash.*

When a solution of hydriodate of potash is made to crystallize, the oxygen combined with the metal, and the hydrogen with the iodine, unite together, and form water, and we obtain crystals of ioduret of potassium similar to those of chloruret of sodium. This salt easily melts, and sublimes at a red heat. When heated in contact of air, it undergoes no alteration. It is more deliquescent than the hydro-chlorate of soda. 100 parts of water, at the temperature of 64°, dissolve 143 of the salt. We may consider it as a hydriodate while it is in solution in water; but when melted, or even dried, it is obviously an ioduret of potassium. I find that when ioduret of potassium is dissolved in water, and afterwards dried, its weight is not increased.

Ioduret of potassium is composed of

Iodine .....	100
Potassium .....	31.342

and the hydriodate of

Hydriodic acid .....	100
Potash .....	37.426

*Hydriodate of Soda.*

I obtained it in pretty large flat rhomboidal prisms. These prisms uniting together form larger ones, terminated in echelon, and striated longways, like those of sulphate of soda. They contain a great deal of water of crystallization, and yet are very deliquescent. Heat drives off this water, melts the salt, and then renders it somewhat alkaline. It does not sublime so easily as hydriodate of potash. 100 parts of water, at the temperature of about  $57^{\circ}$ , dissolve 179 of the salt. When dried, it must be considered as an ioduret of sodium. I found that 100 parts of iodate of soda give, when decomposed by heat, 24.45 of oxygen. From the data given by that analysis, we may conclude the composition of the ioduret of sodium and hydriodate of soda to be as follows:—

Ioduret of sodium .....	{	Iodine .....	100
		Sodium .....	18.536
Hydriodate of soda .....	{	Acid .....	100
		Soda .....	24.728

The hydriodates of potash and soda converted into iodurets by desiccation, are the only ones not altered when heated to redness in contact with the air. The reason is, that iodine decomposes the oxides of potassium and sodium.\*

*Hydriodate of Barytes.*

This salt crystallizes in very fine prisms, very similar in appearance to the hydro-chlorate of strontian. After about a month's exposure to the air, I found it partly decomposed. Water dissolved the hydriodate coloured by iodine, and there remained undissolved subcarbonate of barytes. Hence the hydriodic acid is gradually destroyed by exposure to the air. Its hydrogen has formed water, and its iodine has been dissipated in the atmosphere, or has remained dissolved in the undecomposed hydriodate. The hydriodate of barytes, though very soluble in water, is but faintly deliquescent. When evaporated in a close vessel, and heated to redness, it does not melt; nor is its state of neutralization altered. If air, or, still better, if oxygen, be made to play on its surface when thus heated, vapours of iodine show themselves in abundance, and the salt becomes alkaline. I did not continue the experiment till the iodine ceased to be disengaged; but I presume that the hydriodate would be changed into a subioduret, as we have seen before, that this was the compound obtained when iodine in vapour was passed over barytes at a red heat. I have said that iodine does not disengage oxygen from barytes; yet I have no doubt that a red heat changes hydriodate of barytes into ioduret of barium. I have passed

\* As iodine disengages oxygen from the oxides of lead and bismuth, it is evident that the iodurets of these metals will not be decomposed by the air at a red heat.

hydriodic gas cooled to  $-4^{\circ}$  over barytes obtained from the recent calcination of the subnitrate; the barytes instantly became incandescent, and water made its appearance in the vessel; yet this barytes gave no oxygen gas when dissolved in water; nor did it undergo any alteration when I passed over its surface a current of dry hydrogen gas. I ascertained, likewise, that sulphur disengaged nothing; but that hydro-sulphuric gas produced much water when it combined with it.\* We cannot, therefore, doubt that at a red heat, and even at a lower temperature, hydriodate of barytes is converted into ioduret of barium.

Ioduret of barium . . . .	{	Iodine . . . . .	100
		Barium . . . . .	54.735
Hydriodate of barytes ..	{	Acid . . . . .	100
		Barytes . . . . .	60.622

The hydriodates of lime and strontian are very soluble, and the first is exceedingly deliquescent. I have neither determined the shape of their crystals, nor the quantity of water necessary to dissolve them. The hydriodate of strontian melts below a red heat, while the hydriodate of lime requires a higher temperature for its fusion. If they are heated in close vessels they become only slightly alkaline; but if air or oxygen have access to them while hot, thick vapours of iodine are immediately exhaled. If we consider these compounds as iodurets, the calcium and strontium are oxydized, and abandon a portion of the iodine. If we consider them as hydriodates, the hydrogen of the acid must combine with oxygen, and water be formed. I endeavoured to ascertain whether this was the case by passing dry oxygen gas over hydriodate of lime

\* The action of the hydrosulphuric gas was accompanied by a strong heat. The compound which was partly fused, being treated with hydrochloric acid; hydrosulphuric gas was disengaged, and a little sulphur precipitated. From this it is probable that a sulphuret with excess of sulphur is formed; and hydrogen disengaged. But as I employed a sulphuret which yielded a gas not totally absorbed by the alkalies, I could not ascertain the fact. Yet the abundant production of water which accompanies the combination of hydrosulphuric gas with barytes, and even with strontian, cannot be explained, except by admitting that these alkalies are reduced by the hydrogen in consequence of the united affinities of the oxygen for hydrogen, and of the metals for sulphur. But if this be the case, it is very probable that many metallic precipitates, which have been taken for hydrosulphates (hydrosulphurets) are only sulphurets. At a red heat, all the oxides which combine with sulphur give out water, and are changed into sulphurets when hydrosulphuric gas is brought in contact with them. This fact proves nothing against the existence of hydrosulphates at a low temperature. But hitherto there is not a single decisive experiment in proof of their existence; while the insolubility of them all seems to be a fact of an opposite nature. To confirm these conjectures, I dissolved a determinate weight of zinc in hydrochloric acid. I supersaturated the solution by ammonia, and precipitated by hydrosulphuric acid. The precipitate dried in the temperature of between  $140^{\circ}$  and  $176^{\circ}$  assumed the appearance of horn. Its weight was too great for a sulphuret, and too small for a hydrosulphuret. When heated to  $212^{\circ}$  it gave out water, and a new quantity was disengaged at a higher temperature. This experiment is not entirely decisive; but from the appearance of the precipitate I think it was a hydrate. At all events, this experiment is rather favourable than otherwise to my conjecture.

at a red heat; but the muriate of lime through which the gas afterwards passed, did not increase sensibly in weight. Hence every thing leads us to consider the hydriodates that have been melted or dried, as converted into metallic iodurets. The hydriodate of lime made with hydriodic acid, may be dried in the air without being decomposed. On the contrary, what has been made with iodine and lime becomes deep coloured as it is concentrated, though we evaporate at a very moderate heat. The reason is, that this last hydriodate holds in solution a certain quantity of iodate, and these two salts have the property of decomposing each other, when brought to a certain degree of concentration. The hydrogen of the hydriodic acid and the oxygen of the iodic acid form water, and the iodine which is thus disengaged is dissolved in the undecomposed portion of hydriodate, and gives it a reddish brown colour. The whole hydriodate is not destroyed, because there is only a small quantity of iodate present; and when calcined in close vessels, it is completely freed from colour.\*

Hydriodate of ammonia results from the combination of equal volumes of ammoniacal gas and hydriodic gas. It is usually prepared by saturating the liquid acid with ammonia. It is nearly as volatile as the hydrochlorate of ammonia; but it is more soluble and more deliquescent. I have obtained it crystallized in cubes. When heated in close vessels only a very small portion of it is decomposed. What sublimes is greyish black. If it be sublimed in contact of air, a much greater proportion of it is decomposed, and it becomes more or less coloured. It may be deprived of its colour by adding a little ammonia, or by exposing it to dry air. In the last case, the iodine to which it owes its colour is gradually volatilized.

The hydriodate of magnesia, formed by uniting its constituents together, is deliquescent, and crystallizes with difficulty. When heated to redness in close vessels, the magnesia abandons the acid in the same way as it abandons the hydrochloric acid. Having heated together iodine, magnesia, and water, to ascertain whether hydriodate and iodate was formed, as happens with the other alkalis, I obtained a flocky compound, which exactly resembled well prepared kermes. The liquid which covered it was scarcely coloured, and I ascertained in it the presence of hydriodate and iodate of magnesia, but in very small quantity. When evaporated, a flea-coloured matter is deposited on the sides of the vessel, quite similar to that of which I have spoken; and towards the end of the process, the liquid acquires a deep colour. This phenomenon is owing (as in the case of lime) to the mutual decompo-

\* To evaporate or calcine the hydriodates without the contact of air, I put them into a retort, to the neck of which I fit a tube, which, after having received the form proper for collecting gases, rises at its extremity parallel to the descending branch, and assumes nearly the shape of the letter U. When the aqueous vapour has expelled all the air of the retort, I place the ascending branch under a glass jar filled with hydrogenous azotic gas, above the level of the water.

sition of the hydriodate and iodate of magnesia when they reach a certain point of concentration, but it is much more marked with magnesia.

The flea-coloured matter is decomposed when put upon burning coals. Iodine is disengaged, and magnesia remains. Potash decomposes it likewise. When boiled in water, its colour is not changed, but the liquid is found to contain a little iodate and hydriodate. If the quantity of water be considerable, pure magnesia remains, and the water contains iodate and hydriodate.

From these facts it appears that the flea-coloured matter is an ioduret of magnesia, and that its existence in water depends upon the property which the iodate and hydriodate of magnesia have of mutually decomposing each other when concentrated to a certain point. When the water is in great quantity, no ioduret of magnesia appears; but it is deposited as the concentration advances.

This phenomenon does not take place with the iodates and hydriodates of potash and soda. It begins to show itself with those of strontian. It becomes more sensible with the iodate and hydriodate of lime, and is very conspicuous with those of magnesia. But this last alkaline basis has a weaker affinity than the others; and it is perhaps because the oxides of zinc and iron, &c. have a still weaker, that they do not condense a sufficient quantity of hydriodic and iodic acids to prevent them from acting on each other, and that in treating them with iodine no iodates and hydriodates are formed, though these salts may be obtained separately.

(To be continued.)

## ARTICLE X.

### ANALYSES OF BOOKS.

*An Attempt to establish a pure scientific System of Mineralogy by the Application of the Electro-Chemical Theory, and the Chemical Proportions. By J. Jacob Berzelius, M. D. F. R. S. Professor of Chemistry at Stockholm. Translated from the Swedish Original by John Black. 1814.*

THE doctrine of chemical proportions, though but recently introduced into chemistry, has produced a great reform in the science, and has given birth to a degree of accuracy, both in experimenting and reasoning, which has already placed chemistry on a footing with the mathematical sciences. Nobody has contributed more to produce this reform than Professor Berzelius. He has made the most numerous and accurate analyses which we at present possess, and has pointed out several general conclusions, which serve greatly to facilitate this kind of investigation. His activity, which surpasses that of any other chemical experimenter of the

present day, has led him to apply the doctrine of chemical proportions to all the different departments of the science. The object of the present little work is to show that minerals are all real chemical compounds, that every species consists of constituents combined according to the laws of chemical proportions, and that they are susceptible of an accurate chemical arrangement into classes, orders, genera, and species, according to the nature of the substances of which they are composed.

The work bears evident marks of the great abilities and extensive knowledge of the author, and must suggest many important and useful ideas to every mineralogist who will read it with sufficient attention; though perhaps a more deliberate consideration of the subject, and a more minute acquaintance with the details of mineralogy, might have led to a modification of some of the opinions which Professor Berzelius has advanced; for instance, he seems to rate the knowledge of the external characters of minerals very low, and to consider the whole of the science of mineralogy as confined to an acquaintance with the constituents of which every mineral is composed. But it is necessary to recollect that, before the chemical analysis of any mineral can be of importance to the science, or lead to any useful inferences, we must be sure that the specimen which we subject to analysis belongs really to the species which we suppose, and that it is quite pure and unmixed with any other mineral. Now this knowledge can only be acquired by an acquaintance with the external characters of minerals—a branch of knowledge which must therefore precede all useful chemical analysis. Hence it must always serve as the basis of our mineralogical knowledge. In fact, the labours of the chemist, who applies his practical skill to minerals, can only be of utility when he takes care to make his experiments upon correct and pure specimens. If a chemist, for example, analyze a specimen of *mica*, and publish the result under the name of *an analysis of talc*; or if he give the name of *stilbite* to what in reality is *mesotype*, his labours, instead of being useful, must be injurious to mineralogy. Yet these mistakes have been committed by chemists of acknowledged skill. The same injurious effects arise from the analysis of impure specimens, as when a mixture of *felspar* and *quartz*, or of *felspar* and *garnet*, is analyzed under the name of *felspar*. Unless I am much mistaken, errors of this kind have been lately committed by some of the most accurate analysts of the present day.

The knowledge of the constituents of minerals is always interesting, and in many cases indispensable. Thus the art of mining is founded on the knowledge of the different metals which may be extracted from the different ores. But to conceive that the whole science of mineralogy consists in a knowledge of the constituents of minerals, and that every thing else is of no consequence, is what no person can possibly do who has taken the requisite pains to make himself acquainted with the science. The *diamond* was accurately distinguished by its external properties, and was applied to as

many uses by our predecessors, who were ignorant that this mineral consists entirely of pure *carbon*, as it is by us, who are acquainted with that fact—a fact which must be admitted to be curious and important; but not to constitute every thing of any value with respect to the mineralogy of the diamond. *Gypsum* was well known by its properties, and was applied to all the purposes for which it is used at present, before Margraaf and Lavoisier ascertained it to be a compound of sulphuric acid and lime. A mineralogist may be very well acquainted with the characters of *gypsum*, capable of distinguishing it from all other minerals, and aware of the different uses to which it is applied, though he be ignorant of the constituents which enter into its composition.

We must not, therefore, confine the science of mineralogy to the mere knowledge of the constituents of minerals. It includes many other particulars of great importance, and has frequently got the start of chemical analysis in its conclusions. Thus *calcareous spar* and *aragonite* were considered by mineralogists as two distinct species, even when the most expert chemists were unable to discover any difference in their composition. When chemical analysis shall have arrived at a state of perfection, we may expect to find it agree in every respect with the conclusions drawn from the external characters; but in its present imperfect state, such discrepancies cannot be avoided; and when they do occur, it is but reasonable to give the superiority to the deductions from the external characters, as less likely to mislead us than an imperfect chemical analysis.

The object of Berzelius in the present little work is to show that all mineral species are really chemical compounds, composed of ingredients combined in definite proportions, and capable of being classified into orders, genera, and species, according to their composition, just as may be done with the salts. Though numerous analyses of minerals exist, yet it must be confessed that these definite proportions, this chemical composition according to the atomic theory, can be perceived only in a small number of individuals; while the great body of the mineral kingdom seems to bid defiance to the application of the laws of chemistry. But this discordance Berzelius considers, and I believe with justice, as only apparent, and not real. He ascribes it to three causes: 1. The inaccuracy of experimenters. 2. The mechanical mixture of foreign bodies with the chemical compounds, in consequence of the situation in which they were when they became solid or crystallized. 3. The deposition of two different compounds in contact with each other, which gives occasion to a form different from that of either of the compounds. Thus *aragonite* owes its form to the deposition of an atom of carbonate of strontian in contact with carbonate of lime at the time of its crystallization.

The minerals which it has hitherto been impossible to bring under the laws of chemical combination are the stones, composed chiefly of silica, alumina, lime, and oxide of iron, united in various proportions. Professor Berzelius conceives that in these

minerals the silica performs the function of an acid, and that it is chemically combined with the other earths or oxides which perform the function of bases. According to this notion, which has likewise been advanced by Mr. Smithson, the stony bodies are in reality salts. But they are often of a more complicated nature than the salts composed of the common acids and bases; for in the *siliciates*, as Berzelius terms these stones, we find not only the combination of silica with one base, but with two, three, or four bases, and often in various proportions, so as to constitute *subsiliciates* or *super-siliciates*.

If we suppose silica, alumina, magnesia, and lime, to be composed of one atom of oxygen and one atom of base, as I have done in the table published in the second volume of the *Annals of Philosophy*; or of two atoms of oxygen and one atom of base, as Berzelius has done, it is obvious that the number of integrant particles in any stony body may be determined by ascertaining the proportion which the oxygen of the various constituents bear to each other. Thus suppose we examine a mineral composed of silica and lime, and find that the oxygen in the silica is three times as great as in the lime, it follows that the mineral is composed of three integrant particles of silica and one integrant particle of lime; so that it may be termed a *trisilicate*. This is the mode which Berzelius has taken to determine the constitution of the various stony bodies. It has the advantage of being at once easy and accurate, supposing us acquainted with the composition of the different earths.

Berzelius supposes silica to be composed of 50.36 silicon + 49.64 oxygen. If therefore we consider it as a protoxide, it follows that the weight of an atom of silicon is 1.007, and the weight of an integrant particle of silica 2.007. He considers alumina as composed of 53.3 aluminum + 46.7 oxygen. The following table exhibits the weight of an atom of the different bases according to the analyses employed by Berzelius in this work.

	Weight.		Weight.
Silicon .....	1.007	Silica .....	2.007
Aluminum ....	1.141	Alumina .....	2.141
Magnesium ....	1.631	Magnesia .....	2.631
Calcium .....	2.571	Lime .....	3.571
Barium .....	8.523	Barytes .....	9.523

This table will enable the reader to calculate the composition of the different stony bodies, which have been subjected to an accurate analysis. For example, *schaalstein*, or *table spar*, is a *hydrous bisilicate of lime*; *somnite*, or *nepheline*, is a *silicate of alumina*; one of the species of *calamine* analyzed by Smithson, and composed of silica and oxide of zinc, is a *silicate of zinc*; *cerite* is a *silicate of cerium*. Berzelius gives examples of more complex

silicates. Thus *ichthyophthalmite* is a hydrate consisting of water combined with five integrant particles of *trisilicate of lime* and one integrant particle of *trisilicate of potash*; and *malacolite* consists of one integrant particle of *bisilicate of lime* and one integrant particle of *bisilicate of magnesia*.

Berzelius proposes to divide minerals into as many families as there are simple substances known, which amount to about 46. The families are divided into orders, according to the different electro-negative bodies with which the most electro-positive are combined: as, for example, 1. Sulphurets: 2. Carburets: 3. Oxides, &c. He gives the following examples of this arrangement, which will make it more intelligible.—SILVER FAMILY. 1st order: Pure silver. 2d order: Sulphurets. 3d order: Stibiets, consisting of antimonious silver ore and *silberspies-glanz*. 4th order: Tellurets, containing the various ores of tellurium. 5th order: Aurets, containing electrum and auriferous silver. 6th order: Hydrargyrets, containing native amalgam. 7th order: Carbonates. 8th order: Muriates.—IRON FAMILY. 1st order: Native iron. 2d order: Sulphurets. 3d order: Carburets. 4th order: Arseniets. 5th order: Tellurets. 6th order: Oxides. 7th order: Sulphates. 8th order: Phosphates. 9th order: Carbonates. 10th order: Arseniates. 11th order: Chromates. 12th order: Tungstates. 13th order: Siliciates. 14th order: Tantalates. 15th order: Titanates. 16th order: Hydrates.—FAMILY OF ALUMINIUM. 1st order: Sulphates. 2d order: Fluates. 3d order: Fluo-silicates. 4th order: Siliciates. 1st subdivision: Single silicates, comprehending nepheline, collyrite, &c. 2d subdivision: Double silicates, comprehending beryl, emerald, Euclase; mealy zeolite, lomonite; harmotome, or cross-stone; mesotype, schorl; felspar, lepidolite, leucite. 3d subdivision: Triple and complex silicates, comprehending prehnite, kounpholite, mica.

Five appendices are added to the dissertation itself. In the first, Berzelius gives the ground of his calculations, as they have been already explained in his paper, On the Cause of Chemical Proportions, printed in the second and third volumes of the *Annals of Philosophy*. The second appendix exhibits the number of atoms of oxygen existing in the various oxides. In the third appendix he explains the different symbols which he employs in his discussions. The fourth contains a valuable set of experiments on magnetic iron ore, proving it to be a compound of black and red oxides of iron. This valuable paper shall be inserted in a subsequent number of the *Annals*. We shall likewise in a future number insert the fifth appendix, in which Berzelius determines the proportion of oxygen contained in glucina.

## ARTICLE XI.

*Proceedings of Philosophical Societies.*

## ROYAL SOCIETY.

*Feb. 23.*—At this meeting a part of a paper by Sir H. Davy, entitled, *Some Experiments and Observations on the Colours used in Painting by the Ancients*, was read.

*March 2.*—At this meeting the conclusion of Sir H. Davy's paper was read. The following are the results of his observations:—

The experiments were made on colours found in the baths of Titus, in the baths of Livia, and other ruins of Ancient Rome, as well as of Pompeii. Some of them were discovered in vases found below the ruins of the Palace of Titus. These were identified with the colours employed in various fresco paintings in this palace.

Three kinds of red were found in a vase two years ago; the first, approaching to orange, was minium; the second, a dull red; and the third, a purplish red, were ochres. Another red, found in fresco paintings, was plainly vermilion, called minium by the Romans, who gave the name of *cerussa usta* to our minium, which Sir H. Davy supposes to have been a natural carbonate of lead burnt. The yellows are ochres diluted with different quantities of chalk, and yellow oxide of lead, or massicot. No sulphurets of arsenic were detected, though they were known to Pliny and Vitruvius.

Lumps of a deep blue frit were collected among the baths of Titus, which were composed of soda, silica, and oxide of copper. All the blues were composed of this compound, and the intensity of their colour was reduced with carbonate of lime. Sir Humphry supposes this to be the blue colour described by Theophrastus as having been discovered by an Egyptian King, and as manufactured at Alexandria. The *cœrulium* of Vitruvius was made by heating sand, natron, and copper.

No traces of indigo have been detected, though it was known to the ancients. Cobalt was observed colouring glass.

The greens are carbonate of copper, except one which approaches to olive. This is the common green earth of Verona. Sir H. thinks it not improbable that many of the carbonates of copper were originally acetites.

In the bottom of a broken vase found in the baths of Titus a substance externally cream-coloured, internally carmine, existed, which was combustible, and left about  $\frac{1}{10}$  of siliceous, aluminous, and calcareous earths. It is regarded as a *lake*, though it is impossible to determine whether it be animal or vegetable.

The browns are mere oxides of iron, and sometimes mixtures of iron and manganese.

No cerusse was found among the whites, which were generally carbonates of lime and fine white clays.

The ground on which the painting was executed was powdered marble, cemented with lime, which was polished. No wax or animal gluten was discovered on any of the paintings.

March 9 and 16.—At these two meetings a paper was read from Dr. Brewster, on the laws which regulate the polarization of light by reflection from the second surfaces of transparent bodies.

#### LINNÆAN SOCIETY.

At the meeting of the 7th March a paper was read from Mr. Sowerby, on a branched fossil found in Green Sand, Warminster, to which he proposes to give the name of *pedunculum lobatum*. A figure was also exhibited.

At the same meeting a paper was read from the Rev. Patrick Keith on the epidermis of plants, in which, after stating the general analogy pointed out by Grew, and other early vegetable physiologists, between the epidermis of plants and animals, he examines the doctrine especially supported by M. Mirbel, who regards that substance as merely the external surface of the parenchyma acted upon and hardened by the atmosphere. He then shows that this last opinion is unfounded, and finally endeavours to establish the general analogy adopted by the early writers.

A memoir from Dr. Leach on the distribution of the orders composing the class Insecta, with descriptions of some new genera and species was read. In the following table an outline of his classification is inserted.

#### CLASS INSECTA.

##### SUBCLASS I.—AMETABOLIA.

Insects undergoing no metamorphoses.

ORDER I. *Thysanura*.—Tail armed with setæ.

ORDER II. *Anoplura*.—Tail without setæ.

##### SUBCLASS II.—METABOLIA.

Insects undergoing metamorphoses.

CENTURY I. *Elythroptera*.—Insects with elytra.

*Odontostoma*.—Mouth with mandibles.

\* Metamorphoses incomplete.

ORDER III. *Colcoptera*.—Wings transversely folded; elytra crustaceous, covering the wings, with the suture strait.

\*\* Metamorphosis nearly coarctate.

ORDER IV. *Strepsiptera*.—Wings longitudinally folded; elytra coriaceous, not covering the wings.

\*\*\* Metamorphoses semi-complete.

ORDER V. *Dermaptera*.—Wings longitudinally and transversely folded; elytra somewhat crustaceous, abbreviated; with the suture strait.

ORDER VI. *Orthoptera*.—Wings longitudinally folded; the internal margin of one elytron covering the same part of the other; elytra coriaceous.

ORDER VII. *Dictyoptera*.—Wings longitudinally folded twice or more; elytra coriaceous, nervous, one decussating the other obliquely.

*Siphonostoma*.—Mouth with an articulated rostrum.

ORDER VIII. *Hemiptera*.—Elytra somewhat crustaceous, or coriaceous; towards the apex generally membranaceous, horizontal, one decussating the other obliquely. Metamorphoses half complete.

ORDER IX. *Omoptera*.—Elytra entirely coriaceous, or membranaceous, and meeting obliquely, with a strait suture. Metamorphoses semi-complete, or incomplete.

CENTURY II. *Medamoptera*.—Insects without wings or elytra.

ORDER X. *Aptera*.—Mouth with a tubular sucking rostrum. Metamorphoses incomplete.

CENTURY III. *Gymnoptera*.—Insects with wings, but no elytra.

COHORS I. *Glossostoma*.—Mouth with a spiral tongue.

ORDER XI. *Lepidoptera*.—Wings four, membranaceous, with pterigostea, covered with meal-like scales.

COHORS II. *Gnathostoma*.—Mouth with maxillæ and lip.

ORDER XII. *Trichoptera*.—Wings four, membranaceous, with pterigostea, and hairy.

COHORS III. *Odontostoma*.—Mouth with mandibules, maxillæ, and lip.

ORDER XIII. *Neuroptera*.—Four highly reticulated wings, generally equal in size; anus of the female without a sting, or compound borer.

ORDER XIV. *Hymenoptera*.—Four venose wings, hinder ones smallest; anus of the female with a sting, or with a compound borer or oviduct.

COHORS IV. *Siphonostoma*.—Mouth tubular, formed for sucking.

ORDER XV. *Diptera*.—Wings; and halteres or balancers two.

At the meeting of the 21st of March, a paper containing some additions to the *Novi Systematis Ornithologici Prolusio*, by M. Vieillot, concluded at a former meeting.

#### GEOLOGICAL SOCIETY.

Jan. 20, 1815.—The reading of Dr. Macculloch's paper on Glen Tilt was concluded.

The remarkable appearances attending the junction of the granite and of the schistose rocks at Glen Tilt were first pointed out by Dr. Hutton, and were considered by him as affording strong proofs of the original fluidity of granite, and of its having forcibly intruded itself into the situation which it now occupies at a period subsequent to the formation of the schist with which it is intermixed.

The supporters of the Wernerian theory who have visited this remarkable valley have denied the accuracy of some of Dr. Hutton's descriptions, and have called in question the justness of the conclusions deduced by him from these appearances. The advocates for Dr. Hutton's theory have brought forward counter statements.

Hence it becomes a matter of considerable importance that the real nature of the facts appealed to by both parties should be clearly ascertained.

Most of the previous observers appear to have confined their researches to particular parts in the bed of the Garry (the river which flows through the Glen), where the rocks may be observed with least trouble, and where the junctions of the granite with the schist are the most obvious. In the present memoir the entire bed of the river from one end of the Glen to the other, where it was accessible, is particularly described, as well as the slopes of the mountain which form the sides of the Glen.

The right side of Glen Tilt consists for the most part of red granite, passing sometimes by the intermixture of crystals hornblende into sienite. This granite may be traced to Ben Deary, and thence to the great body of granite forming the centre of the Highlands, and of which body the right bank of Glen Tilt may be considered as the southern boundary.

The surface of the granite is irregularly undulated and protuberant. Incumbent on the granite are several stratified rocks, the lowest of which appears to be quartz rock, which is covered by, and alternates with, mica-slate, either pure or passing into gneiss, with clay-slate and with granular lime-stone, either the common blue variety, or white marble, which latter sometimes contains veins and concretions of noble serpentine, and is sometimes interstratified with thin beds of tremolite and of sahlite.

The beds lying in the granite are sometimes broken off abruptly; and this latter rock making its appearance in the intervals frequently gives the appearance of an alternation of granite with the stratified rocks that in reality lie only on its surface.

The stratified rocks are for the most part very regular in their position; but where they are in contact, or approach very near to the granite, very remarkable disturbances, and other changes, are observed to take place. The lime-stone is much indurated, and more siliceous than usual. Sometimes it contains small grains of reddish-brown felspar; sometimes the lamina of the lime-stone are separated, and the interstices are filled up with granite; sometimes veins of various magnitude pass from the body of the granite into the superincumbent beds, occasioning flexures, fractures, and various intimate and fantastical intermixtures of all the adjacent rocks; sometimes, on the other hand, lumps and masses of the stratified rocks are to be found involved in the granite.

Quartz rock is not always the immediate covering of the granite; on the contrary, the several varieties of schist, and even lime-stone, are sometimes found in that position, a circumstance that appears to be irreconcilable with the theory of the stratified rocks having been deposited on a basis of pre-existing granite, but easily accounted for on the theory of the forcible irruption of the granite from below in a fluid state: in which case it would rupture and penetrate the superincumbent beds, more or less, according to the

force originally impressed upon it, and to the degree of resistance offered by the varying thickness and solidity of the stratified rocks.

A letter from the Rev. Richard Hennah, jun., dated Jan. 19, 1815, was read.

The object of this letter is to state that, in addition to the information contained in the former letters of this gentleman, (read Nov. 18, 1814,) concerning the occurrence of organic remains in the Plymouth lime-stone, he has recently found many well-defined shells of the genus *turbo*, and of other genera, at the east end of the Hoe under the citadel; and also fragments of shells in a quarry at Cat-down.

A memoir on the native oxide of uranium of Cornwall, by W. Phillips, Esq. M. G. S. was read.

Uranite in green tabular transparent crystals, imbedded in ferruginous quartz, has been found by Mr. Phillips, at Carharrock, near St. Die. It has also been found by him in very minute crystals, varying in form and colour, and accompanied by pitch blende, at Tincroft mine near Redruth; also in well defined crystals, and accompanied by pitch blende, at Tol Carne mine.

At Huel Jewel this mineral occurs on red copper ore; and in a single instance has been found on the surface of Wavellite from Stenna gwyn near St. Austle. Specimens of extraordinary beauty have been discovered within the last three or four years at Gannis lake copper mine near Callington, on decomposed granite.

The larger crystals of this mineral are all deeply striated; and even the most minute ores, although seemingly well defined, are not capable of being accurately measured by the reflecting goniometer. From some very satisfactory clearages which Mr. Phillips has obtained, it appears to have natural joints parallel to the lateral faces, and to the two diagonals of a tetrahedral prism.

The paper concludes with a description, illustrated by drawings, of 46 varieties of crystalline form.

A paper, entitled *Supplementary Observations on Quartz Rocks*, made in 1814, by Dr. Macculloch, was read.

This rock forms the larger portion of the great valley of the Tunnel, where it alternates with micaceous and with argillaceous schist, with lime-stone, and with porphyry. It may be observed passing insensibly into granite, and, when it alternates with mica-slate, the proportion of quartz rock will be found to prevail in the vicinity of granite; and that of mica-slate at a distance from the granite. In Ben Glou it occurs incurvated and contorted; and on Cairn Gower are some beds consisting wholly of large and small rounded pebbles. In Mar forest it alternates with large incomplete garnets. In Glen Tilt it presents a finely laminated structure, like the schistose sand-stones of the coal strata, and is divided by natural joints into rhomboidal tables. In Ben Glou it occurs of a beautiful pink colour; and in Glen Farnat it is mixed with scales of mica forming an *avanturine* capable of receiving a good polish.

*March 3.*—The reading of Mr. Horner's paper on the *Geology of the South Western part of Somersetshire* was begun.

## ARTICLE XII.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS  
CONNECTED WITH SCIENCE.

## I. Lectures.

*Middlesex Hospital.*—Dr. Merriman's next Course of Lectures on Midwifery and the Diseases of Women and Children will be delivered at the above hospital during the months of April and May. The Introductory Lecture will be read on Monday, April 10, at half-past ten o'clock.

II. *Smithson Tennant, Esq.*

It is with much concern that we announce the death of Smithson Tennant, Esq. F.R.S. and Professor of Chemistry in the University of Cambridge; a man in whom genius, talents, and virtue, were united in their highest forms. Although his industry was checked by a frame naturally weak, and a languid state of health, his acquirements in science were remarkably general, and in many branches profound.

The circumstances of Mr. Tennant's death were most afflicting. He was returning from France, where he had been several months, and was waiting at Boulogne for a favourable wind. He had actually embarked on Wednesday the 22d of February, but the vessel was obliged to put back; and it was determined, if the weather should be tolerable, to make another trial in the evening. During the interval Mr. T. proposed to a German officer of distinction, whom he had accidentally joined on the road, and who was also going to England, to ride with him to Buonaparte's Pillar, near Boulogne. In returning he deviated a little to look at a fortification near the road. But as they were attempting to pass a draw-bridge, which, owing to some neglect, was not properly secured, the bridge gave way, and they were precipitated into the trench. The officer fortunately escaped without any serious hurt; but Mr. Tennant was found fallen under his horse, and was taken up speechless, his skull and one of his arms being considerably fractured. He was conveyed with difficulty to the hospital at Boulogne, as the nearest and most convenient place to receive him, and expired in half an hour. His remains were interred at Boulogne.

A fuller account of this interesting philosopher is preparing, and will be given in an early number of the *Annals*.

III. *Dr. Seetzen, the German Traveller.*

Dr. Seetzen is little known in this country, except through a translation, by the Palestine Society, of a brief account by him of the countries adjoining the lake Tiberias, the Jordan, and the Dead Sea. As his merits are very extraordinary, it will be interesting to give a short sketch respecting him.

*Ulrich Jasper Seetzen*, born in the Russian lordship of Jever, is the son of an affluent farmer. He was educated at Göttingen,

where he took a degree in medicine; and having steadily regarded as the final end of his studies the exploring the interior of Africa, he prepared himself by the study of natural history in all its branches, Eastern languages, and astronomy, geography, &c. In 1802 he left Gotha, accompanied by a young man of the name of Jacobsen, and went to Vienna, whence he proceeded with some difficulty to Constantinople, where he arrived in December, 1802. Thence he went to Smyrna, which he quitted in October, 1803, having previously explored the adjacent country. He then resided at Aleppo, and afterwards at Damascus; afterwards he examined the geological structure of the country towards Jerusalem, Lebanon, and Latitibanon. In 1805 he travelled through Arabia into Egypt, and finally arrived at Cairo. During residence at this last place (for two years) he transmitted his very valuable observations in separate dissertations to Germany, where many have been published, and many remain in the possession of his relations. In 1809 he quitted Cairo, returned into Arabia, and in Nov. 1810 he was at Mokka, since which no correct account of him has reached this country. A report has been received of his death, but in a very suspicious form. It would be gratifying to his friends to know his fate, as they would then be enabled to give the whole of his valuable researches to the world, a part of which only has hitherto appeared in the journal of his liberal friend Baron Von Zach.

IV. *Gas Light.—Conveyance of Steam in Pipes.—Smallest Angle at which Objects are perceptible.*

(To Dr. Thomson.)

DEAR SIR,

I have been much gratified by the information which has come out respecting the saccharometer, in consequence of my inquiry in your *Annals*. The "*Philosophical Intelligence*" is a most interesting part of that work.

Permit me to suggest to you that accidents, arising from philosophical negligence, if I may use the expression,—such as the late *explosion* of a brewer's store cask, and a recent accident with a gas-light apparatus at Birmingham,—deserve to be recorded in your *Annals*, for obvious reasons. A friend of mine was going to light two large manufactories with coal gas, but since the late accident he has been undetermined whether to proceed or not. Will it be presuming too much to ask your opinion respecting the new mode of illumination with gas? The opinion of so experienced a chemist and philosopher would have a most beneficial influence. And what sort of coal is to be preferred for producing the gas in question? I presume the diameters of the conduit pipes have hitherto been taken at random, and that the pressure on the reservoir of gas for sending a stream through the pipes has not yet been accurately observed, or at least not published.

I am also desirous to inquire how far steam has been known to be conveyed, or how far it probably can be conveyed, for heating water, and how large the conducting pipes should be to produce a required

effect. Is any damage to be expected from the sudden condensation of the steam on its first application to the cold water: I have been told that the pipes are liable to burst from this cause; a circumstance which I think Count Rumford never mentions in his essays on this subject.

One more query, and I have done. Has any experiment been published which ascertains the smallest object or angle that is perceptible to a sound unassisted eye, or an eye assisted with a given magnifying power? In a philosophical work translated from the French I have read that the eye is incapable of perceiving an object which subtends an angle of less than half a minute of a degree.

I remain, Sir, most respectfully, your obliged servant,

Feb. 3, 1815.

M.

V. Meteorological Table. From the Register kept at Kinfauns Castle, N. Britain. Supposed Lat.  $56^{\circ} 23\frac{1}{4}'$ . Above the Sea 129 feet.

1814.	Morning, 8 o'clock.		Evening, 10 o'clock.		Depth of Rain In. 100	No. of days		
	Mean height of		Mean height of			Rain or Snow.	Fair.	
	Barom.	Ther.	Barom.	Ther.				
Jan. ....	29.78	24.64	29.80	25.39	.60	11	20	
Feb. ....	30.06	34.03	30.04	34.50	.63	8	20	
March ....	29.90	36.48	29.91	36.80	.85	17	14	
April. ....	29.90	46.43	29.90	46.10	1.75	18	12	
May. ....	30.15	47.06	30.16	44.77	.75	8	23	
June. ....	30.16	53.06	30.02	50.50	.57	6	24	
July. ....	29.93	59.35	29.94	57.10	1.72	15	16	
Aug. ....	29.91	56.77	29.93	54.86	2.60	18	13	
Sept. ....	30.10	51.90	30.10	52.66	.37	8	22	
Oct. ....	29.62	44.54	29.84	44.51	1.02	11	20	
Nov. ....	29.74	38.70	29.74	38.16	2.30	12	18	
Dec. ....	29.65	34.80	29.66	35.38	2.23	18	13	
Aver. of year.	29.925	43.980	29.920	43.394	15.59	150	215	

#### ANNUAL RESULTS.

##### MORNING.

BAROMETER.		THERMOMETER.	
Observations.	Wind.	Observations.	Wind.
Highest, May 11 ...	S E ... 30.64	July 24.....	S E ... 64
Lowest, Jan. 16 ...	S E ... 28.70	Jan. 14.....	W ... 12

##### EVENING.

Highest, May 10 ...	E ... 30.65	July 25.....	S E ... 65
Lowest, Mar. 2 ...	S E ... 28.91	Jan. 15.....	W ... 12

Weather.	Days.	Wind.	Times.
Fair .....	215	N and N E .....	3
Rain or Snow.....	150	E and S E .....	109
		S and S W .....	65
		W and N W .....	188
	365		365

Extreme Cold and Heat, by Six's Thermometer.

Coldest, 14th January .....	7°
Hottest, 24th July .....	76
Mean for 1814 .....	45.168

## Result of three Rain Gages.

In. 100.

No. 1. On a conical detached hill above the level of the sea 600 feet....	33.84
— 2. Centre of garden, 20 feet .....	20.05
— 3. Kinfauns Castle, 129 feet.....	15.59

Mean of the three gages ..... 23.61

## VI. Royal Medical Society of Edinburgh.

The Royal Medical Society of Edinburgh having agreed to appoint a Committee for the purpose of receiving the communications of members, and of others through their medium, who may favour the Society with interesting facts and experiments in Medicine, or with unusual appearances in morbid dissection, beg leave earnestly to invite the members to transmit such communications to the Society as soon as possible.

The Committee, consisting of six extraordinary members resident in Edinburgh, together with the four Presidents ex officio, will proceed immediately to consider such papers as may be transmitted to them, with an ultimate view to publication.

## VII. On Imaginary Cube Roots.

(To Dr. Thomson.)

SIR,

You sometimes insert algebraical communications in your *Annals of Philosophy*. I have lately discovered that all numbers have four imaginary cube roots, which is contradictory to the generally accepted theory, that "every equation has as many roots as it has dimensions, and no more."

The imaginary cube roots of the number 64 are  $-2 \pm \sqrt{-12}$ ; but the following quantity raised to the third power shows that it is also a cube root of the same number 64.

$$\begin{aligned}
 & \left( \frac{3 + \sqrt{-3}}{2} + \sqrt{-\left(\frac{1}{2} - \frac{3}{2}\sqrt{-3}\right)} \right) \\
 & - \frac{3 + \sqrt{-3}}{2} + \sqrt{-\left(\frac{1}{2} - \frac{3}{2}\sqrt{-3}\right)} \\
 & \hline
 6 + \frac{6\sqrt{-3}}{4} - 3 + \sqrt{-3}\sqrt{-\left(\frac{1}{2} - \frac{3}{2}\sqrt{-3}\right)} \\
 & - \frac{1}{2} + \frac{3}{2}\sqrt{-3} \\
 & \hline
 -5 + 3\sqrt{-3} - 3 + \sqrt{-3}\sqrt{-\left(\frac{1}{2} - \frac{3}{2}\sqrt{-3}\right)} \\
 & - \frac{3 + \sqrt{-3}}{2} + \sqrt{-\left(\frac{1}{2} - \frac{3}{2}\sqrt{-3}\right)} \\
 & \hline
 12 - 2\sqrt{-3} + 3 + 3\sqrt{-3}\sqrt{-\left(\frac{1}{2} - \frac{3}{2}\sqrt{-3}\right)} \\
 24 + 2\sqrt{-3} - 5 - 3\sqrt{-3}\sqrt{-\left(\frac{1}{2} - \frac{3}{2}\sqrt{-3}\right)} \\
 & \hline
 36 + 0 + (-2 + 6\sqrt{-3})\sqrt{-\left(\frac{1}{2} - \frac{3}{2}\sqrt{-3}\right)}
 \end{aligned}$$

To find the value of the last term, the first member must be squared, the second member must be multiplied into the square, and the sign of the square root prefixed to the result. Thus,

$$\begin{array}{r}
 - 2 + 6 \sqrt{-3} \\
 - 2 + 6 \sqrt{-3} \\
 \hline
 4 - 24 \sqrt{-3} \\
 - 108 \\
 \hline
 - 104 - 24 \sqrt{-3} \\
 - \frac{1}{2} + \frac{3}{2} \sqrt{-3} \\
 \hline
 676 + 156 \sqrt{-3} \\
 108 - 156 \sqrt{-3} \\
 \hline
 784
 \end{array}$$

And prefixing the sign it becomes  $\sqrt{784} = 28$ , which added to 36 found before gives 64.

I am, Sir, your obedient servant,

Tubney Park, March 5, 1815.

JAMES LOCKHART.

#### VIII. On the Use of the Cerebellum and Spinal Marrow.

(To Dr. Thomson.)

SIR,

London, Feb. 14, 1815.

In the 26th Number of your Journal is announced a discovery of the use of the cerebellum and structure of the spinal marrow, by Dr. John Cross of Glasgow. The following are the words of this announcement:

"I was led about a year ago to conclude, that as the cerebrum is the fountain of sensation and intellect, the cerebellum must be the organ which supplies with nervous energy the face and other parts of the head, extrinsical of the brain, &c." Again, "I found that the spinal marrow is uniformly composed of four longitudinal divisions; two larger, anterior, which may be traced into the cerebrum, and two smaller, posterior, which may be traced into the cerebellum. I suppose that the two anterior portions are the organ of feeling; the two posterior, of motion."

Now, Sir, this, though more vaguely stated, is so closely imitative of a statement which I actually published nearly *six years ago*, (in Archives of Universal Science, vol. iii. for July, 1809,) that I am compelled to beg you will do me the justice to point it out. The following is that statement.

"From the peculiar opposition which subsists between the situation of the face and cerebellum, we are entitled to expect a similar opposition in their functions. As the face, therefore, occupied by the chief organs of sense, is the seat of sensation, so we might expect the cerebellum to be the organ of volition. This supposition receives additional force from the consideration that, as the organs

of sense and the cerebellum are the first and the last portions of the nervous system; so sensation and volition are the first and the last of its functions. But this supposition is completely confirmed, when we recollect that the degrees of voluntary power always bear a close analogy to the various magnitudes of the cerebellum. In fishes, for instance, which possess amazing locomotive power, the cerebellum is often larger than the hemispheres! (P. 176, Op. Cit.)

"The spinal marrow actually consists of *four columns*, of which *two* are *anterior* and *two posterior*. In the anterior columns terminate the anterior fasciculi of all the spinal nerves; and these *columns* themselves, passing through the medulla oblongata, crura cerebri, and corpora striata, *terminate in the hemispheres* of the cerebrum. The anterior fasciculi of the spinal nerves are, therefore, their ascending fasciculi, and the anterior columns of the spinal marrow are its ascending columns. *From the cerebellum again descend the posterior columns* of the spinal marrow, and from these proceed the posterior fasciculi of the spinal nerves. The posterior columns of the spinal marrow are, therefore, its descending columns, and the posterior fasciculi of the spinal nerves are their descending fasciculi. *Hence it is that these nerves and almost all the nerves of the body, have filaments of ascending and filaments of descending impression; are at once nerves of sensation and nerves of locomotion.*" (Page 142.)

These quotations are, I believe Sir, sufficiently decisive as to the original author of those discoveries, which Dr. Cross intends to make the subject of his promised work.

On this subject I may take this opportunity of observing, that my friend Dr. Spurzheim, (in his recent and very valuable work on the Anatomy, Physiology, and Physiognomy of the Brain,) has most certainly erred in denominating the cerebellum the organ of amateness; for *physical love no further depends on the cerebellum than that its degree is of course inseparably connected with the degree of voluntary power which is the proper function of that organ*; and hence it is, that the bull and stallion, having larger cerebella and more energetic voluntary power, have also a stronger propensity to physical love than the ox and cow, or the gelding and mare. Notwithstanding this objection however, I reckon that work the most important accession to our anatomical knowledge which ever has been made by any philosopher.\*

I remain, Sir, with great respect, your obedient Servant,

ALEXANDER WALKER.

\* Since writing the above, Sir, I observe in the 27th number of your Journal a letter addressed to you by Dr. Leach, in which he indicates the large work of Gall and Spurzheim on the brain, published at Paris in 1807, as the original source of the discoveries so unfairly claimed by Dr. Cross. In answer to Dr. Leach, I need only say, 1. That Gall and Spurzheim's work actually contains no such statements; and 2. That such statements are even in direct opposition to their doctrines.

IX. *On the Organic Remains in Plymouth Lime-stone.*

(To Dr. Thomson.)

SIR,

Citadel, Plymouth, March 9, 1815.

In the last Number of the *Annals of Philosophy* there is a letter on my having found madrepores and shells in the lime-stone rocks at Plymouth. I should probably not have troubled you with a reply, had it not been stated that no shells had been observed in those rocks "in situ." It appears Dr. Leach is not aware of the discovery of a bed of lime-stone, abounding in shells, in the dock-yard; and probably he is equally unacquainted with my communications to the Geological Society, and the specimens which I have sent for the use and inspection of its members. In addition to these, I could show him many varieties of shells which I have obtained from various parts of the rocks in this neighbourhood. I might add, about the same time.

With respect to the madrepores, Dr. Lockyer, with some other friends, accompanied me in one of my visits to the quarry where I first found them; and he was of opinion, I remember, that they had more the appearance of imbedded stalactites than of animal remains; and, as I was of a different opinion, in sending some specimens to the Geological Society, I thought they might be interesting at least, if they did not tend to any useful purpose.

As to the specimen in my possession of what Dr. Leach calls madreporite of a fine blood-red colour, I never considered it as such, but of a quite different nature. It was given to me several years ago, as coming from the neighbourhood of Stonehouse, or Devil's Point, and believing it to be the case, I have made many attempts to discover its native bed, though without success.

I have no time for uninteresting or useless writing; but deeming the field of Science and Natural History open to all who are disposed to enter it, I shall continue my researches as hitherto, without giving, I hope, at least without intending to give, offence to any one; and certainly without assuming to myself the merit (if any) which others may think due exclusively to their own labours.

I have the honour to remain, Sir, your obedient Servant,

RICHARD HENNAH, JUN.

## ARTICLE XIII.

*Scientific Books in hand, or in the Press.*

Dr. Ronalds, of Coventry, is preparing for the Press a Translation of the celebrated little work of Cabanis on Certainty in Medicine.

Mr. Huish will shortly publish a Scientific and Practical Treatise on the Management of Bees, with Plates.

Mr. Parkes, the author of the Chemical Catechism, has in the Press a series of Chemical Essays, to be published in four pocket volumes, including a variety of Explanatory Notes, and an Index. It will be illustrated with more than 20 Copper-Plate Engravings.

## ARTICLE XIV.

## METEOROLOGICAL TABLE.

1815.	Wind.	BAROMETER.			THERMOMETER.			Evap.	Rain.
		Max.	Min.	Med.	Max.	Min.	Med.		
2d Mo.									
Feb. 1		29.45	29.36	29.405	48	39	43.5		
2	E	29.62	29.45	29.535	46	36	41.0		
3	W	29.62	29.39	29.505	49	39	44.0		.13
4	S W	29.80	29.39	29.595	54	36	45.0		
5	S E	29.92	29.74	29.830	50	38	44.0		
6	S W	29.74	29.55	29.645	47	42	44.5		4
7	Var.	29.70	29.55	29.625	52	36	44.0		—
8		29.62	29.49	29.555	50	38	44.0	.15	.15
9	S E	29.66	29.63	29.645	47	37	42.0		
10		29.63	29.43	29.530	46	40	43.0		4
11	Var.	29.40	29.28	29.340	51	39	45.0		.11
12	S W	29.51	29.37	29.440	50	48	49.0		.21
13	S W	29.60	29.40	29.500	52	32	42.0		
14	E	29.65	29.40	29.525	50	37	43.5		—
15	Var.	29.65	29.56	29.605	51	42	46.5		—
16	S W	29.56	29.50	29.530	54	35	44.5	.18	.16
17	N W	30.26	29.50	29.880	52	33	42.5		2
18	S W	30.26	30.16	30.210	51	34	42.5		
19	S W	30.16	29.71	29.935	48	45	46.5		—
20	W	29.82	29.55	29.685	54	42	48.0		.26
21	N W	30.15	29.82	29.985	56	48	52.0		
22		30.20	30.15	30.175	57	39	48.0		
23		30.15	30.00	30.075	51	40	45.5	.27	
24	W	30.00	29.97	29.985	52	47	49.5		
25	S W	29.97	29.92	29.945	53	46	49.5		
26	N W	30.40	29.92	30.160	54	29	41.5		5
27		30.47	30.44	30.455	48	25	36.5		
28	S E	30.44	30.15	30.295	45	31	38.5		
3d Mo.									
Mar. 1	S E	30.19	30.15	30.170	52	33	42.5	.20	1
		30.47	29.28	29.785	57	25	44.43	.80	.18

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

## REMARKS.

*Second Month.*—1, 2. Misty dull weather. 3. Cloudy: wind and rain in the night. 4. Temp.  $49^{\circ}$  at the time of observation: fine: *Cumulus*, beneath hazy *Cirrus*. 5. *Cirrostratus* and haze at sun-rise, and much dew, which did not evaporate: a very fine day: temp.  $56^{\circ}$  in the sun: p. m. *Cumulostratus*. 6. The same, followed by rain. 7. a. m. Overcast, dripping: p. m. wind N.W.: groups of *Cumulostratus*, *Cumulus*, and *Cirrostratus*, occupying the whole S. from one to two, coloured light indigo, with red haze above, and attracting the smoke. 8. p. m. The wind rose at S. with the usual hollow sound: a beating rain from that quarter ensued. 9. Misty, from a diffused *Cirrostratus*, and cloudy above. Water from a well, which might be  $10^{\circ}$  warmer than the air, emitted a visible steam. 10. Cloudy: dripping at intervals. 11. The same: dark *Nimbi* passed in the S., the wind being W. p. m. 12. a. m. Misty: cloudy: wind and showers. 13. A wet windy morning, succeeded by a very fine day: *Cumulus*, with *Cirrus*. 14. A very moist *Cirrostratus*, a. m.: then fine, with various clouds: a little misty rain at night. 15. Much dew: the rain kept off till evening. 16. Windy: wet: a lunar corona. 17. Morning clear, with dew: then *Cumulus*, which becoming dense inosculated with the clouds above and with the smoke: a little rain p. m.: lunar corona. 18. Slight hoar frost: haze, passing to *Cirrus* and *Cirrostratus*. 19. Heavy *Cumulostrati* through the day: much wind. 20. Windy: wet: lunar halo at night. 21, 22. *Cumulostratus*: windy. 23. Fine. 24. Ramified *Cirrostratus*, indicating wind, which followed. 25. Blustering wind, with *Cumulostratus*, and strong evaporation: a little rain notwithstanding. 26. Showers, which laid the dust: a sudden clearing in the W. at sun-set. 27. Very white frost: a fine day, out of the smoke. 28. Hoar frost, and a frozen mist: a serene day.

*Third Month.*—1. Hoar frost: *Cirrus* in a fine elevated veil, passing to *Cirrostratus*, and exhibiting between ten and eleven a solar halo: a very fine day, the pollution of the smoke excepted.

## RESULTS.

Winds variable, but for the most part Southerly.

Barometer: Greatest height . . . . . 30.47 inches

Least . . . . . 29.28

Mean of the period . . . . . 29.785

Thermometer: Greatest height . . . . .  $57^{\circ}$

Least . . . . . 25

Mean of the period . . . . .  $44.43$

Evaporation, 0.80 inch. Rain, 1.18 inch.

TOTTENHAM,

L. HOWARD.

*Third Month*, 10, 1815.

# ANNALS OF PHILOSOPHY.

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MAY, 1815.

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## ARTICLE I.

*Biographical Account of Joseph Black, M. D. F. R. S. E. &c.  
Professor of Chemistry in the University of Edinburgh.*

By Thomas Thomson, M. D. F. R. S.

**THE** materials from which the following account was drawn were first given to the public in the preface to Dr. Black's lectures, edited by Professor Robison. Mr. Robison informs us that he was indebted for most of his facts "to a paper read to the Royal Society of Edinburgh by the near relation of Dr. Black, Dr. Adam Ferguson, Professor of Mathematics in the University, and well known in the republic of letters by works of the very first rank."

Dr. Joseph Black was born in France on the banks of the Garonne in the year 1728. His father, Mr. John Black, was a native of Belfast, in Ireland, but of a Scotch family, which had been some time settled there. Mr. Black resided for the most part at Bourdeaux, where he carried on the wine trade. He married a daughter of Mr. Robert Gordon, of Hilhead, in Aberdeenshire, who was also engaged in the same trade at Bourdeaux.

The mother of Dr. Black, and the mother of Mr. James Russel, Professor of Natural Philosophy in the University of Edinburgh, were sisters; and the mother of Dr. Adam Ferguson was their aunt, a circumstance which was the origin, though not the cement, of a friendship subsisting between them through life.

In the year 1740 young Black, then in the 12th year of his age, was sent to Belfast, that he might have the education of a British subject. After finishing his grammar school education, he went, in 1746, to the University of Glasgow. Dr. Cullen had commenced his great literary career, and having made choice of philosophical

chemistry as a field still untrodden, was delivering lectures upon that science in the University of Glasgow. These lectures caught the congenial fancy of young Black, who speedily became a zealous chemist, and the favourite pupil and friend of his master. Mr. Black had made choice of medicine as the profession to which he proposed to attach himself; and in 1750 or 1751 he went to the University of Edinburgh to finish his medical studies. Here he lived in the house of his cousin German, Mr. James Russel, Professor of Natural Philosophy, in whose society he must have passed his time both agreeably and profitably.

At this period the opinions of the medical professors were divided about the manner in which certain lithontriptic medicines acted in alleviating the excruciating pains of the stone. One of these medicines was lime-water. They all belonged to the class of bodies called *caustic*, and their efficacy was ascribed to this *causticity*. Now this causticity was always induced, either directly or indirectly, by the fire. Thus lime-stone in its natural state possesses no caustic properties; but by exposure to a strong heat it is converted into the caustic substance called quick-lime. The investigation of the nature and cause of this causticity was considered as very important. It drew the particular attention of Mr. Black. He investigated the subject with his accustomed precision and coolness; and having ascertained it in a satisfactory manner, made it the subject of his inaugural dissertation in 1754, when the degree of Doctor of Medicine was conferred upon him by the University of Edinburgh. Next year he published his experiments upon *magnesia alba*, *quick-lime*, and other *alkaline substances*, in which the whole subject was developed at length.

Just at this time Dr. Cullen was removed to Edinburgh, and the chemical chair in Glasgow became vacant. Dr. Black's experiments on magnesia and quick-lime, which afforded by far the finest specimen of chemical investigation hitherto offered to the public, secured him that chair. Accordingly he was appointed Professor of Anatomy, and Lecturer on Chemistry, in the University of Glasgow. Not considering himself as sufficiently qualified for the anatomical class, he exchanged tasks with the Professor of Medicine. While in Glasgow, therefore, his lectures on the institutes of medicine constituted his chief task. He engaged likewise in the practice of medicine; and from the sweetness of his manners, and the goodness of his heart, soon became a favourite practitioner.

While in Glasgow, he brought to maturity his speculations concerning heat, which had occupied his attention from the very commencement of his medical studies. They constitute one of the most important additions ever made to science, and furnish the most delightful proofs of the wisdom and beneficence of the Author of nature.

Dr. Black continued in the University of Glasgow from 1756 to 1766. In that year Dr. Cullen, Chemical Professor in Edinburgh, was appointed Professor of Medicine, and thus a vacancy was made

in the chemical chair of that University. There was but one wish with respect to a successor. Indeed, when the vacancy happened in 1756, by the death of Dr. Plummer, Dr. Black's reputation stood so high, that, had it depended on the University, he would have been appointed to the chair. He had now greatly added to his claim of merit by his important discovery of the procedure of Nature in producing fluidity and vapour; and he had acquired the high esteem of every one by the singular moderation and scrupulous caution which marked all his researches. Dr. Black was appointed to the chemical chair of Edinburgh, to the general satisfaction of the public; but the University of Glasgow sustained an irreparable loss. In this new scene his talents were more conspicuous, and more extensively useful. The number of his pupils underwent a progressive and annual increase during the whole time that he was Professor. Many of these pupils were from the workshop of the artist or manufacturer, and had not enjoyed the advantage of a liberal education. Yet such persons, in the opinion of Dr. Black; constituted by no means the least important part of his class. He laboured, therefore, with the greatest assiduity, to bring his lectures to a level with this least informed part of his audience; and thus every year he rendered them more and more elementary. His lectures were always listened to by his audience with inexpressible delight. His voice was low; but sweet and distinct. His language was simplicity itself; but always apposite, and never vulgar. His experimental illustrations were exactly suited to the object in view, and carried full conviction to the mind of the spectator: there was no glare, no parade, no showman exhibition; but an attic elegance and simplicity highly delightful to a refined and cultivated mind. I describe the lectures such as I listened to them myself, about the year 1796. But at that period Dr. Black's vigour was nearly gone. Indeed his state of health was such that he was obliged to employ an assistant to help him in his experiments, and to lecture for him occasionally. But even with all these disadvantages, the effects of his lectures were such as I can never forget: they made an impression on my mind which no time can efface.

Dr. Black's health had been always delicate. The least exertion brought on a cough, with a spitting of blood. This obliged him to remain a tranquil spectator of the chemical discoveries which were constantly pouring in from all quarters, and to leave it to others to explore the tempting fields which he had originally laid open. Towards the latter period of his life, when undue advantages were taken by certain foreigners of the discoveries which he had made, without any acknowledgment of obligations to the original discoverer, he was urged by his friends to lay an historical detail of the whole of his labours before the public. He began this task more than once; but was always obliged to desist almost immediately, in consequence of the illness brought on by this unusual exertion of thought.

By abstaining from all exertion, by living in the most abstemious manner, and by constant, though moderate exercise, he contrived to enjoy an almost uninterrupted, though feeble state of health, and to prolong his life to a considerable old age, happy to the last day, and capable of enjoying the conversation of a few select friends. His only apprehension was that of a long continued sick bed; and this perhaps less from any selfish feeling than from the humane consideration of the trouble and distress occasioned to attending friends; and never was this modest and generous wish more completely gratified. "On the 26th Nov. 1799, and in the 71st year of his age, he expired without any convulsion, shock, or stupor, to announce or retard the approach of death. Being at table with his usual fare—some bread, a few prunes, and a measured quantity of milk diluted with water; and having the cup in his hand when the last stroke of his pulse was to be given, he set it down on his knees, which were joined together, and kept it steady with his hand in the manner of a person perfectly at ease; and in this attitude expired, without spilling a drop, and without a writhe on his countenance; as if an experiment had been required to show to his friends the facility with which he departed. His servant opened the door to tell him that some one had left his name, but getting no answer, stepped about half way towards him, and seeing him sitting in that easy posture, supporting his bason of milk with one hand, he thought that he had dropped asleep, which he had sometimes seen happen after his meals. He went back and shut the door; but before he got down stairs some anxiety, which he could not account for, made him return and look again at his master. Even then he was satisfied, after coming pretty near him, and turned to go away; but again returning, and coming quite close to him, he found him without life.

"So ended a life which had passed in the most correct application of reason and good sense to all the objects of pursuit which Providence had prescribed to his lot, with many topics of agreeable recollection, and few things to ruffle his thoughts. He had long enjoyed the tender and affectionate regard of parents whom he loved, honoured, and revered; with the delightful consciousness of being a dutiful son, and being cherished as such—one of a family remarkable for sweetness of disposition and manners, he had lived with his brothers and sisters in terms of mutual love and attachment. He had never lost a friend, but by the stroke of mortality, and he felt himself worthy of that constancy of regard. He had followed a profession altogether to his taste; and had followed it in a manner, and with a success, which procured him the esteem and respect of all competent judges, and set his name among the most eminent; and he was conscious that his reputation was not unmerited; and with a success in point of emolument which secured the respect even of the ignorant; which gave him the command of every rational gratification, and enabled him to add greatly to the

comforts of the numerous descendants of his worthy parents—heirs, not only of their name, but likewise of their unambitious moderation and amiable simplicity of character.”

The publications of Dr. Black (omitting his inaugural dissertation) amount only to three short papers; for respecting his discoveries and speculations concerning heat, he published no account at all. In the year 1803, or three years after his death, his lectures were published by Dr. John Robison, Professor of Natural Philosophy in the University of Edinburgh, who undertook the task of editor at the request of Dr. Black's friends. They appear to me to be a pretty fair representation of his lectures such as they were about the year 1796; and though it was by no means doing justice to Dr. Black to give to the world lectures never intended for publication, and just at a period too when the science had assumed a new form, quite different from that under which he had been accustomed to view it; yet they convey a very happy view of the elegant simplicity of manner by which he was distinguished, of the very apt illustrations by which the different subjects that he treated were elucidated, and of the true philosophical caution with which all his conclusions were drawn.

His Experiments upon *Magnesia Alba*, Quick-Lime, and other Alkaline Substances, were published in 1755, and constitute in my opinion one of the very best specimens of analytical investigation ever offered to the public. It was known that when lime-stone is exposed to a violent heat for a sufficient time, it is converted into quick-lime, a substance which has a strong caustic taste, gives a green colour to vegetable blues, corrodes animal and vegetable substances, &c. If pearl ash be mixed with three or four times its weight of quick-lime, and agitated for some hours in a quantity of water, the quick-lime is converted into chalk, and the pearl-ash becomes exceedingly caustic and corrosive. These facts were sufficiently known; but no satisfactory explanation of them had been offered, though it was generally believed that the fire had imparted something to the lime to which it was indebted for its causticity. Dr. Black demonstrated, by the most decisive experiments, that lime-stone is a compound of two substances; namely, quick-lime and a peculiar species of air which acts the part of an acid, and to which he gave the name of *fixed air*. The fire disengages this air, and drives it off. The quick-lime remains in a state of purity, and exhibits its natural caustic properties. Pearl-ash is in like manner a compound of pure potash and fixed air. Quick-lime has a stronger affinity for fixed air than potash has. Accordingly it separates the fixed air from the alkali, and is converted into lime-stone; while the alkali, thus freed from the acid with which it was united, exhibits its natural caustic properties in all their energy. Such is the outline of the theory which Dr. Black establishes in his dissertation. He establishes likewise the peculiar properties of magnesia, and shows that it differs from every other earthy body.

Dr. Black's speculations respecting heat have had such an effect upon the progress of the science, that it would be unpardonable not to notice them here. A very good account of them will be found in the first volume of his lectures. Indeed, his lectures on heat constituted the most finished, and by far the most valuable, part of his course. It is well known that the freezing point of water is  $32^{\circ}$ , that whenever the thermometer sinks below  $32^{\circ}$  water begins to freeze, and whenever it rises above  $32^{\circ}$  ice and snow begin to melt. At the first view of the subject one would be disposed to expect that as soon as the thermometer sinks below  $32^{\circ}$  the whole water would immediately become ice, and that when it rises above  $32^{\circ}$  the ice would be as speedily converted into water; but every body knows that these speedy changes never take place. In cold weather a crust of ice is formed upon the surface of rivers and lakes; and if the cold continue, this crust becomes gradually thicker. But unless the water be very shallow, it is very seldom or never totally converted into ice. The warm weather returns while a considerable portion of the water of the lake is still unfrozen. We remark the same slowness in the conversion of ice into water. When snow is accumulated in great quantities in mountainous countries, it resists the united action of the sun and the wind for weeks, or even months. It is always melting, indeed, but it melts very slowly; and in some cases the cold weather returns again before the liquefaction is completed. Such were the facts which had been obvious to all the world from the beginning. Dr. Black was the first person who examined them closely and endeavoured to explain them. According to him, water is a compound of two substances—ice and heat. It cannot freeze or be converted into ice till it has parted with its heat; and as the heat makes its escape but slowly, the water freezes but slowly. Ice, on the other hand, can only be converted into water by combining with a certain quantity of heat; and as this combination takes place but slowly, the ice melts but slowly. This view of the subject Dr. Black confirmed by simple but satisfactory experiments. The heat which thus renders water fluid he called *latent heat*, because its presence is not indicated by the thermometer. He showed that the latent heat of water is  $140^{\circ}$ . He ascertained likewise that fluidity in all cases is owing to the combination of latent heat with the body becoming fluid.

It is well known that water and other liquids, when exposed to heat, increase in temperature till they become boiling hot, but after that their temperature remains stationary. They gradually indeed boil away, and are converted into steam or vapour, an elastic fluid possessing many of the properties of air; with this difference, that when exposed to the action of cold it is again converted into the very liquid from which it was originally produced. Dr. Black showed that vapour or steam is a combination of the liquid from which it was produced and latent heat. The latent heat of the vapour of water or steam he found about  $950^{\circ}$ . It was this doctrine

respecting the nature of steam that led Mr. Watt to his great improvements on the steam-engine—improvements which have been of incalculable benefit to the manufactures of Great Britain.

Dr. Black was the first person who pointed out that every substance is possessed of a peculiar *specific heat*, or that different bodies have different *capacities* for heat. This subject was afterwards further investigated by Dr. Irvine, of Glasgow, and by Mr. Wilcke, of Stockholm.

A very short paper by Dr. Black was published in the 65th volume of the Philosophical Transactions, for the year 1775, giving an account of some experiments showing that recently boiled water begins to freeze more speedily than water that has not been boiled. He found that if the unboiled water be continually stirred, it begins to freeze as soon as the boiled water. He gives the following explanation of the phenomenon. Water by boiling is deprived of a portion of air. When exposed to the atmosphere it begins to absorb this air, and continues to do so till it has recovered its original quantity. This absorption produces a disturbance in the water, not indeed sufficient to be perceived by the eye, but sufficient to prevent it from becoming colder than  $32^{\circ}$ , without beginning to freeze.

The only other paper written by Dr. Black was published in the second volume of the Transactions of the Royal Society of Edinburgh. It is an analysis of the Geyzer and Rikum springs in Iceland. A quantity of the water of these springs was brought from Iceland by Sir John Thomas Stanley, and sent to Dr. Black. This paper may be taken as a model of the proper manner of examining mineral waters. The following were the constituents found in 10,000 grains of each of these waters:—

	Rikum.	Geyzer.
Soda .....	0·51 gr.	0·95 gr.
Alumina .....	0·05	0·48
Silica .....	3·73	5·40
Common salt .....	2·90	2·46
Sulphate of soda .....	1·28	1·46
	<hr/> 8·47	<hr/> 10·75

## ARTICLE II,

*On the Doctrine of Fluxions.* By Alexander Christison, Esq.  
Professor of Humanity in the University of Edinburgh.

(To Dr. Thomson.)

MY DEAR SIR,

It is very much to be regretted that many students at the universities of North Britain acquire no knowledge of fluxions. They

seem to think it extremely difficult to obtain even the first principles of that important branch of mathematical science. In what follows, (which, if you think proper, you may insert in the *Annals of Philosophy*.) I intend to solve inductively the fluxional problem as extensively as Newton has demonstrated it in the second lemma of the second book; then to demonstrate that problem rigorously in the manner of the ancients, independently of infinitesimals, of motion, or of vanishing quantities; and, lastly, to subjoin some observations.

I am, my dear Sir, yours faithfully,

Edinburgh, March 20, 1815.

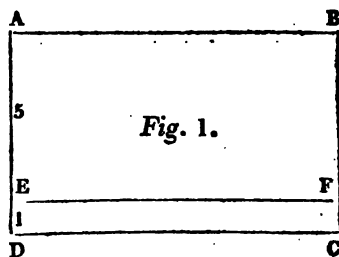
ALEX. CHRISTISON.

### Of Fluxions.

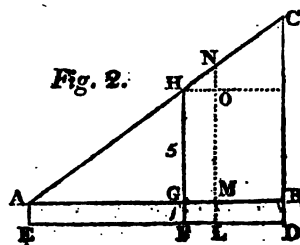
In consequence of repeated trials, I have long thought that a boy, duly prepared, passes from common algebra into fluxions as easily as he does from multiplication into division.

In solving the fluxional problem three things are to be distinguished—the conception, the notation, the demonstration.

1. With regard to the conception, I shall, in order to be easily understood, proceed as if I were questioning a learner. Ask him thus—If a straight line,  $A D$ , fig. 1, move parallel to itself at right angles along  $D C$ , blackening the parallelogram  $A F$ , whose side  $A E$  or  $B F$  is 5; and reddening the parallelogram  $E C$ , whose side  $E D$  or  $F C$  is unit; at what rate does it always blacken the one parallelogram and redden the other? He will answer—As 5 to 1. The conception is much aided, at first, by his imagining that the two parallelograms are generated of different colours.



Ask him now thus—If a straight line move parallel to itself, at right angles along  $E D$ , fig. 2, so that it can generate only the parallelogram  $A D$ , whose side  $A E$  is unit, and the triangle  $A B C$  half a square, while all the rest of the space is covered; at what rate does it, at  $H F = 6$ , generate the triangle and the parallelogram? He will answer—As 5 to 1; or as  $5 \times 1 : 1 \times 1$ ; that is, as the area of the parallelogram  $H M$  is to the area of the parallelogram  $G L$ ; but he will probably add, that the instant before the rate was less, and that the instant after it will be greater. He may be told, that it is not the rate the instant before, nor the instant after,



which is wanted. He will now understand that if the line  $FH$  were to proceed for ever of the same length, it would generate two parallelograms which would have always to one another the rate of 5 to 1. If, therefore,  $x$  represent the base or the perpendicular of the triangle, the rate will be as  $x$  to 1.

2. With regard to the notation, if  $x$  and 1 be both multiplied by any quantity whatever, their rate will not be changed; instead of  $x$  to 1, therefore, we may employ  $x \dot{x}$  to  $1 \dot{x}$ ,  $\dot{x}$  being any line more than nothing, and less than infinite:  $x \dot{x}$ , then, is the fluxion of the triangle, and  $1 \dot{x}$  the corresponding fluxion of the parallelogram; and as the triangle is the half of a square, the fluxion of a square whose side is  $x$  is to the fluxion of a parallelogram whose side is 1 as  $2x \dot{x} : 1 \dot{x}$ .

If now the following series of fluents be set down, the learner will easily continue the fluxions.

Fluents.	Fluxions.
1 .....	0, for 1 being invariable has no fluxion.
$x$ .....	$1 \dot{x}$
$xx$ .....	$2x \dot{x}$
$xxx$ .....	$3xx \dot{x}$
$xxxx$ .....	$4xxx \dot{x}$
.....	.....
$x^n$ .....	$n x^{n-1} \dot{x}$

A learner who sees in the series of the fluxions above the two laws of the three first terms, that of the numeral coefficients, and that of the letters, will be able to continue the series to any length, and to give the general expression  $n x^{n-1} \dot{x}$  as the fluxion of  $x^n$ ; because he observes that the numeral coefficients increase by unity, and that there are as many letters in the fluxion as in the corresponding fluent, with the last letter always dotted.

If the result be expressed in words, we have the following rule for finding the fluxion of any power of a variable quantity.

Multiply the fluxion of the root by the exponent of the power, and the product by that power of the same root whose exponent is less by unity than the given exponent.

By this rule the fluxion of  $x^{\frac{m}{n}}$  is  $\frac{m}{n} x^{\frac{m}{n}-1} \dot{x}$ ; of  $x^{-\frac{m}{n}}$  is  $-\frac{m}{n} x^{-\frac{m}{n}-1} \dot{x}$ ; of  $x^m y^n$  is  $m y^n x^{m-1} \dot{x} + n x^m y^{n-1} \dot{y}$ , when both  $x$  and  $y$  vary, by considering first  $x$  as variable, and then  $y$  as also variable; of  $\frac{x^m}{y^n} = x^m y^{-n}$  is  $\frac{m y^n x^{m-1} \dot{x} - n x^m y^{n-1} \dot{y}}{y^{2n}}$ .

From all that has been said, the fluxional or differential calculus may, in the case of one variable quantity, be defined a method for finding the rate of change in a quantity, and its dependance or function. Thus the rate of change in  $x$  and its function  $x^n$ , is as  $\dot{x} : n x^{n-1} \dot{x}$ , or as  $1 : n x^{n-1}$ .

As this is not a treatise, but a short essay, I say nothing of second fluxions, which bear the same relation to first fluxions that first



have  $\frac{y''}{i^2} = \frac{y'''}{i}$ . Now it is evident that  $FG$  is the limit of  $y'$ ,  $y''$ , &c.; for  $y'$ ,  $y''$ , &c. may approach, by continued bisections, nearer the point  $G$  than by any assigned difference, how small soever, but can never pass that point.  $\frac{FG}{EF}$  is the geometrical limit of  $\frac{y'}{i}$ ,  $\frac{y''}{i}$ , &c. which are successive values of  $\frac{y' - y}{i}$ , while  $i$  is diminished by continual bisections; as  $n x^{n-1}$  is the algebraical limit.

That  $n x^{n-1}$  and  $\frac{FG}{EF}$ , the limits of  $\frac{y' - y}{i}$ , are equal to one another, may be proved thus:—If they be not equal, let  $\frac{FG}{EF}$  be the greater, and let  $D$  be the difference between  $\frac{FG}{EF}$  and  $n x^{n-1}$ ; then, because  $\frac{y' - y}{i}$ , or its equal  $\frac{y'}{i}$ , &c. is always greater than  $\frac{FG}{EF}$ ,  $\frac{y' - y}{i}$  cannot approach nearer to  $n x^{n-1}$  than by more than  $D$ , but  $\frac{y' - y}{i}$  approaches nearer to  $n x^{n-1}$  than by any assigned difference; therefore  $\frac{FG}{EF}$  is not greater than  $n x^{n-1}$ ; and in the same manner it may be proved not to be less; wherefore  $\frac{FG}{EF}$  is equal to  $n x^{n-1}$ . Q. E. D.

If  $x$  be put for  $EF$ , and  $y$  for  $FG$ , then  $\frac{y}{x} = n x^{n-1}$ .

4. *Observations.*—This reasoning seems to me to remove Lagrange's objection lately mentioned, and to do so by employing an incremental fraction  $\frac{y}{i}$ , &c. equal to an approximating fraction  $\frac{y'}{i}$ , &c. of which approximating fraction the denominator is always  $i = x = EF$ , the numerator continually approaches to  $FG = y$ , and the vanishing quantity is the difference between the numerator  $y'$ , &c. and  $FG = y$ .

This approximating fraction  $\frac{y'}{i}$ , &c. is always greater than  $\frac{y}{x}$  when the curve, as in fig. 3, is convex to the axis; if a curve concave to the axis be drawn through the point  $E$  in the figure, another approximating fraction less than  $\frac{y}{x}$  will approach  $\frac{y}{x}$  from below the tangent, and  $\frac{y}{x}$  is, in the strictest sense of the word, the limit which the fractions approximating from above and from below the tangent can never pass.

Thus another objection of Lagrange is removed. This objection is, that what in the fluxional or differential calculus are called limits, are not so, in the strict ancient meaning of the word limit.

Whatever be the magnitude of each of the quantities  $E$   $F$  and  $F G = \dot{x}$  and  $\dot{y}$ , the fraction  $\frac{\dot{y}}{\dot{x}}$  is of the same value at the point  $E$  of the curve; the fluxions or differentials, therefore, are not zeroes, though Euler and others have endeavoured to show that they are zeroes only; but they are indeterminate quantities with a determinate ratio. They are, therefore, common algebraic quantities; and may be sometimes zeroes, sometimes infinite.

I have taken, as a function of  $x$ ,  $y = x^n$ . But I might have taken another function of  $x$ . Perhaps it would have been better, because more general, if I had taken  $y = f x$ ;  $y' = f(x + i) = f x + p i + q i^2 + r i^3 + \&c.$ ,  $i$  being an indeterminate quantity; the reasoning, however, would, with certain conditions, have been the same, but probably less intelligible to a learner. The conditions are, that from  $x$  to  $x + i$  the curve turns its concavity to the same side, and that the ordinates continue to increase.

From what has been said, it is easy to teach the learner the formulas for tangents, quadratures, cubatures, complanations, and rectifications of lines, whether of single or of double curvature.

It is likewise so easy to teach him to calculate a few logarithms, by the series derived from the asymptotic spaces of the hyperbola, that I think the calculation and demonstration should always precede trigonometry. All this he may do without hearing the hyperbola even named.

From the 47th proposition of the first book, or from the 14th proposition of the second book, of Euclid, he may derive an algebraic equation to the circle; and from that equation he may, without a previous knowledge of the conic sections, obtain equations to the ellipse, hyperbola, and parabola; he may, indeed, obtain an equation to the parabola without any knowledge of an equation to the circle. Thus he would have various subjects to which he might apply the fluxional calculus.

Might not those authors who in algebra treat of variable quantities, subjoin the rate of variation, and thus make fluxions a part of common algebra?

### ARTICLE III.

*On the Methods of detecting Carbureted Hydrogen Gas in Coal-Mines without setting it on Fire.* By John B. Longmire.

It is not generally known that miners are in possession of means which, if properly used, will detect the presence of carbureted

hydrogen, or inflammable gas, in coal-mines, before it can do them any injury. For this purpose they make use of the smell and the sight simply, and the changes induced on the top of a burning candle.

A tabular body of mist-like gaseous matter is sometimes observed under the roof of the mine, and every motion communicated to the air gives it a corresponding one. Miners have reasons, deduced from dear-bought experience, to suspect the inflammability of a body presenting this appearance. Such a body, however, does not always possess this property: for it may consist of a mixture of carbureted hydrogen gas and the vapour of water, which on entering the mine ascends to the roof, and remains there till it is displaced by a current of air, or accidentally set on fire by the miners; or it may be the tobacco-smoke of the hewers and the vapour of water which proceeds from the breath and bodies of the hewers, and the persons employed to remove the coals; and which, along with the tobacco-smoke, is dispersed through the air by the people at work, but which during their absence ascends to the roof, and collects into a stratum of vapour, so similar in appearance to the mixture of gas and vapour, as not to be distinguished from it but by the most experienced eye. In this view of the subject, this method is fallible; and as the carbureted hydrogen gas is very often pure, or unmixed with any other gaseous matter, its body is not visible. Hence in either case a miner who trusts to his sight only will be very often deceived.

A miner who uses the smell leaves his candle in a working at a sufficient distance from its forehead, and advances slowly towards it; he then traverses the working in many directions; and if he finds no smell indicative of the presence of the inflammable gas, he brings his candle to the forehead. But pure carbureted hydrogen gas is odourless, and of course when its presence can be known by the smell, it must be mixed with other kinds of gaseous matter. It is said sometimes to have a sulphureous odour. I have not, however, been able to detect it by this; but I have often done so by an empyreumatic odour, which probably proceeds from a subtile oily substance that sometimes leaves the coal along with the gas. As carbureted hydrogen gas in coal-mines is often pure, its existence in them cannot always be known by the smell. Neither the sight nor the smell, therefore, are to be implicitly relied on; but they are often very useful; for a man may be travelling through a mine hastily, or inattentive to the state of the circulating air, with his head in a mixture of inflammable and common air, and his hands and candle in the common air only, and be advised by the sight or smell of the presence of the inflammable air, before he accidentally elevates the candle and sets it on fire.

But the miner can place the greatest reliance on certain changes induced by the inflammable air on the top of a burning candle. The miner's term of the candle-top will not be understood without an explanation. It is that faintly luminous yellowish-brown body

which closely surrounds the flame, and which is largest above it, and at last passes into the smoke. This body is rather obscurely seen round the flame of a candle in the night, in a room not much illuminated by any other body; but it may be distinctly seen by using the following means:—Hold the candle in one hand at about a foot or 18 inches from the eye, interpose the other hand between the flame and the eye at about one inch from the candle, and let only a very small portion of the upper part of the flame be seen. By this arrangement the vivid light emanating from the flame will be intercepted, and the body in question observed for one-eighth to a quarter of an inch, and sometimes higher, above the flame. By changing the position of the hand, so that a small portion of the flame round either side may be seen, this body will have, from the top to the middle of the flame, a yellowish-brown colour, about one-sixteenth of an inch broad; below the middle, the same colour inclining a little to a purple; and downwards it gradually passes into an ultramarine blue, which is the colour of the flame at its bottom. At that place too, this body joins the flame. Such is that body which the miner calls the candle-top, as it appears when a candle is burning in pure atmospheric air; and as he uses only that part of it which is above the flame, his term of *top* is very applicable.

Now there are certain differences in the top of a candle between its burning in a mixture of atmospheric air and carbureted hydrogen gas, and in the air only. These differences are proportionate to the quantity of gas in the mixture, and point out to the miner when the quantity is such that a very little more would cause the instantaneous combustion of the whole body of gas. The alteration in the top consists of a change in colour, and of an increase in its dimensions. These changes become greater as the quantity of gas increases. The change of colour which is universally considered as a sure indication of the existence of carbureted hydrogen gas, and of course the most dangerous change, is from *yellowish-brown to greenish-blue*; especially when it is accompanied with a rapid assension of very small luminous bodies, generally called points, which appear to be propelled upwards through the flame and top with a very faint crackling noise. The precise dimensions of a dangerous top is not the same in all coal-pits, nor in every situation in the same pit, nor in every candle in the same situation: so that the top which might be considered very dangerous, in point of size, in one situation, is not so much so in another. Hence a great latitude is left for the miner's judgment and prudence; and if he be experienced in his candle symptoms, and well acquainted with local differences, he may, if necessary, venture much further in a mixture of airs, than a stranger to that particular situation with the same degree of judgment. I have seen the greenish-blue top one inch and a quarter long above the flame of the candle. Other miners say they have seen it longer; but others say the inflammable air has fired when it was shorter. With respect to this variation in

the size, one general rule may be adopted, which is, not knowingly to get past the limits of certain experience, unless the object to be obtained is much greater than the risk run. The size of the top appears to me to depend in a great degree on the heat of the mine. I have examined the top in parts of a mine where the air was stagnant but pure, and I always found it greatest in the hottest part. In making these experiments I have often seen it three, sometimes four, and once five, inches above the flame, without any difference in the colour except in intensity, which increased with the size of the top. I once advanced towards the forehead of a working in which the air was still, with a candle having a deep yellowish-brown top, four inches long, till I observed longitudinal streaks of bluish-green were mixed with the yellowish-brown. I then began to retreat, being convinced that my situation was dangerous. I have often thought the moisture in the air of the mine affected the top; but I never met with proper situations to make so many experiments as would enable me to come to very accurate conclusions.

When a miner is about to use his candle he snuffs it, and then waits till the top of the wick is inflamed. As on such an occasion as this he generally carries his candle as near the ground as his arms will permit, he never raises his candle, but depresses his body till his head is nearly on a level with the candle, and probably he may kneel on one knee; he then, holding the candle in the manner before described, and having his eye steadily fixed on the top, rises slowly, till the candle is nearly as high as his face when he is standing upright. If he finds the top has a favourable appearance, he gently sinks his candle again, and advances a few yards; but if the top indicates the presence of the inflammable gas, he experiments more cautiously, and advances a less distance between every trial; if the quantity of gas appear to be increasing, he advances very slowly, with his eye constantly fixed on the top, and his body rather bent forward; and if the quantity be still increasing, and the appearance of the top rather alarming, he advances still more slowly, and pauses at every yard in length, to be more exact in his examination. And if the quantity of gas reach its maximum before he considers the top to be dangerous, he advances slowly to the end of his journey; but if it continue increasing, he will be forced to return.

Much steadiness is required in these trials; for sometimes the top suddenly assumes so alarming an appearance, that a person may be intimidated so much as to make a precipitate retreat; and in doing so sets the gas on fire, by bringing a quantity of it, or a *sop*, as the miner calls it, upon his candle, by his rapid movement. Before he tries, he should first consider what he ought to do in every probable exigency; so when the top suddenly appears dangerous, he will sink the candle slowly to the ground, and bend his body as much as he can at the same time, then turn about and retreat as slowly as avoiding, if possible, the direction that he advanced in during the dangerous part.

## ARTICLE IV.

*Population of the United States of America, and Territories thereof, agreeable with an actual Enumeration made according to Law, in the Year 1810.*

Names of Districts and Territories.	Males of all ages.	Females of all ages.	Strangers.	Slaves.	Total in each district.
Maine .....	115,509	112,227	969	—	228,705
Massachusetts .....	229,742	235,561	6,737	—	472,040
New Hampshire .....	105,982	107,508	970	—	214,460
Vermont .....	109,951	107,194	750	—	217,895
Rhode Island .....	35,743	37,471	3,609	108	76,931
Connecticut .....	126,373	128,806	6,453	310	261,942
New York .....	474,221	444,418	25,333	15,017	959,049
New Jersey .....	115,357	111,511	7,843	10,551	245,562
Pennsylvania .....	401,566	355,238	22,492	795	810,091
Delaware .....	28,006	27,555	13,156	4,177	72,874
Maryland .....	120,210	114,907	33,927	111,502	380,546
Virginia .....	280,038	271,496	30,570	392,518	974,622
Ohio .....	119,657	109,204	1,899	—	230,760
Kentucky .....	168,805	155,432	1,713	80,561	406,511
North Carolina .....	188,632	187,778	10,266	168,824	555,500
East Tennessee .....	46,813	44,668	510	9,376	101,367
West Tennessee .....	64,950	59,444	807	35,159	160,360
South Carolina .....	109,547	104,649	4,554	196,365	415,115
Georgia .....	75,845	69,569	1,801	105,218	252,433
Territory of Orleans .....	18,940	15,871	7,555	34,660	76,556
Mississippi .....	12,850	10,274	240	17,088	40,352
Louisiana .....	9,387	7,838	607	3,011	20,843
Indiana .....	12,590	11,320	393	237	24,520
Illinois .....	6,340	5,131	613	168	12,252
Michigan .....	2,837	1,781	120	24	4,762
District of Columbia .....	8,130	7,949	2,549	5,395	24,023

Total of free inhabitants in the United States .....

Territories .....

Total of strangers .....

Slaves .....

Grand total .....

*Population of the principal Towns in the United States of America.*

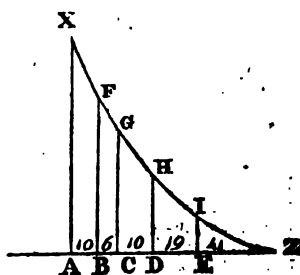
Boston .....	33,230	Philadelphia .....	33,722
Balem .....	12,613	East and West Northern Liberties of ditto .....	21,374
Providence .....	10,071	Baltimore .....	35,563
Newport, Rhode Island .....	7,907	Washington county .....	18,730
Newhaven city, Connecticut .....	5,772	Charlestown .....	24,711
New York .....	96,373	New Orleans .....	17,348
Newark, New Jersey .....	8,008		

I am indebted for the following curious speculation on this subject to John Leslie, Esq. F.R.S.E. Professor of Mathematics in the University of Edinburgh.—T.

The population of America at the different periods of life may be nearly represented by the differences between the complimentary spaces of a parabola.

Thus, the population under ten years of age may be represented by the space A B F X, of which the boundary X F is a portion of a parabola, X Z, whose vertex is Z.

From the property of the curve, D H is as  $D Z^2$ . Therefore D H  $\times$  D Z is as  $D Z^3$ ; and therefore any trilinear area, as D H Z, being one-third of the rectangle D H  $\times$  D Z, is also as  $D Z^3$ . Therefore the area



A F is as	A Z <sup>3</sup> — B Z <sup>3</sup>	or as	86 <sup>3</sup> — 76 <sup>3</sup>	=	636056	—	438976	=	197080
B G	B Z <sup>3</sup> — C Z <sup>3</sup>		76 <sup>3</sup> — 70 <sup>3</sup>	=	438976	—	343000	=	95976
C H	C Z <sup>3</sup> — D Z <sup>3</sup>		70 <sup>3</sup> — 60 <sup>3</sup>	=	343600	—	216000	=	127000
D I	D Z <sup>3</sup> — E Z <sup>3</sup>		60 <sup>3</sup> — 41 <sup>3</sup>	=	216000	—	68921	=	147079
E I Z	E Z <sup>3</sup> — 0 <sup>3</sup>		41 <sup>3</sup> — 0 <sup>3</sup>	=	68921	—	0	=	68921

If now we multiply these proportional areas by 10, and retain the nearest figure in the place of thousands, they will stand as in the second column below, while the gross return of population is as in the first.

	Aggregate Return of Males and Females.	Proportional Numbers on the Parabolic Theory.
Under 10 years of age ..	1,970,000 .....	1,970,000
10—16 .....	897,000 .....	960,000
16—26 .....	1,085,000 .....	1,270,000
26—45 .....	1,089,000 .....	1,471,000
45 and upwards ..	691,000 .....	689,000

It appears, therefore, that in the first and last periods the areas are very nearly proportional to the numbers alive at those periods. But in the second, third, and fourth, periods, these areas exceed the return; which excess increases from the earlier to the more advanced ages; so that the American population is increasing more rapidly than the parabolic theory supposed.

## ARTICLE V.

*Observations on the Objections that have been made to Mr. Prevost's Explanation of the Effect produced by a Mass of Ice on a Thermometer in the Focus of a Reflecting Mirror. By Mr. Richard Davenport.*

(To Dr. Thomson.)

SIR,

BEING on a visit in Edinburgh, I took advantage of the opportunity it gave me of attending the lectures of the Professor of Chemistry, and witnessing a series of admirably conducted experiments. In a lecture on caloric an experiment was made with a conical tube of polished metal, the result of which has been considered as tending to prove the actual radiation of positive cold. On considering the phenomenon here presented, it appeared to me that it might be accounted for exactly on the principle on which an apparent radiation of cold from a body of ice in the focus of a concave metallic mirror, on a thermometer in the focus of a similar mirror set opposite to it, has been explained; which explanation I had considered as satisfactory, and I had thought was generally admitted. However, on looking into Mr. Murray's *Elements of Chemistry*, I found he objected to Mr. Prevost's explanation, and considered the phenomenon as not yet accounted for. I found also in the *New Edinburgh Encyclopædia*, Mr. Murray's objections stated and admitted, as objections unanswered, and the whole question left in an undecided state.

Now as it appears to me that the phenomena both of the mirrors and the cone may be satisfactorily explained without supposing the actual existence and radiation of cold; and as the authorities I have mentioned are much looked up to; and as they leave an important question in philosophy in a state of indecision; if that explanation which I have said appears to my mind satisfactory, should on perusal appear so to you, you will perhaps not object to employ a page or two in the *Annals of Philosophy* by the admission of the following sketch.

I am, Sir,

Your obedient humble servant,

RICHARD DAVENPORT.

It is well known that if a body be placed in the principal focus of a concave parabolic reflecting metallic disc, and a hot body in the corresponding focus of a similar reflector standing at a distance from, but duly opposite to, the first, the radiating caloric of the hot body is reflected and condensed upon the body in the focus of the first reflector; proving that radiating caloric is reflected in like manner with rays of light. But if a comparatively cold body is

substituted for the hot radiating body in the focus of the second reflector, a thermometer in the focus of the first indicates a temperature below that of adjacent bodies not in that focus. This experiment was, I believe, first brought forward by Mr. Pictet, and the phenomenon at first appeared to favour the hypothesis of the actual radiation of cold; but a well-known explanation of it has been given by Mr. Prevost. I have not been able to see this explanation in Mr. Prevost's own words, but I collect it as referred to in other books.

He considers that caloric is radiated from bodies at all temperatures. The thermometer, therefore, itself gives out heat by radiation. In consequence of this universal radiation, all bodies must also receive radiations. The thermometer, therefore, is receiving heat also; and while it receives an equivalent for what it gives out, no change of temperature is to be expected. But he adds, that the quantity radiated from all bodies is in proportion to the difference of their temperatures. Since, then, comparatively cold bodies emit comparatively weak radiations, when ice is placed in the focus of the second mirror the thermometer receives less than it emits, and thus loses in temperature.

"But," says the writer of the article in the *Encyclopædia*, "this explanation is totally inconsistent with the most obvious facts attending the cooling of bodies;" for, he continues, "it is evident that on this hypothesis a hot body ought to cool more slowly when it is placed near a large body of inferior temperature than when near a small one; because in the former case it must receive more caloric emanations than in the latter."

This is precisely what I deny, notwithstanding the deference I feel for the opinions of the writer of this article, as well as for those of Mr. Murray, whose objections follow.

The article continues, "It is also very properly objected by Mr. Murray, that of different surfaces which at a given temperature radiate different quantities of caloric, that which radiates least must be least powerful in returning caloric to the thermometer, and must therefore have least effect in counteracting the reduction of its temperature." It then goes on to quote Mr. Murray as saying, that if Mr. Prevost's explanation were just, a blackened surface (which it is proved by Mr. Leslie's experiments radiates the largest quantity of caloric) ought to produce the least cooling effect on the thermometer, and a metallic surface (which radiates the least) ought to produce the greatest."

I shall beg leave to answer this objection with the preceding one.

The *Encyclopædia* continues, "Rejecting then this explanation of Mr. Prevost's, we shall proceed to consider that of Pictet;" to which also they object, and therefore leave the phenomena entirely unaccounted for.

The writer of the article, however, does not infer from this phenomenon the absolute existence of a frigorific principle, but only argues "that the objections against Mr. Prevost's and Mr.

Pictet's explanation of the indication of decrease of temperature in the focus of the reflector, apply with equal force in cases of cooling by direct radiation and absorption ;" which is certainly true.

I will now describe also the experiment I have above alluded to with the metallic cone.

A metallic conical tube of about 18 inches in length, whose respective apertures were one inch and five inches (the interior being polished so as to reflect powerfully and radiate little), being supported in a horizontal position, one ball of a differential thermometer was presented to the larger aperture, while a bottle of ice was presented at the smaller aperture. The liquor in the tube of the thermometer rose very little towards the bulb nearest the aperture, slightly indicating a lower temperature. The relative situations of the thermometer and the ice bottle being reversed, (*i.e.* the ice carried to the large end, and the bulb of the thermometer placed before the smaller end,) the liquor rose much faster towards the bulb, indicating in this position a much more rapid decrease of temperature.

In this case it was argued that since the rays admitted at the wide opening were brought to a focus at the smaller one, if the thermometer at the wide end radiated caloric, its rays must be lost upon the ice in the focus, and it ought to indicate a loss of temperature ; which was not the case, or only in a very small degree : but that when transposed, as this was the only focus (the experiment in that respect differing from that of the mirrors), the loss of temperature which it so rapidly indicated could arise only from a collection of actual cold rays in the focus ; and that this experiment constituted another objection to Mr. Prevost's theory.

This objection I shall endeavour to remove with the rest.

It will be convenient first to consider in what way a focus of rays (whether of light, or of heat, or of cold,) is produced.

Every point of a luminous body emits rays, proceeding in every direction in straight lines diverging from that point at all angles. Different parts of the same luminous body emit rays which cross each other at all angles ; but which, after a few reflections, are absorbed, or dissipated into boundless space. But since every body possesses free caloric, more or less, and every point of these emits rays of caloric, as the points of luminous bodies do those of light, it follows that every body is surrounded on all sides by radiating and by receiving points ; and may therefore be considered as surrounded by a sphere, from every point of which rays proceed and fall on it : and since the intensity is inversely as the squares of the distances, if the temperature of the parts of which the whole imaginary sphere consists is uniform, it receives equal quantities on all sides, although some of the surrounding bodies are much nearer than others ; because the quantity of surface of distant bodies, intercepted by the intervening near bodies, is conversely in the same ratio ; *i.e.* as the squares of the distances. Thus if a circular screen, whose area we will suppose equal to 36 square inches, be held at

the distance of one foot from the thermometer, and the wall of the room be at the distance of eight feet from the same, the screen intercepts from the thermometer an area of 2304 square inches; but the intensity of the radiation from the wall, which is at eight times the distance of the screen, is to the intensity of the radiation from the screen only as one to 64, or as 36 to 2304; and thus the difference in quantity of radiating surface is compensated by the difference in intensity.

Now by mere reflection the actual number of rays, or the intensity of any given number, cannot be either increased or diminished. The intensity (whether of light or heat) is increased in the focus solely by the direction to one point of a number of rays subtracted by a change of direction from other points on which they would have fallen in their natural course; for it is self-evident that whatever radiation is superadded by mere reflection on one spot, must be subtracted from some other; and if the sensible heat is increased in the focus, it must be diminished somewhere else.

Some kinds of surfaces receive and emit heat by radiation with great facility; others reflect more and radiate less; as Mr. Leslie has shown. Mr. Leslie has also shown that the powers of receiving and giving out heat are in the same surface equal; and also that its powers of radiation and reflection are in inverse ratios. This seems also demonstrable from all usual appearances; for if any body could radiate either more or less than it receives, or reflecting a part only of what falls on it, did not give out by radiation a sum equal to that which enters, not only would adjacent bodies be affected by it, but the sensible heat of the body itself must continue to increase or diminish, without apparent limits: but it is not found that the temperatures of adjacent or remote bodies differ from each other by mere position, where there is no source of heat or active cause of increase or diminution of the actual quantity of free caloric; and since different surfaces of the same body radiate or reflect one more than another, if the emission and intromission by radiation were not equal in the same surface, or if the reflecting surface did not radiate all which it does not reflect, bodies exposed near one surface must be affected differently from those on the other. Thus if a flat plate of metal were polished on one side, and blackened on the other, two bodies of equal temperature with the plate must receive different temperatures from it; still more would a body in the principal focus of a reflecting mirror be affected; for there is an actual accumulation of reflected rays upon it. Yet a body placed in or out of the focus, or even before the concave or behind the convex side of the mirror, suffers neither increase nor decrease of temperature, neither is it affected by the proximity or removal of either the plane reflector or the mirror. Where is the compensation? In the case of the plane reflecting plate, the radiation of nearly one half of the surrounding sphere is intercepted, viz. all behind the plate; but the same quantity of radiation from the hemisphere in front of the plate which would have (which has, indeed,) passed

the object, is reflected on it; and so much as the imperfection of the reflecting power of the plate suffers it to absorb, is radiated by it in the same direction. Here, then, is the compensation; and here I think Mr. Murray's objection fails. A blackened surface radiates much, it is true; but it intercepts an equal volume of radiation or reflection from behind. A polished surface radiates less, but it reflects as much as it fails to radiate. So when the writer in the *Encyclopædia* says "a hot body ought to cool more slowly when near a large body of inferior temperature than when near a small one," he forgets that this large body intercepts an equal volume from beyond it, and therefore the hot body so placed ought *not* "to cool more rapidly." He says the *reverse* is the case; but I apprehend it would be found, by careful experiment, that *if* the adjacent body be of the same temperature as those whose rays it intercepts (the screen, for instance, of the same temperature as the wall), the *reverse* will *not* be the case, but the time of cooling will be the *same*. (N. B. Dr. Wells's lately detailed interesting experiments on the formation of dew, in consequence of the loss of heat by uncompensated radiation, will be found to be in perfect conformity to this view of the subject.)

Then in the case of the concave reflector it is true that one hemisphere of converging rays is intercepted from behind, and only a cylinder of parallel rays in front, brought to the focus by reflection; yet this is a compensation: for the intensity of the rays so brought to the focus is equal to that of the converging rays which would have passed in the same direction through the space occupied by the disc, as might be easily shown by the general laws of radiation and reflection. When a second mirror faces the first, all this cylinder of rays is intercepted; but then all the rays which pass through its focus to its face are reflected in the direction of the intercepted cylinder, and a cylinder is formed of reflected rays, similar to, and in lieu of, the intercepted one. The whole cylindric space comprised between the two mirrors now consists of two sets of rays proceeding in exactly opposite directions from and to both the foci.

Let us now place a hot body in the focus of the second reflector. This last cylinder now consists of extraordinary calorific rays; and of course a body (as a thermometer) in the focus of the first mirror receives superabundant heat. Substitute an extraordinary cold body; then, as all the rays which would have passed through this focus, and would have constituted the second cylinder, are now absorbed by a body which has very little to radiate, the radiation of the thermometer towards the first mirror is uncompensated; and it indicates decrease of temperature.

In Mr. Pictet's detail of the experiment it was observed that when the temperature of the ice was lowered by the addition of nitrous acid, the thermometer sunk several degrees lower than *before*. The theory proceeds here with perfect regularity. Ice at  $+ 32$  radiates something towards the compensation: at  $- 20$ , still

something, though much less ; and therefore the small compensation was again reduced by the refrigerating mixture.

The comparing of the reflection of heat to that of light (though they are in many respects analogous) is apt to lead into some error, because in the latter case the reflector is presented in a particular direction towards a particular source (as to the sun, or to an aperture admitting the rays) ; but the rays of heat are received equally from every point of the surrounding sphere : but if we bear this difference in mind, the analogy between them will afford an evident illustration of the theory. Vision is merely the sensation of rays of light on the retina of the eye. We see a white or coloured object by means only of light reflected by it. Black, we say, reflects no light ; yet we see a black object. If the blackness of the surface were really perfect, and reflected no light, we could not (strictly speaking) see it at all ; we perceive or distinguish it by its interception of other forms and objects ; but we can also magnify it by dioptric lenses or mirrors. To put the comparison in a still stronger view, suppose a white object (as a paper box blackened within), and a small hole cut in one of its surfaces, we say we see that hole. Do we see darkness ? Through a convex lens, or by a concave mirror, or by the intervention of two mirrors (placed exactly as in Piétié's experiment, the eye being in the place of the thermometer), we may magnify that dark hole. Do we say darkness radiates ? And where is the difference between the two cases ; the one, where the ice in the focus of the second mirror lowers a thermometer in that of the first ; the other, where a dark spot in a similar situation gives the sensation of a magnified dark spot on the retina of the eye ? A perfect analogy holds here, and applies to all the cases, and removes at once (as it appears to me) all the objections made to Mr. Prevost's theory. It is true that in the quotations I have met with of Mr. Prevost's explanation he has not expressly described the *interception* by means of the ice, of radiation *aliunde* ; but I think it follows by necessary inference that he had it in his mind as a part of his theory, and meant to be so understood. I have said before that his own words I have not met with.

I have now to answer the supposed objection arising from the experiment with the metallic tube ; but if the explanations above given of the several phenomena are satisfactory, I have only to trace the analogy between them and this new one.

I think we are in possession of facts that warrant the following conclusions :—

1. That all bodies receiving and admitting heat by their surfaces emit by radiation an equal quantity ; and that as much of what falls on them as they cannot so admit, they reflect.
2. That when the radiation of any body is not compensated by counter radiation, its temperature must decrease.
3. That the proximity or the form either of radiating or reflecting surfaces produces no difference in the quantity or intensity of the rays received by any given point, provided there is no active cause

of increase or decrease of the actual quantity of free caloric in the surrounding sphere; but that where there is such cause, such given point will be affected by the form and nature of the reflecting surfaces which change the direction of rays that fall on, or proceed from, the point where such active cause exists.

4. That where heat is increased by more reflection on one spot, it must be diminished somewhere else, and *vice versa*.

5. That when any body whose temperature (arising from extraneous causes) varies from that of others in its neighbourhood, if hotter, it affects a body placed in any given point, by throwing on it a more intense radiation than that which it gives out; if colder, by intercepting and absorbing rays of free caloric from some part of the sphere by which it is surrounded, and thereby depriving it of its due and usual compensation.

If, then, by means of the conical tube, or by any other means, a thermometer indicates decrease of temperature from its relative position with that of a cold body, we have only to see where, and how are radiations that would have fallen on the thermometer intercepted by that cold body.

The mechanical form of the polished interior of this tube will effect this according to the known laws of optics. The effect of this reflection differs from that of the mirrors; for instead of a single focus at one point, there is an indefinite number of foci all along the axis. The incident rays which are parallel to the axis are reflected in the form of cones, and cross in the axis; however, the largest hollow cylinder of incident rays will form the focus of greatest intensity, and a multitude of other rays will be brought by complex reflections from the polished interior to the same spot. The tube is of course a *truncated cone*; for the smaller end has a diameter. The principal focus is not at the apex of the cone, but short of it; and we suppose the thermometer to be placed at this spot near the aperture. I need not trespass more on you by specifying all the circumstances of the situation; but it is easy to see that a large portion of the rays of the surrounding sphere is intercepted from this point by the tube, and reflected from its exterior surface in foreign directions. The loss of these rays is compensated by the concentration of the cylinder of rays brought to this point by reflection from within the tube, else the thermometer in this place must indicate decrease of temperature without the presence of the cold body, contrary to the rule No. 3. But when the bottle of ice is brought to the wide aperture, all this cylinder of radiation is intercepted and absorbed, and the radiation from the thermometer is uncompensated. A hot body in the place of the ice would have its radiation condensed on the thermometer. A visible object would be magnified in an eye placed in the room of the thermometer, and a dark hole in a white surface would have its diameter apparently increased; and thus a perfect analogy subsists between this experiment and those of Mr. Pictet,

Mr. Pictet's theory appears also capable of accounting for the

phenomena of his experiment, but appears to have been misunderstood. Still it involves some strong improbabilities; while that of Prevost, if we only admit certain analogies (some of which we know exist) between the radiation of heat and that of light, is even demonstrable. I was about to add a comparison between these two theories; but I have already made my letter much longer than I intended.

## ARTICLE VI.

*On the Cerebellum.* By W. Elford Leach, M.D.

(To Dr. Thomson.)

MY DEAR SIR,

IN the last number of your *Annals of Philosophy* Mr. A. Walker claims the merit of being the discoverer of what Dr. Cross has stated respecting the structure of the spinal marrow and the use of the cerebellum. He has at the same time answered my letter to Dr. Cross, and has stated that the work of Gall and Spurzheim "actually contains no such statements" as those to which I alluded.

Permit me, Sir, to assure you that the letter from Hufeland to Portal contains precisely the same opinion respecting the use of the cerebellum as that given by Mr. Alexander Walker and by Dr. Cross.\*

When I perceived Dr. Cross's observations on the anatomical structure of the spinal mass of nerves, I recollected that in the work of Gall and Spurzheim the same statements were given, not as existing in nature, but as erroneous suppositions. In the folio edition of their *Anatomie et Physiologie du Système Nerveux en général*, vol. 1. p. 35 and 40, the following statements may be found:—"Bartolin says that the spinal marrow is composed of four fibrous cords;"—"Soemmerring maintains the opinion that the spinal marrow is composed of four cords."†

My object in answering Dr. Cross was merely to show that although his opinion might have been original, yet that the same opinion had been entertained by preceding writers. Since writing that answer, I have carefully examined the structure of the spinal mass of nerves; and I most certainly agree with Drs. Gall and Spurzheim in maintaining that the spinal mass of nerves does not consist of "four columns," and that Mr. A. Walker merely participates in an error common to older writers in maintaining this opinion.

\* Willis likewise considered the cerebellum as the source of voluntary power.

† Highmore even goes farther: he pretends that the spinal marrow is formed of eight little cords.

It appears to me that it is of much greater consequence to disprove these statements, than to dispute about their origin. I shall, therefore, in the first place, examine the statements relating to the structure of the spinal marrow, and shall then produce some objections relative to the functions of the cerebellum as stated by Willis and by Mr. A. Walker.

It is said that the spinal marrow is composed of four longitudinal divisions, two larger, anterior, which may be traced into the cerebrum; and two smaller, posterior, which may be traced into the cerebellum. This statement is erroneous in two respects: 1. There are not four divisions; there exist only two fissures, one at the anterior part of the spinal marrow, the other at the hinder part; but there are no lateral fissures. Such are the statements of Gall and Spurzheim; and my recent examinations have convinced me of their truth. Gall and Spurzheim have shown that the spinal mass of nerves is composed of a series of as many swellings as there are hairs of nerves, and they have pointed out that the anterior fissure\* is less deep than the posterior, and that the posterior nerves are more considerable than the anterior. It is, therefore, erroneous to say that of the four divisions the anterior are larger, and the posterior smaller.

Mr. A. Walker considers the anterior fasciculi as ascending, and the two posterior as descending. Gall and Spurzheim have shown that the brain and cerebellum cannot be considered as the continuation of the spinal marrow, any more than the spinal marrow can that of the brain and cerebellum. There are examples of monstrous foetuses having only the lower parts of the spinal marrow, and yet the posterior and anterior (or, according to Mr. A. Walker, the ascending and descending,) fasciculi have been found to exist. It is, therefore, evident, that they cannot be considered as descending from the cerebellum. Gall and Spurzheim have also shown that not only the cerebellum, but also the hinder parts of the brain, are brought in communication with posterior parts of the spinal mass of nerves, and that only a small fasciculus of the hinder part of the spinal mass of nerves belongs to the cerebellum.

The physiological statements of Mr. A. Walker are not more accurate than his anatomical ones; they are merely suppositions, and at variance with nature. The cerebellum he considers as the organ of volition, because it is placed opposite to the face, which contains the chief organs of sense. The expression *opposite* as to function has no specific meaning. It cannot indicate a counteraction; if this were the case, the ears, which are situated opposite to one another, should act in opposition to each other.

I must repeat Mr. A. Walker's supposition in his own words:—"From the peculiar *opposition* which subsists between the situation of the face and cerebellum, we are entitled to expect a similar *opposition* in their functions. As the face, therefore, occupied by

\* Anat. et Phys. &c. pl. ii. fig. 6.

the chief organs of sense, is the seat of sensation, so we might expect *the cerebellum to be the organ of volition*. This supposition receives additional force from the consideration that, as the organs of sense and the cerebellum are the first and the last portions of the nervous system; so sensation and volition are the first and the last of its functions. But this supposition is completely confirmed when we recollect that the degrees of voluntary power *always* bear a close analogy to the various magnitudes of the cerebellum."

If we desire to listen to, or to look at, any object, the muscles support the action of seeing and of hearing: hence sensation and voluntary motion are not in opposition. It is also quite erroneous to suppose that only the posterior nerves of the spinal mass go to the muscles, and the anterior to the organs of sensation; and it is incorrect to believe that almost all the parts of the body have filaments of ascending and filaments of descending impressions. The two roots of nerves of each half of the spinal marrow, namely, the anterior and posterior, go to different parts of the body; and it is erroneous to admit that filaments of the anterior and posterior fasciculi of the spinal mass of nerves go to almost every part: the muscles and skin of the back receive their nerves from the posterior roots, whilst the muscles and skin of the abdomen receive theirs from the anterior roots, and yet the fore and back parts of the body have sensation and voluntary motion.

Mr. Alexander Walker, in considering the cerebellum in opposition to the face has neglected to take any notice of the brain. Does he therefore consider that part to have nothing to do with sensation and volition?

The degree of voluntary power bears no proportion with the size of the cerebellum.

Gall and Spurzheim have shown that the cerebellum in children is much smaller in proportion than in the adult, and yet children have more of muscular agility than adults.

The large shark described in the Philosophical Transactions for 1809 and 1813, an animal said to have the greatest locomotive power, has a remarkably minute cerebellum. See vol. an. 1809, pl. xix. *a*, and pl. xx. *a a*.

Swallows, which possess amazing locomotive powers, have a small cerebellum; and Cuvier has shown (*Leçons d'Anatomie Comp.* tom. ii. p. 153), that the proportions of the cerebellum and cerebrum in the bull and in man are absolutely the same, yet their degree of voluntary power is very different. It is true that fishes have a larger cerebellum in proportion to their brain, and that they possess a very strong locomotive power; and the rongeurs have likewise a very large cerebellum; and many of them, as the hare, squirrel, &c. possess a great locomotive power; but this is no general principle.

I shall here subjoin a few comparisons of the cerebellum and

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brain extracted from Cuvier (loc. cit.), and show that there is  
really no proportion between the degree of voluntary motion and  
the size of this organ.

Man and the ox .....	1—9
Dog .....	1—8
Horse and wild boar .....	1—7
Hare and cat .....	1—6
Sheep .....	1—5
Mole .....	1—4½
Rat .....	1—3½
Beaver .....	1—3
House mouse ( <i>mus musculus</i> ) .....	1—2

Mr. A. Walker has further observed, that “ Dr. Spurzheim has  
most certainly erred in denominating the cerebellum the organ of  
amativeness.” I have not time to give the facts which have led  
Drs. Gall and Spurzheim to hold this opinion, and must therefore  
refer to Dr. Spurzheim’s work on the Anatomy and Physiology of  
the Brain, where the statements are fully detailed.

I am, dear Sir,

Your obedient servant,

WM. ELFORD LEACH.

## ARTICLE VII.

*On Pyrophorus, and the making of Potassium.* By Dr. Hamel.

IN the appendix to vol. iv. of Priestley’s Experiments and Ob-  
servations relative to various Branches of Natural Philosophy,  
p. 481, mention is made of the following experiment of Mr. Wm.  
Bewly:—“ I mixed equal parts of sal-tartar and vegetable or  
animal coal, or sometimes three parts of the former with two of the  
latter, and calcined them in the usual manner (for making Hom-  
berg’s pyrophorus). This composition, on being exposed to the air,  
generally kindled in the space of half a minute or a minute. It  
did not burn with so much vivacity as the *vitriolic* pyrophori, as it  
contained no sulphur. I shall hereafter denominate this the *alkaline*  
pyrophorus. It differs in no one circumstance from M. du Suignys  
*neutral* pyrophori, except in its *not* containing that very principle to  
which he ascribes their accension. Lest it might be suspected that  
the salt of tartar which I employed might accidentally contain  
vitriolic tartar, or vitriolic acid, I repeated the experiments with  
tartar calcined by myself, as well as with nitre fixed or alkalized by  
*deflagration* with charcoal and with iron filings; but in all these

cases the event was the same. It is rather surprising that this *alkaline* pyrophorus should not have been discovered before, as I have more than once, since I first noticed it, on preparing the Prussian alkali, seen the lower part of it take fire soon after its having been turned out of the crucible and bruised, even when the matter had not been covered with sand."

If these experiments of Mr. Bewly be correct, they evidently show that the inflammability of Homberg's pyrophorus depends (at least partly) on the mixture of potassium (and perhaps also alumina) with fine powder of charcoal, and that the common theory, according to which the inflammability of pyrophorus depends on the sulphuret of potash, imbibing moisture from the atmosphere, and by that means setting at liberty heat enough to inflame the charcoal mixed with it, is insufficient. It appears that Mr. Bewly followed nearly the same method in order to make his pyrophorus, as Bucholz, Tromsdorf, and Curaudau, did with a view to obtain potassium; and if Mr. Bewly had happened to introduce into the vessel containing the heated mixture a cold metallic body (as Curaudau did in order to collect the potassium which is formed during the process, and driven off in vapours), he would have seen more than 30 years ago one of those bodies by the discovery of which Sir Humphry Davy's name is now immortalized. As Mr. Bewly's experiment was inconsistent with the then prevailing theory, there was hardly any attention paid to it, though it was published as long ago as the year 1779; at least I do not recollect that it was repeated. I found that an alloy of *potassium with iron* is a pyrophorus also. When I prepared potassium the first time, in the way which Gay-Lussac and Thenard describe, I did not know the property of this alloy; and having cut off that part of the barrel containing the potassium, I began to scrape it out, holding the barrel over a glass bason containing petroleum. I had poured out my whole stock of petroleum, perhaps two pounds, into this bason, that I might be able to immerse into it the whole piece of the barrel, in order not to lose any potassium. After I had scraped out that portion occupying the centre of the barrel, which is the purest, I came to that in immediate contact with the inner surface of the barrel itself, which is generally alloyed with iron; and a small piece of this alloy, being just scraped off, and having therefore its surface quite free from oxide, in falling down took fire, by coming in contact with the air, and lighted the petroleum contained in the glass bason, which cracked, and, the burning fluid flowing over my clothes, placed me rather in a dangerous situation. I afterwards have seen the same effect frequently. I think this pyrophorus of potassium and iron leaves no doubt that there may be one, consisting of potassium and charcoal merely; and this will throw light on the nature of Homberg's pyrophorus.

In order to obtain a potassium free from iron, I several times followed a method which is very simple, and on that account de-

serves perhaps to be known, though it did not yield me quite so large a quantity as the apparatus of Gay-Lussac and Thenard, which, however, does not yield so good potassium, and comes much more expensive. I have made a good deal of potassium, and found, 1. That the chief secret to obtain it in considerable quantity is to use a very strong fire, in order to drive every particle of it over. 2. To prevent the barrel from melting in such a high degree of heat a good tube is wanted. 3. The barrel, notwithstanding these precautions, will generally serve only once. 4. For this reason all the contrivances, where there are different parts combined by grinding them together, or by means of screws, come too expensive, as they are lost after having been used once. I used to make my potassium in old gun-barrels, which were bent (like Gay-Lussac's and Thenard's) in two places, and had no other piece added to them. The middle part of this barrel is placed almost horizontally (only a little elevated towards the open end) through a cylindrical furnace, and contains the iron turnings; the thicker end, which projects on one side from the furnace, is bent upwards, and contains the potash. It can easily be tightened by screwing in the breech, and putting a little clay into the touchhole. To the open end, which projects on the other side from the furnace, and is bent downwards, at first I adapted, as the French chemists did, a receiver made of iron, from which a tube descended into a vessel with quicksilver. I likewise, according to their prescription, screwed into the upper part, containing the potash, an iron tube, the end of which was placed under mercury also; but I found this to be a very troublesome and expensive apparatus, as I could not use it a second time; nor did I always observe the gas coming out through the quicksilver, as it would find its way out somewhere else, either where the receiver was adapted, or where the pipes were screwed in. Finding, therefore, these pipes quite useless, I closed the upper end of the barrel, after the introduction of the potash, entirely by screwing in the breech and putting some clay into the touchhole, and instead of adapting to the lower end any receiver; I put this end, open as it is, into a small iron bason, containing a little vegetable oil previously well heated, in order to drive off the watery particles. (Petroleum could not be used for this purpose, as it would be set on fire by the gas, and even by the heat of the furnace.) When the formation of potassium was going on, one bubble after the other of hydrogen gas holding potassium in solution rose through the oil, and coming in contact with the air took fire, like phosphoric hydrogen gas, but with a more considerable explosion. The purest part of the potassium run down in drops into the oil, in which it rose to the surface, from whence I took it with a spoon, and put it into petroleum. The potassium so obtained did not contain any iron, but was of a very bright lustre, much like silver, whereas that collected in the barrel is generally alloyed with iron, and of a bluish colour, more like lead. Living near an iron-

[illegible]

Fig. 1.

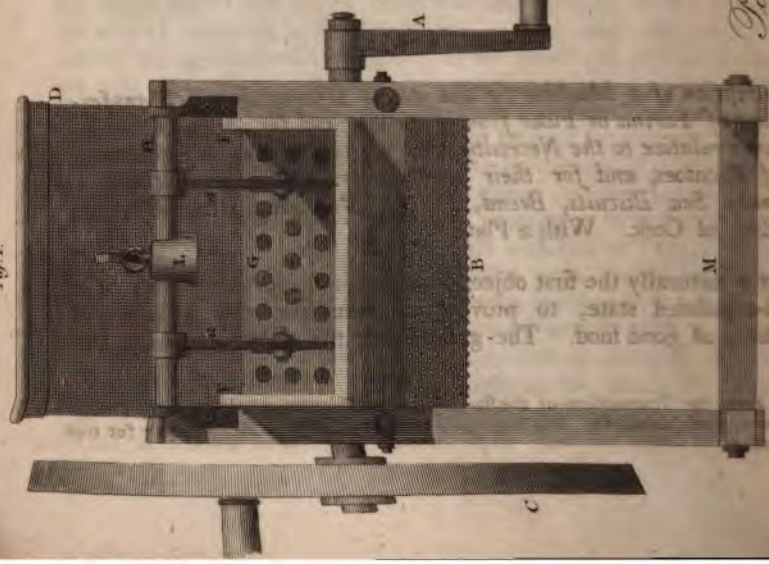
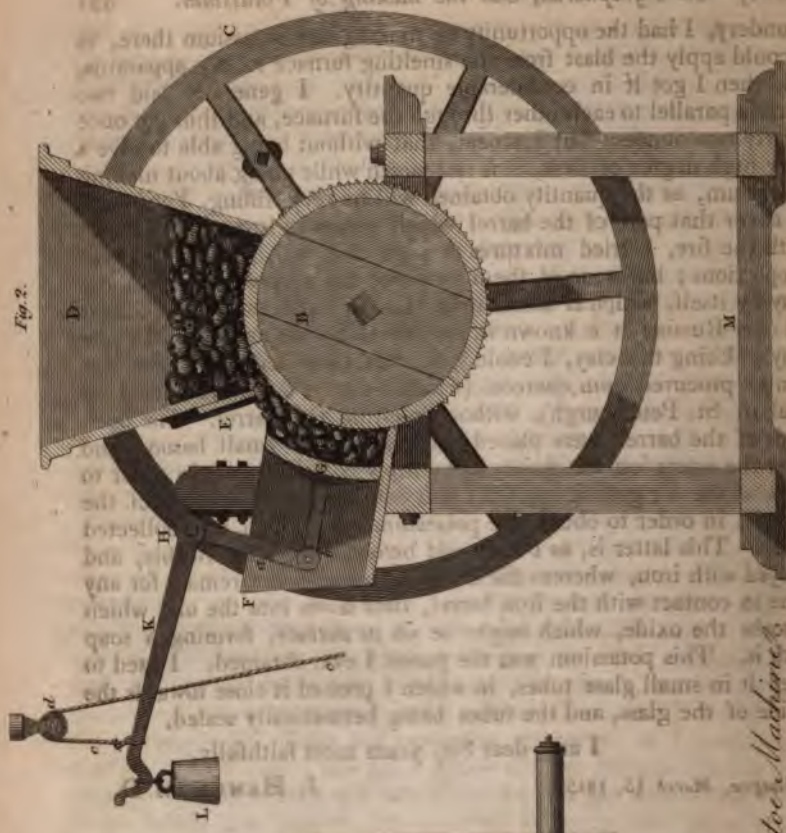


Fig. 2.



Potatoe Machine

foundry, I had the opportunity of making my potassium there, as I could apply the blast from the smelting furnace to my apparatus, and then I got it in considerable quantity. I generally laid two barrels parallel to each other through the furnace, and thus got once nearly two ounces; but I repeat, that without being able to give a very high degree of heat, it is not worth while going about making potassium, as the quantity obtained will be very trifling. For a lute, to cover that part of the barrel which comes in immediate contact with the fire, I tried mixtures of pure clay with sand in different proportions; but none of them answered my purpose so well as the clay by itself, which is found near Moscow, and used for crucibles all over Russia; it is known in commerce by the name of Moscow clay. Using this clay, I could give almost any degree of heat that can be procured from charcoal (which I always used, we having no coal in St. Petersburg), without melting the barrel. The open ends of the barrels were placed under oil in one small bason; and after the operation was finished, I simply corked them, in order to prevent the air getting in, till I had cut off the under parts of the barrels, in order to obtain the potassium also which was collected there. This latter is, as I have said before, always less fusible, and alloyed with iron, whereas the other, which does not remain for any time in contact with the iron barrel, runs down into the oil, which absorbs the oxide, which might be on its surface, forming a soap with it. This potassium was the purest I ever obtained. I used to keep it in small glass tubes, in which I pressed it close towards the inside of the glass, and the tubes being hermetically sealed.

I am, dear Sir, yours most faithfully,

Glasgow, March 15, 1815.

J. HAMEL, M.D..

## ARTICLE VIII.

*Description of a Machine for the speedy Separation and Manufacture of Farina or Flour from Potatoes, and for various Observations relative to the Necessity of encouraging an extended Growth of Potatoes, and for their Applications, in various States, to make Sea Biscuits, Bread, Pastry, &c.* By John Whately, Esq. of Cork. With a Plate.\*

It is naturally the first object, and forms the primary duty of a well-regulated state, to provide its members with a sufficient quantity of good food. The greater are the advantages, and the

\* From the Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce. The lesser gold medal was voted to Mr. Whately for this invention.

more independent is the situation, of that country which can furnish within itself a full and ample supply of the means of subsistence to its inhabitants. We have long ceased in this country to feel the benefits arising from such a state of independence, and we have during the last 10 or 15 years incurred a very large expenditure in the purchase of foreign corn. This subject has engaged much of my attention ; and I have endeavoured to devise a plan by which the agriculture of this country may be improved, a great expenditure saved, and a considerable reduction of prices effected, by the introduction into bread of an article of known nutritious properties derived from the potatoe. This substance is the farina or flour of that valuable root, which I have tried with advantage to a very great extent, having previously gone through a long course of experiments on making bread, biscuit, and pastry, from mixtures of raw potatoes, and boiled potatoes and farina, with different proportions of flour, &c.

It is well known that poor light soils, unfit for the culture of wheat, and from which only crops of barley or oats have been produced, and of which, in many situations, there are large tracts of land altogether uncultivated, will produce potatoes of excellent quality : two circumstances have, however, hitherto prevented their culture to the desirable extent, namely, the great expense of their carriage in a raw state from the interior parts of the country to a proper market, and their not keeping well more than six months after the crop is gathered.

These inconveniences I have obviated by the invention of a machine which is now at the Society's house, by which one individual can grind down 15 hundred weight, or 1700 lbs. net of potatoes, into soft pulp in one day, yielding about two hundred weight of farina or flour when dried.

By calculation, the power of a single-horse will be equal to the grinding of 22 tons of potatoes per week ; the machine is capable of being applied to any power required. The farina or flour has been known to keep good 17 years ; and it may be afforded so as to yield a fair profit to the farmer and the manufacturer at 3d. per lb. The present price of wheaten flour is about 5½d.

It would be tedious now to enter into a long detail of this business by letter ; but my son is in London ; and if he is permitted to attend a Committee of the Society, and to give proofs of the trials I have made, by exhibiting specimens of the bread, flour, and biscuits, and by furnishing such other particulars as may be required, I trust they will perceive the great benefits that may result from this article, and particularly if my communications should merit the sanction of the Society, and be honoured with their approbation.

*Remarks from Mr. Whately on Potatoes.*

*It may not be generally known, but it is capable of the most*

satisfactory proof, that the same quantity of land will yield above one half more of farina or flour where potatoes are cultivated, than if the same land was applied to the production of flour from wheat.

I have proved from experiment, that 2619 lbs. of pure farina or flour may be produced from an acre of land planted with potatoes, and only 1660 lbs. of flour from an acre of wheat. It will therefore be obvious that, if we can apply this great excess to the same purposes as the flour of wheat, the advantages arising from it will be of the highest importance to the community. We now require from foreign countries about 500,000 quarters of wheat annually, for which we incur an expenditure of about two millions sterling, in ordinary years, and which has increased in years of scarcity to upwards of seven millions sterling, and there seems no probability that, under the continuance of the present system of cultivation, we shall ever arrive at the period when we can supply our own wants, and feel independent of foreign aid for one of the first articles of subsistence.

We have had for the last ten years not only a course of favourable seasons, but the farmer has had in the greater part of that period the benefit of high prices to stimulate his exertions to the greatest possible production of corn; and if they have failed, under such favourable circumstances to satisfy our wants, what evil may we not expect, if the seasons should prove unpropitious, and the inconveniences of our situation aggravated by scarcity? I admit that the prospect of supplies from foreign countries may be great; but we should recollect, that only a few months have elapsed since the ports of Europe and America were shut against us, or only open on a system of license, and that in a situation of dependance which no well-wisher to his country can be desirous to see recur; nor the possibility of which he will not feel anxious to prevent. This, then, is one of the advantages which will attend the consumption of potatoe flour in bread; the earth yields it so much more abundantly, that the very first year's cultivation of potatoes for that purpose, to the extent of only 25,000 acres, would relieve us from the necessity of any foreign importation, and instead of importing, we should speedily become an exporting country. Potatoes are found to possess such highly nutritive properties, that it seems hardly necessary to enter upon that subject. They constitute, it is well known, in their natural state, the principal food of the Irish, and they are daily forming a great part of our own. This farina or flour, which contains the substantial nourishing properties of the root, deprived of its watery particles, cannot but be highly nutritious; the use of it, therefore, in bread, is not likely to be the subject of dispute, provided it is in that state palatable, and can be afforded at a price not exceeding the price of wheaten flour. That it possesses the first-mentioned property, I have only to refer to the bread produced to the Society, containing different proportions of this flour, any, and all of which will, I believe, be found as wholesome and agreeable as bread made wholly of wheaten flour.

In some respects it seems an improvement, as it keeps much longer, and it rises and becomes very light and agreeable when toasted. Upon the second question, I shall be able to show satisfactorily that the flour of potatoes can be produced at a cheaper price than the flour of wheat. All objection, therefore, to its use in bread, is not only removed, but I conceive the most important advantages will flow from its adoption. In the specimens before you one-fifth of the farina is the smallest proportion made use of, and two-thirds of farina and boiled potatoes together, when in bread, the largest; calculating, however, that no greater proportion than one-fifth should be generally introduced, the saving of wheaten flour alone, from that circumstance, would be immense. The population of Great Britain consists, according to the latest returns, of upwards of twelve millions and a half, and of this number it may be conjectured that about ten millions consume wheaten bread. Each individual's consumption has been considered equal to a quarter of wheat per annum. The introduction of potatoe flour, therefore, generally, would produce a saving of two millions of quarters of wheat per annum; and instead of being, as we now are, an importing nation, we should, after supplying our present deficiency, have a surplus of about 1,500,000 quarters for exportation, which, at the present permitted export price, under 54s. per quarter, would be equal in value to four millions sterling! That this surplus would not be obtained without some sacrifice of other crops, I am ready to admit; but when the increased productiveness of the potatoe is considered, when it is calculated that large tracts of land may be cultivated upon this system that would not be cultivated upon any other, and that the plan may be generally adopted, which prevails universally in Ireland, and has the recommendation of distinguished agriculturists in this country, of making the potatoe crop precede the crop of wheat, the sacrifices would upon the whole be few, and the advantages immense. I know that objections exist from the scarcity of manure, &c. but I am satisfied that many of them would yield in practice. The poor Irish contrive to manure the land, and to produce crops that serve to form the winter supply of the city of Dublin, in situations distant from cities, and where artificial manure is not to be had; lime, sand, or sea-weed, are all excellent, and are in many places easily procured; the burning of the surface in rough uncultivated land is almost sure to produce a large crop, and is generally one of the first, as it is found one of the best preparatives to cultivation. The manure from hogs has been stated to afford very great returns; and as it is almost essential to the economical pursuit of the business of manufacturing the potatoes into flour that many hogs should be kept, for the purpose of consuming the skins, refuse, &c. it is a manure that would be abundant in those places where the manufacture was introduced. Perhaps no situations would so strongly feel the benefit of this system as those which are remote from sea-ports, and distant from water-carriage. The ground is now necessarily employed in pasture, or it remains waste.

The expense of carriage is an insurmountable bar to the pursuit of agriculture. But let us now consider the situation of a farmer adopting this system. He chooses a remote part of the country, where, perhaps, cultivation did not before exist. He takes the land at probably one-fifth part of the rent which he would pay in more frequented spots, and he either invites the manufacturer of the farina to settle along with him, or he resolves to become a manufacturer himself. The first year he begins by burning and paring the surface of a part of his land, and by this preparation ensures an abundant crop. He sows the next year barley or oats on the land thus cleaned and prepared, which will serve as food for his cattle, and for the large stock of hogs that, as a manufacturer of farina, he must support; and he proceeds to bring a further part of his land under cultivation by the same means. The potatoes are manufactured on the farm; and if he could afford to send the potatoes seven miles to market in their original state, he can, without disadvantage to himself, convey them 50 in their manufactured state, as they are then reduced to about one-seventh or eighth part of their bulk. His hogs supply him with such a considerable quantity of manure, in addition to that of his farm-yard, as will enable him to maintain a constant rotation of potatoe and light corn crops. His expense in machinery is small; and the waggon that carries his flour to market brings back his coals, his groceries, and his household requisites, without additional expense. It would be necessary for him to have a considerable number of labourers, and it would be his interest to attract them around him by allowing them a small portion of land; and by being himself the purchaser of their superfluous produce, he would speedily raise a colony about him; and as the source which gave it life is connected with our subsistence, and the just necessities of our nature coeval with our wants, its prosperity must be ensured so long as we continue in the disposition to supply them. In the general complaint of the scarcity of farms, and the high rents which lands at present bear, perhaps no speculation offers stronger inducements to the young farmer, who can command a moderate capital, and who is contented to forego some of the gratifications of a large society. If he possesses activity and enterprise, and has engaged his land upon the favourable terms upon which it ought to be had at a distance from market, his success in a few years will be certain, provided the system meets with support from the public.

One of its many advantages consists in the incorruptibility of the farina; it is not like the flour of wheat, liable to decay, but it may be preserved for years sound and good, perhaps improved, but certainly uninjured by age. Thus our supply will be at all times rendered more equal, and the year of abundance will more effectually contribute to alleviate the year of scarcity: scarcities themselves are likely to be of less frequent occurrence, as we should have the advantage of two essential crops instead of one; and the weather

injurious to wheat is not unfrequently favourable to the crop of potatoes, and *vice versâ*. I must also observe that the farina forms an excellent ingredient in sea biscuit, in the proportions of two parts of farina to three of wheaten flour, or even of three parts farina to four of wheaten flour. A composition of this sort is more likely to resist the effects of climate, from the incorruptible property already mentioned, than if the biscuits were wholly made of wheaten flour.

Such are some of the more leading advantages likely to be produced by the proposed introduction of the flour of potatoes into bread, and the change of system to which it would give rise. We are now, perhaps, on the eve of another alteration in the Corn Laws, calculated to render the present high prices perpetual. A Committee is sitting upon them, composed of men of landed property; and I have heard that it is in contemplation to prevent the importation of foreign wheat when the price of British wheat is under 96s. per quarter. This, then, is peculiarly the moment to introduce an alteration in the system, which would render such a measure unnecessary, and which, instead of perpetuating high prices, would produce amongst us all the blessings of plenty. They may be summed up in these words:—We shall have a greater regularity, and a certain reduction of price; an immense increase of consumable food; its more equal distribution through years of scarcity and years of plenty;

A consequent diminution of the poor's rates;

An increase of comforts to the poor, and to all classes of society, and a great accession to our resources in every branch of national wealth.

April 29, 1813.

J. WHEATLY.

#### EXPERIMENTS ON BREAD.

##### First Experiment.

No.	Weight.	Materials.	Price.		Amount.		Weight of Bread.		Cost per lb.	Observations.
			l.	s.	d.	lb.	oz.	d.		
1.	2 8	Flour .....	5	1	1	3	6	3		Good bread.
2.	2 0	Flour .....	3	0	10	2	11	3		The same.
3.	1 8	Flour .....	5	0	7					
	1 0	Farina .....	3	0	3					
	2 8			0	10	3	4	3		Raw potatoe flavour.

## First Experiment continued.

No.	Weight.	Materials.	Price.	Amount.	Weight of Bread.	Cost per lb.	Observations.
	lb. oz.		s. d.	s. d.	lb. oz.	s. d.	
4.	2 0	Flour.....	54	0 10 $\frac{1}{2}$			
	1 0	Farina .....	3	0 3			
	3 0			1 1 $\frac{1}{2}$	3 13	3 $\frac{1}{2}$	The same.
5.	1 8	Flour.....	54	0 7 $\frac{1}{2}$			
	2 0	Farina .....	3	0 6			
	2 0	Boiled Potatoes	0 $\frac{1}{2}$	0 1			
	5 8			1 2 $\frac{1}{2}$	5 7	3 $\frac{1}{2}$	Slight savour of the raw potatoes; crust soft.
6.	1 8	Flour.....	54	0 7 $\frac{1}{2}$			
	1 0	Boiled Potatoes	0 $\frac{1}{2}$	0 0 $\frac{1}{2}$			
	2 8			0 8 $\frac{1}{2}$	2 9	3 $\frac{1}{2}$	Well tasted bread; moist.
7.	2 0	Flour.....	54	0 10 $\frac{1}{2}$			
	1 0	Boiled Potatoes	0 $\frac{1}{2}$	0 0 $\frac{1}{2}$			
	3 0			0 11	3 0	3 $\frac{1}{2}$	The same.
8.	1 0	Flour.....	54	0 5 $\frac{1}{2}$			
	1 0	Farina .....	3	0 3			
	1 0	Boiled Potatoes	0 $\frac{1}{2}$	0 0 $\frac{1}{2}$			
	3 0			0 8 $\frac{1}{2}$	3 2	2 $\frac{1}{2}$	Excellent bread: I consider it fully as satisfying as bread made wholly of wheaten flour.
9.	1 0	Flour.....	54	0 5 $\frac{1}{2}$			
	1 0	Boiled Potatoes	0 $\frac{1}{2}$	0 0 $\frac{1}{2}$			
	2 0			0 5 $\frac{1}{2}$	1 13	3 $\frac{1}{2}$	Baked heavy, but very sweet bread; moist.

## Second Experiment.

1.	2 0	Flour.....	54	0 10 $\frac{1}{2}$			
	1 0	Boiled Potatoes	0 $\frac{1}{2}$	0 0 $\frac{1}{2}$			
	3 0			0 11	2 15	3 $\frac{1}{2}$	Baked heavy, but not unpleasant bread.
2.	1 8	Flour.....	54	0 7 $\frac{1}{2}$			
	2 0	Farina .....	3	0 6			
	2 0	Boiled Potatoes	0 $\frac{1}{2}$	0 1			
	5 8			1 2 $\frac{1}{2}$	5 6	3 $\frac{1}{2}$	Baked heavy; indifferent bread.

## Second Experiment continued.

No.	Weight.	Materials.	Price.	Amount.	Weight of Bread.	Cost per lb.	Observations.
	lb. oz.		d. s. d.	lb. oz. d.			
3.	2 0	Flour.....	5½	0 10½			
	2 0	Farina .....	3	0 6			
	2 0	Boiled Potatoes	0½	0 1			
	6 0			1 5½	6 0	2½	Excellent bread; improved by being kept a few days, rather over-baked.
4.	1 8	Flour.....	5½	0 7½			
	1 0	Farina .....	3	0 3			
	1 0	Boiled Potatoes	0½	0 0½			
	3 8			0 11½	3 5	3½	This proportion is perhaps superior to the last, but the bread of both is excellent; improved by being kept a few days.
5.	1 0	Flour.....	5½	0 5½			
	1 0	Farina .....	3	0 3			
	1 8	Boiled Potatoes	0½	0 0½			
	3 8			0 9	3 3	2½	Rather too moist, but very sweet and good, and much improved by keeping.
6.	1 0	Flour.....	5½	0 5½			
	1 0	Rasped Potatoes	0½	0 0½			
	2 0			0 5½	1 12	3½	Light pleasant bread; dark coloured.

## Third Experiment:

1.	2 0	Flour.....	5½	0 10½			
	2 0	Farina .....	3	0 6			
	2 0	Rasped Potatoes	0½	0 1			
	6 0			1 5½	6 2	2½	Heavy; discoloured, made from potatoes rasped 48 hours; flavour not pleasant.
2.	2 0	Flour.....	5½	0 10½			
	2 0	Farina .....	3	0 6			
	2 0	Boiled Potatoes	0½	0 1			
	6 0			1 5½	6 1	2½	Heavy.
3.	1 8	Flour.....	5½	0 7½			
	1 0	Farina .....	3	0 3			
	1 0	Rasped Potatoes	0½	0 0½			
	3 8			0 11½	3 9	3½	Rather heavy; discoloured; the boiled potatoes are evidently a more desirable ingredient than the rasped.

## Third Experiment continued.

No.	Weight.	Materials.	Price.			Amount.		Weight of Bread.		Cost per lb.	Observations.
			d.	s.	d.	lb.	oz.	lb.	oz.		
4.	1 8	Flour.....	5½	0	7½						
	1 0	Farina .....	4	0	3						
	1 0	Boiled Potatoes	0½	0	0½						
	3 8					0	11½	3	5	3½	Good bread; toasts extremely well; indeed all bread made of a proportion of potatoe is remarkably good toasted.
5.	2 0	Flour.....	5½	0	10½						
	1 0	Farina .....	3	0	3						
	1 0	Rasped Potatoes	0½	0	0½						
	4 0					1	2	4	0	3½	Rather heavy; discoloured; flavour not good.
6.	2 0	Flour.....	5½	0	10½						
	1 0	Farina .....	3	0	3						
	1 0	Boiled Potatoes	0½	0	0½						
	4 0					1	2	4	0	3½	Excellent bread; superior in colour to No. 4, and rather more compact; this quantity of wheaten flour alone, as appears by No. 7, would have produced 2 lb. 10 oz. of bread; consequently the actual proportion of the produce of the potatoe in this loaf is 1 lb. 6 oz. or, as nearly as possible, one-third.
7.	2 0	Flour.....	5½	0	10½			2	10	4	Rather dark; the appearance of this loaf indicated that the yeast used for this batch was not good.
8.	2 0	Flour.....	5½	0	10½						
	0 8	Farina .....	3	0	1½						
	2 8					1	0	3	4	3½	Excellent bread; whiter than bread made of wheaten flour alone; no flavour whatever of potatoe; not used until nine days old.
9.	2 0	Flour.....	5½	0	10½						
	0 8	Farina .....	3	0	1½						
	0 8	Rasped Potatoes	0½	0	0½						
	3 0					1	0½	3	7	3½	Not equal to No. 10, an additional proof that rasped potatoes are not desirable.
10.	2 0	Flour.....	5½	0	10½						
	0 8	Farina .....	3	0	1½						
	0 8	Boiled Potatoes	0½	0	0½						
	3 0					1	0½	3	5	3½	Excellent bread; rises by toasting; and was quite fresh at seven days old.

From these experiments it appears that various proportions of potatoes and farina are highly palatable in bread; but if it be calcu-

lated that not more than one-fifth part be generally introduced into consumption, the operation of that fifth is sufficiently great to produce all the effects in view, in recommending its general adoption.

Some kinds of potatoes contain a much greater proportion of farina than others, some yielding a seventh part, and even a still larger proportion of it. Experience will best determine the most desirable sort for use, as other kinds produce more abundantly in number; all these considerations should be attended to.

Potatoes may be manufactured into farina during the greater part of the year, but they lose a portion of their farinaceous matter when vegetation begins; the manufacturer, therefore, should provide an early sort to commence his operations upon in August, and cease to grind any after the months of April and May.

Frost-bitten potatoes yield nearly as good farina as the others, provided they are ground before they begin to decay; and potatoes kept over for a whole season, although of inferior value, may by this process be still rendered serviceable food for mankind. These are advantages which ought not to escape notice. Frost-bitten potatoes have hitherto become in a short time a total loss, and farmers have generally been afraid of extending their cultivation of potatoes, for fear of their being spoiled by keeping, both which inconveniences are avoided by preparing the farina from them.

Amongst the many important uses to which the farina may be applied, that of biscuit for sea store is of great consequence. Biscuits made from one part farina and two parts of wheaten flour are whiter and better than those made wholly of common flour. Baked biscuits of these proportions have been repeatedly made with uniform success; and as the farina is in itself less corruptible than wheaten flour, it is likely to produce a biscuit of a less perishable kind. Some have been already kept nearly twelve months without exhibiting any symptoms of moisture or decay; on the contrary, they were as hard and as good as on the day they were baked. Biscuit differs from leavened bread in this circumstance, that all moisture is evaporated from biscuits in baking, whilst it constitutes an essential quality in bread; the weight of bread is greater, the weight of biscuit is less, than the flour of which it is composed. It is, therefore, not inconsistent with the nature of farina that it should exhibit more moisture in bread, and yet discover no symptom of it in biscuit; and it has one convenient property, of which a baker who manufactures it may avail himself, that it may be used for bread or biscuit either in a wet or dry state; consequently the expense of drying it may in some cases be avoided. Its introduction into biscuit would produce a saving of one-third of the wheaten flour now used for that purpose, which will be found very great, when the extensive use of biscuit in the navy and army is considered. The farina of potatoes should constitute, from its general application, a part of the stores of each.

In the navy particularly, the farina would afford a great variety of food, as it forms an excellent ingredient in soup as a substitute for

peas, oatmeal, and rice, and possesses the superior advantage over those substances of not being susceptible of injury. One ounce of farina will thicken sufficiently nearly one quart of water. And when this soup is flavoured with onions, celery, leeks, &c. and seasoned with pepper and salt, it forms a very palatable mixture without the aid of meat. The produce of 21 lbs. of raw potatoes made into farina, and soup formed from it, will furnish a wholesome meal to 28 persons, allowing each person a full quart, which potatoes, in their original state, would scarcely furnish a dinner to one-third of that number.

The farina of potatoes will form a thick mucilage with 26 times its weight of water.

In all cases where the farina is intended to be converted into a gelatinous state, the farina should be previously mixed with some portion of the liquid cold.

A dessert spoonful of the farina mixed with a little cold milk and salt, and added to a pint of boiling milk, and kept stirring and simmering, will form an excellent nutriment.

From various circumstances, it appears that the article called *Semolina* in the shops, and recommended as a nutritious diet for children and sick persons, is wholly composed of the farina of potatoes.

#### *Certificates.*

Mr. Whately, of Cork, has shown me a specimen of flour made from potatoes; and also of biscuit, made partly from this flour, and partly from the flour of wheat. I am convinced that this flour is very wholesome and very nourishing, and may be of great use to the public in supplying the deficiency of wheaten flour.

*Lower Grosvenor-street,  
Dec. 11, 1812.*

M. BAILLIE, M. D.

I have examined the flour from potatoes made by Mr. John Whately, and have no doubt of its containing all the nutritious part of the potatoe, and therefore calculated to form a very good and valuable diet; and as the flour thus produced may be readily transported to any distance, and may be kept for a very long time without losing any of its nutritive properties, I think the conversion of potatoes into flour may prove highly advantageous to the community.

*Soho-square, Jan. 17, 1813.*

GEO. L. TUTHILL.

#### *Reference to the Engravings.*

This machine is of a very simple construction, its moving part consisting of a cylinder covered with tin plates, pierced with holes, so as to leave a rough surface, in the same manner as the graters used for nutmegs, &c. but the holes in this are larger. This cylinder is situated beneath a hopper, into which the potatoes are

thrown, and thence admitted into a kind of trough, where they are forced against the cylinder, which as it revolves grinds the potatoes to a pulp.

Fig. 1, Plate XXXIV., represents the machine in front; or lengthways of the cylinder; and fig. 2 is a section through the middle of it, showing also the hopper with its contents, and the manner of the action of the machine. A represents the handle, by which motion is given to the machine: it is fixed upon the end of the axis of the grating cylinder B; on the opposite extremity of this axis is a fly-wheel C, to regulate and equalize the movement. D is the hopper, into which the potatoes are put; and pressing by their weight upon the top of the cylinder as it revolves, they are in part grated away. On one side of the lower part of the hopper is an opening, closed or opened more or less at pleasure by a slider E; and the degree of opening which this has regulates the passage of the potatoes from the hopper D into the trough F: this is as wide as the length of the cylinder, and has a concave board G fitted into it, which slides backwards and forwards, by the action of levers *a a* affixed to an axis H, extended across the frame of the machine. K is a lever fixed upon the middle of this axis, and terminating in a hook at the end for the suspension of a weight L; this acts upon the board G by means of the levers K and *a a* and the rods *b*, to force or press the potatoes contained in the trough forwards against the cylinder, and complete the grating of them into a pulp.

A line *c* is tied to the end of the lever K, and passing over a pulley *d*, hangs down within reach of the person who turns the handle of the machine. By drawing this line, the weight at the end of the lever K is raised up, and by the rods *b* the board G is withdrawn to the extremity of the trough, and a fresh stock of potatoes falls out at the opening E from the hopper into the trough; then, the line being let go, the weight L presses the board against the potatoes, and forces them against the cylinder, which by its motion grates them away very rapidly, the pulp passing down the space between the edge of the lower board of the trough F, and the cylinder, which space is only a narrow crevice that nothing may pass down but the reduced pulp, which falls into a box or vessel situated between the frame at M. The board G is perforated with a number of large circular holes, to make an uneven surface, in which the potatoes remain steady, whilst exposed to the action of the cylinder upon their opposite sides.

The tin plate covering the cylinder is of course pierced from the inside outwards, and the bur or rough edge left round each forms an excellent rasping surface, for the reduction of such substances as the present. The frame of the machine requires but little explanation, merely consisting of a square frame, containing the cylinder, and supporting the hopper. This frame stands on four legs, two of which rise up to a sufficient height to carry the pivot of the axis H. The legs are morticed into four ground cells, which may be made

to receive a box or chest, fitted in the manner of a drawer, to contain the pulp as it falls from the machine, or it may fall into any vessel placed underneath.

The pulp in the vessels in which it is collected should be completely immersed in water, and well stirred; the separation of the farina, by its falling to the bottom, will speedily take place. The fibrous and refuse parts should be first removed, and the farina afterwards repeatedly washed, until it no longer communicates any tinge to the water. It is then sent to the stove or drying apartment, put into boxes, and dried. Care should, however, be taken that it does not dry too rapidly. Wherever the business is conducted upon a large scale, a machine for washing the potatoes\* should be erected, and it might be contrived to receive motion from the power that directs the grinding machine. The vessels containing the pulp should be so arranged that a stream of water could be made to pass through them at pleasure. The pulp, after the first separation of the farina, still retains a quantity of farinaceous matter; but it will, perhaps, be more profitable to convert it into food for hogs, with such other additions as may be thought necessary, than to steep it again for the purpose of extracting any remaining portion of farina. It will be probably best to give it to the hogs boiled, along with a moderate proportion of boiled potatoes. This disposal of the refuse pulp would nearly defray the whole expense attendant upon the manufacture of the farina.

\* \* \* This machine would operate with good effect for reducing apples to a pulp for making cider, as it is extremely expeditious and effectual in its operation.

## ARTICLE IX.

### *Astronomical and Magnetical Observations at Hackney Wick.* By Col. Beaufoy.

Latitude,  $51^{\circ} 32' 40.3''$  North. Longitude West in Time  $0^{\text{h}} 1^{\text{m}} 0.0^{\text{s}}$ .

Mar. 21, Immersion of Jupiter's	12 <sup>h</sup> 24'	02''	Mean Time at Hackney Wick.
2d Satellite .....	12	24	08.8 Ditto at Greenwich.
Mar. 25, Immersion of Jupiter's	19	24	18.6 Ditto at Hackney Wick.
1st Satellite .....	12	24	25.4 Ditto at Greenwich.
April 3, Emersion of Jupiter's	10	57	56 Ditto at Hackney Wick.
1st Satellite .....	10	58	02.2 Ditto at Greenwich.
April 8, Emersion of Jupiter's	9	24	28.4 Ditto at Hackney Wick.
2d Satellite .....	9	24	25.2 Ditto at Greenwich.
April 12, Emersion of Jupiter's	11	31	35.2 Ditto at Hackney Wick.
4th Satellite .....	11	31	45 Ditto at Greenwich.
April 15, Emersion of Jupiter's	12	00	27 Ditto at Hackney Wick.
2d Satellite .....	12	00	33.8 Ditto at Greenwich.

\* Models of two machines proper for washing the earth from potatoes may be seen in the Society's repository.

*Magnetical Observations.*

1815.

Month.	Morning Observ.					Noon Observ.					Evening Observ.				
	Hour.	Variation.				Hour.	Variation.				Hour.	Variation.			
Mar. 18	8h 40'	24°	15'	24"		1h 40'	24°	25'	16"		5h 50'	24°	14'	25"	
Ditto 19	8 40	24	20	48		1 25	24	23	48		5 55	24	18	04	
Ditto 20	8 30	24	14	43		1 35	24	26	13		5 55	24	16	22	
Ditto 21	8 40	24	14	23		1 00	24	22	45		—	—	—	—	
Ditto 22	8 40	24	14	53		1 40	24	24	07		5 55	24	15	23	
Ditto 23	8 20	24	13	29		—	—	—	—		5 55	24	16	13	
Ditto 24	8 15	24	12	48		1 40	24	27	33		5 55	24	17	48	
Ditto 25	8 15	24	14	19		1 25	24	25	38		5 55	24	17	31	
Ditto 26	8 20	24	13	45		—	—	—	—		—	—	—	—	
Ditto 27	8 40	24	13	33		1 20	24	24	28		—	—	—	—	
Ditto 28	8 40	24	14	39		1 10	24	25	08		5 50	24	15	33	
Ditto 29	8 25	24	14	01		1 30	24	24	19		6 00	24	19	10	

1815.

Mean of Observations in March.	Morning	at 8h 32'.....	Variation	24° 14' 49"	West.
	Noon	at 1 27.....	Ditto	24 23 40	
	Evening	at 5 54.....	Ditto	24 16 43	
Ditto in Feb.	Morning	at 8 37.....	Ditto	24 15 11	West.
	Noon	at 1 31.....	Ditto	24 21 51	
	Evening	at —.....	Ditto	— — —	
Ditto in Jan.	Morning	at 8 47.....	Ditto	24 16 46	West.
	Noon	at 1 36.....	Ditto	24 20 12	
	Evening	at —.....	Ditto	— — —	
1814. Ditto in Dec.	Morning	at 8 44.....	Ditto	24 18 03	West.
	Noon	at 1 30.....	Ditto	24 20 36	
	Evening	at —.....	Ditto	— — —	
Ditto in Nov.	Morning	at 8 41.....	Ditto	24 16 20	West.
	Noon	at 1 40.....	Ditto	24 20 37	
	Evening	at —.....	Ditto	— — —	
Ditto in Oct.	Morning	at 8 39.....	Ditto	24 14 08	West.
	Noon	at 1 42.....	Ditto	24 21 45	
	Evening	at —.....	Ditto	— — —	
Ditto in Sept.	Morning	at 8 32.....	Ditto	24 14 33	West.
	Noon	at 1 39.....	Ditto	24 23 17	
	Evening	at 6 19.....	Ditto	24 16 50	
Ditto in Aug.	Morning	at 8 30.....	Ditto	24 14 13	West.
	Noon	at 1 39.....	Ditto	24 23 48	
	Evening	at 6 57.....	Ditto	24 16 31	
Ditto in July.	Morning	at 8 41.....	Ditto	24 13 29	West.
	Noon	at 1 42.....	Ditto	24 23 44	
	Evening	at 6 58.....	Ditto	24 17 00	
Ditto in June.	Morning	at 8 44.....	Ditto	24 13 10	West.
	Noon	at 1 39.....	Ditto	24 22 48	
	Evening	at 6 52.....	Ditto	24 16 39	
Ditto in May.	Morning	at 8 45.....	Ditto	24 13 12	West.
	Noon	at 1 44.....	Ditto	24 22 13	
	Evening	at 6 38.....	Ditto	24 16 14	
Ditto in April.	Morning	at 8 45.....	Ditto	24 12 53	West.
	Noon	at 1 48.....	Ditto	24 23 53	
	Evening	at 6 29.....	Ditto	24 15 30	
Ditto in March.	Morning	at 8 52.....	Ditto	24 14 29	West.
	Noon	at 1 52.....	Ditto	24 23 08	
	Evening	at 6 11.....	Ditto	24 16 33	

1814.		at	8	47'....	Variation	24°	14'	50"	
Mean of Observations in Feb.	Morning	at	1	52'....	Ditto	24	20	58	} West.
	Noon	at	—	.....	Ditto	—	—	—	
	Evening	at	—	.....	Ditto	—	—	—	} Not obs.
Ditto in Jan.	Morning	at	8	52'....	Ditto	24	15	05	} West.
	Noon	at	1	53'....	Ditto	24	19	03	
	Evening	at	—	.....	Ditto	—	—	—	} Not obs.
1813.	Morning	at	8	53'....	Ditto	24	17	32	} West.
	Noon	at	1	51'....	Ditto	24	20	30	
	Evening	at	—	.....	Ditto	—	—	—	} Not obs.
Ditto in Dec.	Morning	at	8	42'....	Ditto	24	17	17	} West.
	Noon	at	1	54'....	Ditto	24	20	24	
	Evening	at	—	.....	Ditto	—	—	—	} Not obs.
Ditto in Nov.	Morning	at	8	45'....	Ditto	24	15	41	} West.
	Noon	at	1	59'....	Ditto	24	22	53	
	Evening	at	—	.....	Ditto	—	—	—	} Not obs.
Ditto in Oct.	Morning	at	8	53'....	Ditto	24	15	46	} West.
	Noon	at	2	02'....	Ditto	24	22	32	
	Evening	at	6	03'....	Ditto	24	16	04	} Not obs.
Ditto in Sept.	Morning	at	8	44'....	Ditto	24	15	55	} West.
	Noon	at	2	02'....	Ditto	24	23	32	
	Evening	at	7	05'....	Ditto	24	16	08	} Not obs.
Ditto in Aug.	Morning	at	8	37'....	Ditto	24	14	33	} West.
	Noon	at	1	50'....	Ditto	24	23	04	
	Evening	at	7	08'....	Ditto	24	16	43	} Not obs.
Ditto in July.	Morning	at	8	30'....	Ditto	24	12	55	} West.
	Noon	at	1	33'....	Ditto	24	22	17	
	Evening	at	7	04'....	Ditto	24	16	04	} Not obs.
Ditto in June.	Morning	at	8	22'....	Ditto	24	12	09	} West.
	Noon	at	1	37'....	Ditto	24	20	54	
	Evening	at	6	14'....	Ditto	24	13	47	} Not obs.
Ditto in May.	Morning	at	8	31'....	Ditto	24	09	16	} West.
	Noon	at	0	59'....	Ditto	24	21	12	
	Evening	at	5	46'....	Ditto	24	15	25	} Not obs.

*Magnetical Observations continued.*

· 1815.

Month.	Morning Observ.						Noon Observ.						Evening Observ.					
	Hour.			Variation.			Hour.			Variation.			Hour.			Variation.		
April 1	8 <sup>h</sup>	45'	24"	14'	17"		1 <sup>h</sup>	40'	24"	22'	30"		6 <sup>h</sup>	15'	24"	15'	13"	
Ditto 2	8	20	24	13	28		1	25	24	25	39		6	10	24	17	24	
Ditto 3	8	25	24	18	34		1	10	24	29	36		6	15	24	17	48	
Ditto 4	8	20	24	13	54		1	05	24	25	38		6	20	24	15	40	
Ditto 5	8	40	24	13	45		1	20	24	25	43		—	—	—	—	—	
Ditto 6	8	50	24	13	10		1	10	24	25	50		—	—	—	—	—	
Ditto 7	8	40	24	10	40		1	15	24	27	47		—	—	—	—	—	
Ditto 8	8	20	24	12	19		1	45	24	27	19		—	—	—	—	—	
Ditto 9	8	20	24	13	09		1	40	24	28	51		6	30	24	16	37	
Ditto 10	—	—	—	—	—		1	40	24	28	31		6	20	24	18	12	
Ditto 11	8	35	24	16	19		—	—	—	—	—		6	40	24	15	44	
Ditto 12	8	15	24	18	24		—	—	—	—	—		6	45	24	18	20	
Ditto 13	8	50	24	13	07		—	—	—	—	—		6	20	24	13	14	
Ditto 14	8	35	24	24	57		1	00	24	31	07		—	—	—	—	—	
Ditto 15	8	35	24	15	17		1	35	24	30	13		6	25	24	15	44	
Ditto 16	8	35	24	19	08		1	25	24	31	14		6	35	24	17	51	
Ditto 17	8	30	24	17	01		—	—	—	—	—		—	—	—	—	—	

*Comparison of the Monthly Variations in the Years 1813, 1814, and 1815.*

		1813.	1814 and 1815.	Difference.
April	Morning .....	24° 09 18'	24° 12 53"	3' 55"
	Noon .....	24 21 12	24 23 33	2 41
	Evening .....	24 15 25	24 15 30	0 05
May	Morning .....	24 12 02	24 13 12	1 10
	Noon .....	24 20 54	24 22 13	1 19
	Evening .....	24 13 47	24 16 14	2 27
June	Morning .....	24 12 35	24 13 10	0 35
	Noon .....	24 22 17	24 22 48	0 31
	Evening .....	24 16 04	24 16 29	0 25
July	Morning .....	24 14 32	24 13 29	1 03
	Noon .....	24 23 04	24 23 44	0 40
	Evening .....	24 16 43	24 17 00	0 17
Aug.	Morning .....	24 15 55	24 14 13	1 42
	Noon .....	24 23 32	24 23 48	0 16
	Evening .....	24 16 06	24 16 31	0 23
Sept.	Morning .....	24 15 46	24 14 33	1 13
	Noon .....	24 22 32	24 23 17	0 45
	Evening .....	24 16 04	24 16 50	0 46
Oct.	Morning .....	24 15 41	24 14 08	1 33
	Noon .....	24 22 53	24 21 45	1 08
	Evening .....	— — —	— — —	— — —
Nov.	Morning .....	24 17 17	24 16 20	0 57
	Noon .....	24 20 24	24 20 37	0 13
	Evening .....	— — —	— — —	— — —
Dec.	Morning .....	24 17 39	24 18 02	0 23
	Noon .....	24 20 30	24 20 36	0 06
	Evening .....	— — —	— — —	— — —
Jan.	Morning .....	24 15 05	24 16 46	1 41
	Noon .....	24 19 03	24 20 12	1 09
	Evening .....	— — —	— — —	— — —
Feb.	Morning .....	24 14 50	24 15 11	0 21
	Noon .....	24 20 58	24 21 51	0 53
	Evening .....	— — —	— — —	— — —
March	Morning .....	24 14 29	24 14 49	0 20
	Noon .....	24 23 08	24 23 40	0 32
	Evening .....	24 15 33	24 16 43	1 10

In deducing the mean of the observations, the morning observation of March 19, and the noon of March 24, are rejected, the variation being so great. The wind on both days was from the west, and the weather cloudy.

*March 31.*—Immersion uncertain to 10".

Rain fallen { Between noon of the 1st Mar. } 1.528 inch.  
 { Between noon of the 1st Apr. }

## Comparison of the Yearly Variation.

	First 12 months observ.			Second 12 months observ.		
	Morning.	Noon.	Evening.	Morning.	Noon.	Evening.
April .....	24° 09' 18"	24° 21' 12"	24° 15' 25"	24° 19' 53"	24° 23' 53"	24° 15' 30"
May .....	24 12 02	24 20 54	24 13 47	24 12 49	24 22 13	24 16 14
June .....	24 12 35	24 22 17	24 16 04	24 13 10	24 22 48	24 16 29
July .....	24 14 32	24 23 04	24 16 43	24 13 29	24 23 44	24 17 00
August .....	23 15 55	24 23 32	24 16 06	24 14 30	24 23 48	24 16 31
September .....	24 15 46	24 22 32	24 16 04	24 14 33	24 23 17	24 16 50
October .....	24 15 41	24 22 53	— — —	24 14 08	24 21 45	— — —
November .....	24 17 17	24 20 54	— — —	24 16 20	24 20 37	— — —
December .....	24 17 39	24 20 30	— — —	24 18 02	24 20 36	— — —
January .....	24 15 05	24 19 03	— — —	24 16 46	24 20 12	— — —
February .....	24 14 50	24 20 58	— — —	24 15 11	24 21 51	— — —
March .....	24 14 29	24 23 08	24 15 33	24 14 49	24 23 40	24 16 43
Mean .....	24 14 36	24 21 42	24 15 41	24 14 42	24 22 22	24 16 28
				24 14 36	24 21 42	24 15 41
Difference .....				+ 6	+ 40	+ 47
Mean of the three differences..... + 81"						

(To Dr. Thomson.)

MY DEAR SIR,

Hackney Wick, April 17, 1815.

I HAVE the pleasure to send you the conclusion of the second year's observations on the variation of the magnetic needle. The number of observations made in the first year, and set down in the *Annals of Philosophy*, were, in the morning 294, at noon 265, and in the evening 141. But it is to be remarked that the numbers set down are the means of the observations made with two needles, and each consisting of 14 readings off on the arc of the instrument. If, therefore, the former numbers be multiplied by 28, the total amount will be 19600, and the number in the second twelve months will be found 22764. As every attention was paid in making the observations, and the two needles rarely differed three minutes, seldom two, it may be presumed that the variation is truly determined, and has not yet arrived at its maximum, the annual increase being 31".

It certainly would be a desirable circumstance if observations were made, at this period, in different parts of the world, especially in those places where the variation is great: and as I perceive in Number xxv. of the *Annals of Philosophy*, that Mr. Scoresby, jun. of Whitby, has kept meteorological journals during his voyages to Davis's Straits in the years 1813 and 1814, perhaps he, or some other Gentleman, may favour your readers with observations on the

variation of the compass in that part of the world. That a comparison may be made of the present variation with that in the year 1790, I have inserted Capt. Brown's observations, on whose accuracy dependance may be placed. His journals are in my possession. He commanded the ship *Butterworth*; and was furnished with an accurate azimuth compass, made by Mr. Dolland for the purpose of making the observations now inserted.

*Captain Brown's Observations.*

Variation.	Lat. Obs.	Longitude.	By Sun and Moon.
79° 42' W	72° 46' N	—° —'	
79 00 W	72 05 N	— —	
78 15 W	71 26 N	— —	
73 44 W	— —	— —	
74 00 W	70 58 N	54 14 W	
73 40 W	70 55 N	— —	
72 00 W	70 05 N	— —	
71 00 W	66 59 N	57 04 W	
70 40 W	65 44 N	59 31 W	
70 00 W	63 40 N	59 22 W	
68 00 W	63 34 N	58 33 W	

I am not acquainted with the alteration in the variation which has taken place on the Continent. From several observations I made in the year 1787, at La Rochette, near Neuchatel, in Switzerland, the variation was 19° 7' 14" W. The latitude of this place, from many observations, is 47° 00' 15" N.; and the longitude, deduced during my residence there from solar eclipses and occultations of the stars by the moon, 28° 40" E. in time, or 7° 10' 00" in space, of Greenwich.

Not being aware that any observations have been made to determine the situation of the supposed highest mountain in Europe, I take the liberty of sending you the latitude of Monte Blanc, from an observation I made under very favourable circumstances, on its summit, in the month of August, 1787; the latitude, deduced from the meridional altitude of the sun, is 45° 49' 59" N.; and as the summit bears from Neuchatel by the compass S. 20° 54' 07" W. by using the difference of latitude and the true bearing, the longitude in space is 3° 10" W. of Neuchatel, and consequently 7° 6' 50" E. from Greenwich. The lake of Neuchatel I found to be 1428 feet above the English Channel.

I remain, my dear Sir, yours very sincerely,

MARK BRAUFOY.

## ARTICLE X.

*Observations on the Uses of the Dorsal Vessel, or on the Influence which the Heart exercises in the Organization of articulated Animals, and on the Changes which that Organization experiences when the Heart or the Organ of Circulation ceases to exist.* By M. Marcel de Serres.

(Continued from p. 199.)

THIS last series of researches being finished, it remained to determine the cause of the contractions and dilatations of the dorsal vessel, and of what importance that vessel was to the general economy of insects. These contractions and dilatations might depend on various causes, or only upon the peculiar structure of that organ; though it was difficult to adopt such an opinion, because the absence of vascular vessels announced that the humour contained in it did not circulate. To determine the point I began by examining what influence the muscles which surround the dorsal vessel might have upon its contractions.

The mole cricket, being a very lively insect, was chiefly employed by me in these researches. The dorsal vessel being brought in view, I removed the abdominal muscles (and the adipose membranes of necessity) in the middle part of the abdomen, while I left the dorsal muscles quite entire in the upper and lower part of the abdomen. This being done, I remarked that the dorsal vessel always contracted in those parts where the muscles had not been removed, and these contractions were the less lively the nearer they came to the muscles that had been removed. However, by degrees, some contractions took place in the parts of the dorsal vessel from which the muscles had been removed; but they were always weak, and seemed to proceed from portions in which the muscles still existed.

In other cases I removed entirely the dorsal muscles; then the contractions of the dorsal vessel became weaker by degrees, and ceased at last, though after a very considerable interval. Dr. Encontre\* was so good, at my request, as to repeat these experiments. He observed that, after removing the dorsal muscles, the contractions of the dorsal vessel diminished by degrees in the rings from which these muscles had been removed; while they continued still strong in those rings where the muscles still remained. In a ring in which he had left only some traces of muscle, the dorsal vessel still continued to contract, when its contractions had ceased in all the parts from which the muscles had been removed. I must remark, that the insects subjected to these experiments are not dead, when all the dorsal vessels have been removed; but what is still

\* He is the son of M. Daniel Encontre, known with éclat as a mathematician.

more singular, they continue to live after the dorsal vessel has been entirely removed. I have seen the caterpillar of a sphinx atropos breathe for six hours after I had removed the dorsal vessel. The inspirations and expirations still continued, and air bubbles broke from the ends of the tracheæ which had been cut at the removal of the dorsal vessel. If this organ were a heart, how could insects possibly live so long after being deprived of it. To be convinced of the difference, let the heart be removed from those animals in which it exists even in the least complicated state. Not one will resist its loss, most of them will be dead before the removal can be completed. I must observe, that after the removal of the dorsal vessel, insects are no longer able to move about; but life notwithstanding still continues.

This intensity of life is found in all animals that have no centre of action: other animals die very speedily. One would think there ought not to be a great difference in this respect between the *fau-cheurs* and spiders, or between the scolopendras and scorpions, and yet there is a very great one. The scorpions and spiders die almost immediately after exposing their heart; while insects live often six or seven hours after removing the organs most essential to life. Yet both have a nervous system composed of a series of ganglions; but the insects have no centre of circulation, or, which is the same thing, they have no heart.

We have seen the influence of the dorsal muscles on the contractions of the dorsal vessel. As this influence might not be the only one, I endeavoured to determine those which might belong to the tracheæ and nerves. Before engaging in this new set of experiments, I wished to determine whether by means of metallic excitors I could render the contractions of the dorsal vessel more frequent and stronger. When the two excitors were brought into contact the insect exhibited symptoms of pain; but the dorsal vessel, instead of increasing in liveliness, beat always more and more slowly. On continuing the contact of the wires of zinc and copper between the membranes of the dorsal vessel, the humour contained in this vessel gradually coagulated, and the contractions ceased by degrees. Thus the galvanic agent in the present case produces effects similar to the chemical, with this difference, that it acts less rapidly.

It might be concluded, *à priori*, that the tracheæ have a considerable influence on the contractions of the dorsal vessel, on account of the great number of them which enter it and constitute one of its coats, and on account of the great influence of air on the organs of insects. But great difficulties prevent us from determining that influence; the most insurmountable of which is the impossibility of separating the tracheæ from the dorsal vessel: for, without such a separation, we cannot judge of the influence which they exert on its contractions. I have not been able to surmount these difficulties in those insects which have tributary tracheæ. So that I can only depend upon one experiment, which succeeded

very well on a *caleoptera lamellicornis*. I removed with all possible care, in the *ateuchus semi-punctatus*, the vesicular tracheæ which surround the dorsal vessel, as well as their ramifications. When they were removed the contractions of the vessel diminished by degrees. I had indeed removed some muscular fibres; but in so small a quantity, that I do not believe that cause had a remarkable influence. These contractions have continually gone on diminishing, and the diminution was much greater than what is the consequence of pain, and of the cessation of certain functions. I believe, therefore, that the contractions of the dorsal vessel are proportional to the quantity of adipose tissue; the energy of the muscular fibres that surround it, and of the number of tracheæ which enter into it, or of the air which it receives.

Let us see now whether the nerves have not some influence on these contractions, as the dorsal vessel receives a certain number of them. To determine this influence, I removed the spinal marrow with the nerves coming from it, as far as I could distinguish them, and then examining the contractions of the dorsal vessel, I could not perceive that they were sensibly enfeebled. I repeated this experiment on a great number of individuals of different orders, and I always obtained the same result. Hence I conclude, that the nerves have no very sensible influence on the contractions of the dorsal vessel. It is possible, that in all these animals in which the nervous system is very much divided, and the principal centre has little preponderance, the nervous influence is less distinct. This I shall endeavour to determine by future researches.

It remained still to determine whether the contraction of the dorsal vessel does not depend in part on the circulation of the liquid which it contains. We have already observed, and the observation, since the time of Malpighi has been renewed by all anatomists, that the humour of the dorsal vessel has a very irregular motion, and that the fluid often appears to go from the head towards the tail, and at other times to take a contrary direction. Sometimes the dorsal vessel contracts at its two extremities, while these contractions do not take place in the middle of the organ, especially if the dorsal muscles have been removed in this part. So that the liquid in the dorsal vessel appears at one time to move with extreme rapidity, at another very slowly, without any apparent connexion between this irregularity and the state of the insect. But how can this irregular motion be reconciled with the circulation of a fluid analogous to the blood, and how can a circulation take place without vessels? All these facts are in themselves so conclusive, that it is difficult to consider the dorsal vessel as a heart, and, of course, to admit that the contractions are produced by a liquid in circulation. In the second place, if we pierce the heart of any animal whatever, the moment the blood arrives it rushes out in a quantity proportional to that which enters the heart. It was important, therefore, to determine whether this would happen when the dorsal vessel of insects is punctured. For that purpose I chose

several very lively individuals. I pierced the dorsal vessel in different parts at the time when the liquid had flown back, but I never perceived the least moisture issue out of it. As it is difficult to determine this point in a satisfactory manner, I repeated the experiments with all the precaution possible. The results were constantly the same: no liquid flowed out. If this were the only proof that could be given of the non-circulation of the humour contained in the dorsal vessel, it would not be of much importance; but, joined to those that we have already made known, it acquires a certain force, and even a considerable weight. But it may be said, that the reason why the humour does not exude when the dorsal vessel is punctured, is because it is too thick. I am very much inclined, I own, to that opinion; for this humour has always appeared to me very little liquid, and in the voracious larvæ it has a remarkable consistence. This consistence, joined to some other particularities, has put us on the way to determine with some precision the functions of the dorsal vessel in those animals that have no other circulation than that of air. The different movements of contraction and dilatation which we remark in the dorsal vessel, can never induce us to consider that organ as a heart; since, in the animals like the Naiades (*Nereis*, Lin.) in which there exists only a single organ, that of digestion, we observe pulsations as distinct as those exhibited by the heart of other animals. Therefore the pulsations of the dorsal vessel ought not to make us decide in any manner on the use of that organ, nor lead us to consider it as a heart.

In all animals which have a system of circulation and respiration, the influence of the one of these on the other has been perceived.\* Thus often when the respiration is entire, the circulation is only half so, or when the circulation is complete, the respiration does not operate in a complete manner; so that a demicirculation, multiplied by a complete respiration, or *vice versâ*, gives always equal products; that is to say, a semioxygenation of the blood. But in mammiferous animals, in which the circulation and respiration are complete, the oxygenation of the blood is equally so; and in birds which have a complete circulation with a double respiration, the oxygenation of the blood is doubled, in consequence of the quantity of air that combines continually with it.†

If the dorsal vessel of insects is an organ of circulation, it ought to experience the influence of the organs of respiration like the heart of animals with vertebræ. This influence ought to be so much the greater, as insects, like birds, have a double respiration. The air penetrates into all the parts of their bodies, as it does in birds. It continually bathes their nutritive humour, or their blood, which has this particularity, that it is not confined in vessels; just as in birds it acts upon the blood in the great circulation, at the

\* See Cuvier's *Anatomie Comparée*, t. iv. p. 167.

† These speculations about the oxygenation of the blood are quite vague and uncertain. T.

same time that the small circulation experiences the action of the air in the lungs. Some insects have even peculiar reservoirs of air in their bodies, reservoirs so numerous, that those which are provided with them ought to have a respiration more than double. These reservoirs of air, called *vesicular tracheæ*; but which may likewise be called *pneumatic pockets*, exist only in those insects which have to exert a great muscular force, and which, having great spaces to traverse, ought to have their body specifically light. From what has been said, it is evident that the air ought to have a great influence on the general economy of insects. We shall see hereafter, that it is the only fluid which has a real circulation in them. Now the greater this influence is, the more ought it to exert itself on the organ of circulation, provided such an organ exist. As the dorsal vessel has been considered by different anatomists as coming in place of the heart in insects, let us see whether the influence of the respiratory organ is sensible on this vessel. But to lay open that influence in a more certain manner, let us examine in the first place the organs of respiration themselves, and see whether, when these organs undergo modifications, the dorsal vessel feels their effects.

All the organs of respiration in insects may be reduced to simple *tracheæ*. These organs indeed have not all the same composition; we must therefore divide them into two orders: the first, which we shall call *tubular tracheæ*, on account of their disposition, are composed of three membranes, one external, one internal, and one intermediate. The two first are formed of a cellular membrane, pretty thick, and very extensible; while the intermediate one consists of a cartilaginous string, rolled up into a spiral, which may be unrolled with great facility. It is the convolutions of this elastic string which forms those brilliant and silvery looking conduits, destined to hold air, and to transmit it to all parts of the body. These *tracheæ* are always kept stretched by means of this cartilaginous string. They form tubes, and have a great elasticity, which enables them to dilate, when the air, continually circulating in them, expands. These *tracheæ* are likewise the only ones which are ramified, the multiplied branches of which go to all parts of the body, to communicate the impression of air, the aliment of life as well as of flame.

The second order of *tracheæ*, or the *vesicular tracheæ*, do not form tubes like the preceding; they present, on the contrary, pouches more or less extended, which communicate with one another by means of ramifications, always single, and never branched like those proceeding from the spiral *tracheæ*. These *vesicular tracheæ* are composed of two very white cellular membranes, very supple and very extensible. As these *tracheæ* have not the spiral string observable in the first order, they never communicate immediately with the air, but always by means of spiral *tracheæ*. Accordingly, in the species which have need of a great

quantity of air, and which have very extensive vesicular tracheæ, we observe a particular apparatus destined to supply the elasticity which they want.

This apparatus is composed of hemispherical cartilaginous hoops, furnished with particular muscles, and which, in consequence of this disposition, may be considered as a kind of ribs. In fact, these ribs elevate at every inspiration the vesicular tracheæ and increase their size, allowing them to receive a greater quantity of air; on expiration they depress the tracheæ, and thus serve to drive out the air. These ribs, fixed by their base to the coriaceous envelope, are only moveable by their upper parts. They exist only in those insects that have vesicular tracheæ of a certain extent. We do not observe them in the lepidopteræ, the coleopteræ lamellicornes, and the dipteres, in which the vesicular tracheæ are scarcely a half millimetre (0.0196 inch) in extent. In certain orthopteres, on the contrary, as the *gryllus*, *truxalis*, *acrydium*, in which the tracheæ are large, several millimetres in extent, the cartilaginous hoops, or ribs, always exist, and are in these animals very necessary.

Such are the organs which serve in insects as reservoirs of air. Very different from the lungs and bronchiæ, they are not placed in any particular part; we see them, on the contrary, spread every where, with a sort of profusion: so that there is no part of these animals that does not breathe and receive the action of air.

The general disposition of the tracheæ, and the different particularities of the organization of insects, have occasioned the manner in which these organs communicate with the external air, a mode quite different from what we observe in other animals. The organ of respiration in insects being very much extended, and very much ramified, a single opening would not have been sufficient to distribute the air in all parts with that regularity and abundance that the circulation of that fluid required. Accordingly, the openings by which the tracheæ receive air are always more than one, having never fewer than two. Most commonly that number varies from eight to twelve, and sometimes amounts to 20. These openings have been in general called *stigmata*. But, as in certain species there exist some which open and shut, by means of an apparatus of which the true *stigmata* are destitute, we shall divide them into two different orders. We shall call the first *simple stigmata*, and the second *tremaer* \* *stigmata*.

The simple *stigmata* are most frequently placed on the sides of the body between the folds of the membrane of the back and abdomen. They are always disposed in pairs, presenting, in general, a round opening like a button-hole, with a cartilaginous border. Sometimes, however, this border is totally wanting, and the *stigmata* are then surrounded by a cartilaginous scale of a different colour, from the coriaceous envelope of the body. In the cater-

\* The word is derived from *tréma*, opening; and *air*, air.

pillars the stigmata are equally formed of small cavities, pretty deep, the edges of which are surrounded by a brown mark, and at the bottom of which we discover a stripe of the same colour.

A good deal might be said about the number and situation of these stigmata, if we considered these organs in the different orders of insects. But as these details would carry us too far, we shall satisfy ourselves with observing, that the number of these stigmata is so much the more considerable the more need the animal has of air. Accordingly, in caterpillars we reckon 18 or 20 stigmata, and in a great number of the orthopteres there are 12 or 16, without reckoning the two tremaers. In certain species, which only require a small quantity of air, the number of stigmata does not exceed two; among the apteres the *faucheurs* exhibit this disposition. Besides these stigmata, we see in several orthopteres two openings situated at the bottom of the abdomen; but as their use is not the same as that of the stigmata, we think it unnecessary to describe them here. But we shall not pass over the large oval opening which exists in the breast of the locust above the first pair of paws. It communicates with a large trachea, which extends the whole length of that paw. This opening serves too evidently to introduce air into the superior part of the body, not to be considered as a true stigma; especially as it is formed like the other stigmata of the extremities of the tracheæ. As to the situation of the stigmata, it is subject to numerous variations, always depending on the quantity of air which the insect requires. The more air is required the more is the situation of the stigmata such as to admit an easy introduction of it. The orthopteres, most of the larvæ, the lepidopteres, the hymenopteres, and the dipteres, seem the most favoured in this respect. In the coleopteres the tracheæ are generally placed not advantageously for the introduction of air. Some of those that live in water are obliged when they wish to respire to elevate a little their *elytres*, that the air may enter more easily into their tracheæ.

The second kind of stigma, which we shall call *tremaer*, is composed of an oval opening, which opens and shuts by means of two moveable horny pieces, set in motion by a peculiar apparatus of muscles. The general form of that opening is that of an elongated oval, the greatest diameter of which is from below upwards. In expiration the moveable pieces separate from one another; they shut on the contrary when the inspiration is terminated. These moveable pieces open from within outwards, and do not separate from each other more than half a millimetre, (0.0196 inch.) The movements of the tremaers agree sufficiently with those of the stigmata, and like them they communicate with the tracheæ. It is even very easy in dissecting these parts to recognize the tracheæ which go to them, and the muscles which move the tremaers, two in number, or one for each tremaer. These muscles are destined to separate or open the moveable pieces of the tremaers: accordingly, they shut when these muscles cease to act. These muscles

are composed of fibres sufficiently distinct, which are attached in the second cavity of the thorax in the erismes. Hitherto we have observed the tremaers only in a certain number of orthopteres, where they present very various forms. In general, however, they exhibit the arrangement of which we have spoken; and it is only in the mantes that we see them situated on the lateral and external side of the thorax, between the superior and inferior portion of that part. The tremaers have then a triangular form. Instead of presenting two moveable pieces, they have only a single piece moved by a particular muscle. It is always by means of this muscle that the moveable part is raised up; and as soon as the muscle ceases to act, the moveable part falls down and shuts. The membrane situated below the tremaer is so thin, that we can see in the expirations and inspirations the elevation and depression of the trachea, in proportion as the air enters or goes out.

Though the position of the tremaers experiences some variations, being sometimes situated in the neck, sometimes in the breast, this is not the case with the number of these parts. We never see more than two, the size of which is always proportional to the quantity of air which the insects respire. Besides that manner of receiving air the insects swallow it likewise by the mouth. What is obtained in that way indeed can only make its way to the organs of nutrition; and as we have elsewhere explained its influence on digestion,\* I shall not resume the subject at present. The last mode of respiration which certain insects present, is to have their stigmata placed at the anus. Those exhibiting that organization are chiefly the insects that decompose water, as the larvæ of libellulas and dytics. These stigmata, or the openings which allow the water to escape, are surrounded by small triangular and moveable pieces, the principal use of which is probably to keep away those bodies that might hinder the introduction of water into these stigmata, and at the same time to shut the opening exactly when the insects suspend this introduction. Accordingly, when these insects choose to introduce water into their respiratory organs, they separate the moveable pieces of which we have spoken, and they shut them in the contrary case. But when these pieces are separated we easily distinguish the round opening by which water is introduced into the respiratory apparatus. This opening presents a diameter of about a millimetre, 0.03937 inch,) and it is easy by means of it to let out the water which may exist in the interior of the body.

Considered with respect to their respiratory organs, insects form three distinct classes; namely, those that breathe air immediately; those that live in water and are obliged to come to the surface of that liquid in order to receive the impression of the air, though they likewise lay hold of that contained in the water; and those

\* See my *Memoir on the Intestinal Tube of Insects*, inserted in the *Annales du Muséum d'Histoire Naturelle*, tom. 19.

that decompose water in order to obtain its oxygen. It is obvious that those insects which breathe air immediately, ought to be the only ones in which we find the two kinds of tracheæ of which we have spoken. The vesicular tracheæ would not have sufficient elasticity to drive out the superabundant water, which the insect introduces into its tracheæ. Perhaps this elasticity would not even be sufficient for those which live habitually in water, but do not decompose that liquid. In the second place, the insects which decompose water ought to be the only ones that have but one stigma. This stigma ought to be so placed that the animal may receive the water necessary for it; and we see that whenever this disposition exists, it is always at the anus that this opening is found. But before passing to the description of all these different kinds of organization, we shall here give a table which will render them obvious at one view.

### I. Respiration in Air.

- |                                      |   |  |
|--------------------------------------|---|--|
| I. With tubular tracheæ . . . . .    | { | Division I.—Arterial tracheæ.  |
|                                      |   | Division II.—Pulmonary and arterial tracheæ.   |
| II. With vesicular tracheæ . . . . . | { | Always two orders of tracheæ . . . . .   |
|                                      |   | 1. With cartilaginous hoops, or a kind of ribs.<br>2. Without cartilaginous hoops or ribs. |

### II. Respiration in Water.

- |                                 |   |  |
|---------------------------------|---|--|
| Only with tubular tracheæ . . . | { | Division I. { Breathing by true stigmata, and coming to the surface of the water to breathe air. |
|                                 |   | Division II. { Breathing by an opening placed at the anus, decomposing water.                    |

This table shows us that the result of respiration, or the oxygenation of the blood, or of the humour which comes in place of it in insects,\* ought not to be the same in the different modes of

\* By the expression *oxygenation of the blood*, I do not pretend to affirm that in respiration a portion of oxygen is fixed in the blood, and that there is always a diminution in the volume of oxygen. Though this opinion has been supported by the most skilful chemists, as Lavoisier, Goodwin, Davy, and Berthollet, we must acknowledge that the experiments of Crawford, and especially those of Allan and Pepys, seem to oppose that opinion. Thomson, to whom we likewise owe experiments on the same subject, has observed, that the diminution of volume of the oxygen is not constant; and he considers it as an effect unconnected with respiration. But whether respiration produces only a decarbonization of the blood, that is to say, that the volume of oxygen absorbed represents exactly the volume of carbonic acid gas expired; or whether there be a diminution in the oxygen, besides that which has served to the production of carbonic acid at the expense of the blood, we thought that we might employ the word *oxygenation of the blood*, because it is more convenient in explaining the physiological phenomena of respiration.

We shall put the reader in mind here, that M. Vauquelin proved long ago the necessity of oxygen in the respiration of insects, and that common air retains scarcely any oxygen when these animals can no longer live in it. The air which we expire, on the contrary, is composed of three carbonic acid, 18 oxygen, and 79 azote. See *Ann. de Chim.* tom. xii. p. 273 and 282.

the opinion of the unity of species among men, that a change produced in the white European by heat and food becomes hereditary, is exceedingly inconsistent with notorious facts. Nor does it receive any additional weight from its antiquity; that only affords an additional proof of the facility with which even absurdity may be propagated under the sanction of illustrious names. Dr. Pritchard shows that the previous opinions are irreconcilable with fact, and supposes that certain causes exist, which, acting on the parents, "influence them to produce an offspring endowed with certain peculiar characters, which characters, according to the law of Nature, become hereditary, and thus modify the race." In order to develop those causes, Dr. P. shows that, although climates produce very remarkable variations in individuals, both in the animal and vegetable kingdoms, yet the only permanent effects seem to be derived from cultivation and domestication. Of these effects there are ample and apposite proofs in our cultivated fruits, and among domestic animals. The original stocks remain unaltered, while the most extraordinary changes are brought about by culture. Dr. P. institutes a parallel between the culture of plants, the domestication of animals, and the civilization of man; and, while he allows that some connate varieties *may* be produced by climate, he insists that the condition of man in social life influences more extensively the physical structure than any variety of latitude or local temperature: and concludes by ascribing those remarkable diversities among mankind, not to any moral cause, but to physical causes connected with a particular mode of life. Of this, several interesting illustrations might be cited, in addition to those given by Dr. Pritchard.

Having established the probability of these opinions, he proceeds to determine whether the original race were white or black; and having shewn that black, or at least a very dark brown, in all the inferior tribes, is the primitive colour; and that the same complexion prevails among all savages, and a gradual change towards white occurs through successive races of semi-barbarians to perfect civilization, he infers that the prototype of the human race was a Negro. However satisfactorily this conclusion may be drawn, it is necessary to afford some proof, and accordingly the author adduces numerous illustrations calculated to give considerable force to his argument. He urges with much force the singular varieties in form and complexion among the widely scattered nations of the Pacific Ocean; who exhibit almost every variety, from the savage Papuan to the highly refined European. He proves, by similarity of language, of religious rites, and other points of coincidence, their common origin; and then applies the results obviously obtained from these individuals to the whole of the human race.

The outline then of these Researches into the physical history of man, is, that as uniform diversities are produced by certain known causes; so, all similar or analogous diversities should be ascribed to analogous causes. That as an analogy does actually

exist between certain phenomena among mankind, and those which commonly occur among the tribes, they must be all ascribed to one uniform principle of Nature. That every individual species may be traced to *one* original birth-place, and consequently, that the human species have had an original abode. That the cause which produces the diversities among mankind is civilization, which does not produce any effect on the parent which is hereditary; but, by acting on the parent physically, influences the production of certain offspring. And finally, that the change in form and complexion has been from black to white; and that the primitive race of men were negroes. After these conclusions, the remainder of the volume is chiefly devoted to historical researches in corroboration of the last startling inference. From these inquiries it appears, that the most ancient nations of whom we have any record were negroes, that they have gradually lost their characters, and that many, or most of the now existing nations, though widely differing in form and complexion, may be traced to them.

The Hindoos and Egyptians are certainly the oldest nations of antiquity. The correspondence between them is very remarkable, and the importance of the resemblance increases as we find it increase in proportion to the antiquity of the period to which we refer. In morals, in politics, and endless superstition, the resemblance is so unbounded, as to leave no doubt of an identity of origin. The pretensions of the Babylonians, who alone rival them in antiquity, will not admit of scrutiny. It will be worth while to ascertain the physical and mutual connexion of these early races, as by doing so, some light would probably be thrown on the history of the species in general. These nations appear originally to have been absolute negroes.

It is to be hoped that Dr. Pritchard will pursue this very important subject through all the forms of which it is susceptible, and to which he appears so fully competent to give interest.

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II. *Traité des Poisons tirés des Regnes Mineral, Vegetal, et Animal, &c. A Treatise on the Poisons of the Mineral, Vegetable, and Animal Kingdoms, or a general Toxicology considered as related to Physiology, Pathology, and Medical Jurisprudence.* By M. P. Orfila, Pensionary Naturalist of Spain, Doctor in Medicine of the Faculty of Paris, Professor of Chemistry and of Natural Philosophy. Vol. I. Paris, 1814.

This work, if we are to form a judgment from the part of it which is already published, promises to be very complete. The author is a practical chemist, as well as a physician, and he has laid it down as a rule to give an account of no poison without an exact description of its properties, and without ascertaining by experiment the effects which it produces on animals. If we were disposed to find fault we should blame the author for the too great number of parts into which he has subdivided each article. This

has occasioned some repetition, and given rather a stiff appearance to the work. But when the novelty of many of the subjects treated of is considered, and the numerous mistakes respecting them still taught in the most recent books on medical jurisprudence, it was better to err on the side of minuteness and repetition than on that of omission.

M. Orfila divides poisons into six classes; namely, *corrosive*, *astringent*, *acid*, *stupifying*, *narcotico-acrid*, and *septic*. The present volume, published in two parts, includes the first two of these classes.

The corrosive poisons consist of preparations of the following substances: mercury, arsenic, antimony, copper, tin, zinc, silver, gold, bismuth. It includes likewise the following substances; sulphuric acid, nitric acid, muriatic acid, phosphoric acid, fluoric acid, oxalic acid, tartaric acid; the caustic alkalies, barytes, lime; phosphorus; cantharides. The astringent poisons are confined to preparations of lead.

The principal poisonous preparation of mercury is *corrosive sublimate*, or *dechloride of mercury*. The chemical properties of this salt are described at great length. It may be sufficient here to say, that it is a white, heavy substance, having an acrid taste, and soluble in about 11 times its weight of water. When heated it sublimes in a white smoke, which excites coughing; but has not an alliaceous smell. If a plate of clean copper be exposed to this smoke it becomes tarnished, and when rubbed assumes a white colour. The solution of corrosive sublimate is precipitated brick red by an alkaline carbonate; yellow by a caustic fixed alkali and lime-water; white by ammonia; white by prussiate of potash; black by a hydrosulphuret; white by albumen. Corrosive sublimate, when swallowed in a considerable dose, (as 30 grains) acts with great violence, occasioning evacuations both upwards and downwards, and death very speedily ensues. M. Orfila has ascertained that white of egg, dissolved in water, and administered in considerable quantity, and as speedily as possible, constitutes the best antidote to this poison.

All the preparations of arsenic are violent poisons; but the most common state in which it is administered is that of arsenious acid, or white oxide of arsenic. Its properties, and the fatal effects which it produces when taken internally, are so well known, that it would be superfluous to describe them here. When arsenic is swallowed in a state of solution, sulphureted hydrogen taken soon after is an efficacious antidote. But this poison is usually administered in a solid state, and in that case the antidote is totally inefficacious. The proper treatment is to endeavour to get the poison out of the stomach as speedily as possible, by vomiting. Great quantities of hot water, having some sugar or mucilaginous matter dissolved in it, should be swallowed, and vomiting excited, if it does not take place spontaneously, by introducing the finger, or a

feather, into the throat. By persisting in this treatment many persons poisoned by arsenic have recovered.

Of the antimonial preparations there is scarcely any likely to be administered, or to be taken internally, so as to produce death, except tartar emetic, which is a triple salt composed of tartrate of potash and protaitrate of antimony united together. This salt is usually administered as an emetic, and it generally acts in that way with great violence. When vomiting does not take place it acts as a poison, occasions violent spasms of the œsophagus and neck, which prevent the possibility of swallowing. When it is administered to dogs, and their œsophagus is tied up so as to prevent vomiting, the consequence is death. When a person is poisoned with tartar emetic, our object should be to produce vomiting, and for this purpose warm water is the most efficacious remedy. The decoction of yellow bark likewise, proposed by Berthollet, is of service, when administered in such quantities as to decompose the salts.

Perhaps no poisons are so frequent as the preparations of copper. This metal is used for so many purposes connected with the preparation of food, it is so easily oxidized, and all its compounds are of so deleterious a nature, that many instances must occur of injurious effects from it. The most common preparations of copper likely to be applied as poisons are verdigris, acetate of copper, sulphate of copper, nitrate of copper, muriate of copper, and copper dissolved by fat. The taste of all these preparations is exceedingly disagreeable; but they may be mixed in small quantities with food without being perceived. The preparations of copper occasion violent colics, vomiting, prostration of strength, and death. The best antidote is sugar, either swallowed solid, or dissolved in water. It should be taken both ways, and in considerable quantities. The liquid induces vomiting, and thus gets rid of a portion of the poison.

The only preparation of tin likely to be used as a poison is the muriate which is used in considerable quantities by the dyers. Its taste is exceedingly nauseous. It occasions violent colics, vomiting, and death. The best antidote is milk, which, when drank in considerable quantity, seldom fails to cure the patient, by decomposing the salt and removing all the disagreeable symptoms.

Zinc is so little employed for culinary purposes that it is but rarely that it can act as a poison. The sulphate of zinc, however, is so common a salt, that it has been often administered in considerable quantities by mistake. It is by no means a dangerous poison; for it acts speedily as an emetic, and is thrown out of the system before it has time to produce bad effects. The business of the physician is to promote this effect by making the patient swallow considerable quantities of warm water. Milk also, which decomposes the salt, may be administered with advantage.

Nitrate of silver is well known as one of the most corrosive salts employed in pharmacy. When introduced into the stomach it soon

occasions death, by corroding that organ, and bringing on gangrene. When injected into the veins, even in very small quantities, the animal dies almost immediately. The antidote to this poison is common salt dissolved in water, which decomposes the nitrate of silver, and forms an insoluble chloride, which produces no injurious effects upon the animal economy.

Gold is not likely to be administered as a poison. When this metal is dissolved in nitro-muriatic acid it forms a salt, which acts more violently on the animal economy than corrosive sublimate. The symptoms are similar, except that the salt of gold does not produce the same effects upon the mouth and gums. No antidote against this poison is known. The object of the physician must be to get it thrown out of the stomach as speedily as possible by vomiting. I think it very probable that a solution of sulphate of iron would destroy its deleterious effects, by decomposing the salt, and throwing down the gold in the metallic state.

The nitrate of bismuth, and the white pigment for the face, known by the name of pearl white, which is a preparation of bismuth, act with considerable energy as poisons when introduced into the stomach. The best antidotes against these deleterious preparations are milk and mucilaginous liquids, swallowed in considerable quantities.

Sulphuric acid has sometimes been swallowed by mistake, and sometimes taken by persons who wished to destroy their lives. The violence of its action on animal substances is well known. The mouth, the œsophagus, and the stomach, are speedily corroded by it, and their functions destroyed; the consequence is death, attended with the most excruciating pain. From the experiments of M. Orfila it appears that the best antidote against this corrosive acid is calcined magnesia, and that if this substance be administered soon after the acid has been swallowed, it prevents death, and enables the patient to recover.

Nitric acid has been frequently swallowed in considerable quantities by unhappy persons who wished to destroy themselves. It is still more corrosive than sulphuric acid, acts with more violence, and produces dreadful pains. Magnesia is also the best antidote against this poison, and if administered very speedily, it may even save the life of the patient.

Muriatic acid, though it cannot be exhibited in so concentrated a state as sulphuric and nitric acids, produces the same deleterious effects when taken internally, and speedily occasions death, attended with the same dreadful symptoms. Magnesia is likewise the best antidote against this acid.

Phosphoric and fluoric acids, sulphurous acid, phosphorous acid, are all likewise poisonous; but as the chance of their being introduced inadvertently into the stomach is not great, it does not seem necessary to dwell upon them. M. Orfila likewise ranges oxalic and tartaric acids among poisons; but he gives no instance of their deleterious effects.

Caustic potash and soda are not less corrosive than the concentrated acids; hence, when introduced into the stomach, they act with equal violence, and very speedily destroy life. The best antidote is vinegar, administered in such quantity as to neutralize the alkali.

Ammonia likewise acts with considerable violence upon the animal economy when swallowed, and produces convulsions and death. Vinegar answers equally well as an antidote against ammonia as against the fixed alkalies.

Barytes, carbonate of barytes, and muriate of barytes, are known to act as violent poisons when introduced into the stomach. The effects which they produce are similar to those produced by the other corrosive poisons. An alkaline sulphate by converting the barytes into an insoluble sulphate, which does not act upon the human body, is the best antidote against this poison.

Lime is not a very energetic poison, yet when swallowed in considerable quantities, it destroys life by the inflammation which it induces in the stomach. The same mode of treatment answers for lime that was recommended in the case of poison by the fixed alkalies.

Phosphorus, when introduced into the stomach, always proves fatal. It is gradually converted into phosphorous and phosphoric acids, which corrode the stomach and intestines, and produce inflammation. The object of the physician should be to throw the phosphorus out of the stomach as speedily as possible by means of an emetic. When the phosphorus has been introduced in the state of extreme division it is useful to make the patient swallow large quantities of water, holding magnesia in suspension. The liquid, by filling the stomach, prevents the phosphorus from being readily converted into an acid, and the magnesia neutralizes any acid that may be formed.

M. Orfila introduces pounded glass and stone-ware among the number of poisons. He gives a number of cases in which these substances were swallowed without any inconvenience, and others in which they produced destructive effects. It is obvious that these substances act only mechanically. It must depend upon accident whether any of their sharp points wound any part of the stomach and intestines, or whether they make their way without inflicting any wound.

*Cantharides*, *lytta vesicatoria*, or *Spanish flies*, are a set of insects well known in the materia medica, as they constitute the essential ingredient in the common blistering plaster. *Cantharides*, according to the analysis of M. Robiquet, contain a variety of different substances; but the most important is a white substance, having the form of small crystalline plates, insoluble in water, soluble in boiling alcohol; but is deposited on cooling in small crystalline plates like spermaceti. It is soluble in oils. It possesses the blistering property in great perfection, and is the only substance in *cantharides* that has it.

The effect of cantharides when taken into the stomach in any quantity is well known. It produces a most furious satyriasis, which usually terminates in gangrene and death. No antidote against this formidable poison has been hitherto discovered.

All the preparations of lead are poisonous; but those most likely to be taken into the stomach are the oxides of lead, white lead, litharge, and sugar of lead. The water near lead mines, in which the galena is washed, is usually injurious to the health, in consequence of particles of that substance which it holds in suspension. The fumes of lead prove no less injurious to those who are exposed to them.

Lead shows its deleterious effects in those who are exposed to its action; but slowly. Obstinate costiveness and violent colics, known by the name of *colica pictonum*, first attack the patient. This is followed by paralysis and death. It appears from the experiments of M. Orfila, that sulphate of magnesia acts as an antidote against acetate of lead. An insoluble sulphate of lead is formed, which does not injure the animal economy, and the acetate of magnesia acts merely as a purgative. The common method of treating persons poisoned by lead is by a course of purgative and emetic medicines, which seldom fail to cure the patient.

In an appendix M. Orfila gives us a set of experiments on iodine, introduced into the stomach of animals. In small quantities it acts as an exciter. When administered to the amount of about half an ounce it occasions death, if the animal be prevented from vomiting, gradually corroding the stomach and intestines. When taken in larger quantities it destroys life even though the animal be allowed to vomit.

In another appendix M. Orfila shows by experiment, that charcoal powder is not an antidote against corrosive sublimate and white oxide of arsenic, as had been advanced by M. Bertrand.

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## ARTICLE XII.

### *Proceedings of Philosophical Societies.*

#### ROYAL SOCIETY.

On Thursday, the 6th of April, a paper by Mr. Knox was read, on the coloured rings formed when a flat plate of glass is pressed against a convex lens. Mr. Knox conceives that the reason why neither Sir Isaac Newton, Dr. Herschell, nor any other philosopher, was able to give a satisfactory explanation of these coloured rings, was, that they were not acquainted with all the phenomena. He made his experiments according to the method pointed out by Dr. Herschell in his paper on the subject published in the *Phil. Trans.* for 1804, Mr. Knox described a great many new phenomena

which he observed, and of which it is scarcely possible to give an idea without the assistance of figures. He found the phenomena the same when the experiments were tried in vacuo as in the open air. Nor did the introduction of water between the plates alter the phenomena much. Hence he conceives that the rings are not owing, as Newton supposed, to the film of air between the plates. He conceives them to be derived from the reflection of the surface of the glass next the film of air. The changes induced by the passage of the ray from one medium into another may occasion such refractions as to collect together the different bundles of coloured rays so as to produce the coloured rings.

On Thursday, the 13th of April, a paper by Major Rennell was read, stating further proofs in confirmation of the existence of a current setting upon the Scilly Islands, in the chops of the Channel. He adduced three proofs that there exists a current running east along the north coast of Spain. The French navigators are aware that there is a current which sets north along the west coast of France, and it is obviously the Spanish current which has received a northerly direction, from the position of the land. All the sand and alluvial matter which is brought into the Bay of Biscay by the Garonne, the Loire, and the other rivers which empty themselves into the sea on the west coast of France, is found on the north side of the mouths of the respective rivers, and not on the south, a circumstance which can be occasioned only by a northerly current. He brought several facts showing that a northerly current exists about lat.  $49^{\circ}$  at the mouth of the Channel, and rendered it probable that it flows also westerly, as well as north. This current flows at different times with different velocities, and this he assigned as the probable reason why it was not discovered sooner.

There is a current likewise which flows east along the south coast of Ireland, and meeting with the first described current, flows northward into St. George's Channel, and moves in the direction to Cardigan Bay. This current is the cause why ships are so frequently driven into that bay. There is a current which runs up along the west coast of Ireland, turns east along the north coast, and then flows south certainly as far as Dublin, and probably further. There is another current that flows north along the west coast of Scotland, bends round the northern part of the island, and flows south along the east coast of Great Britain as far as Harwich, where it meets with the current in the English Channel. These produce a current north-east along the coast of Flanders and Holland; it then proceeds north along Jutland, receives the current coming out from the Baltic, proceeds to the Naze of Norway, and then runs north along the coast of that country.

On Thursday, the 20th of April, a paper by Sir Humphry Davy on a combination of iodine and oxygen, was read. The author in a former paper had given an account of several unsuccessful attempts to form this compound. It occurred to him that if *euchlorine gas* (*oxide of chlorine*) were made to act directly on iodine, the

result might be more fortunate. Accordingly he caused a current of euchlorine gas, dried by passing through muriate of lime, to act upon iodine. A combination took place. When the solid body formed was exposed to a moderate heat, chloride was driven off in the state of vapour, and an oxide, or compound of oxygen and iodine, remained behind. This substance is solid, of a white colour, and considerable specific gravity, as it sinks rapidly in sulphuric acid. Its taste is astringent. It dissolves rapidly in water, and forms a colourless solution, which has acid properties, and which the author calls oxiodic acid. This liquid first reddens vegetable blues, and then destroys them. Other colours it converts into yellow. When the oxide is exposed to a heat rather below the boiling point of olive oil, it is decomposed, oxygen gas being driven off, and iodine left behind. Sir H. Davy, from various experiments, made however on a small scale, considers this compound as composed of one atom of iodine and five atoms of oxygen. If we reckon the weight of an atom of iodine 15·621, and that of an atom of oxygen 1, this will give us the oxide composed of 15·621 iodine and 5 oxygen, or of

Iodine .....	75·75
Oxygen .....	24·25
	<hr/>
	100·00

Oxiodine has the property of combining with the different bases, and forming oxides, which the author describes. The oxiodic acid likewise combines with the alkalies, earths, and oxides, and forms a class of salts; which he distinguishes by the name of oxiodates.

This substance has the remarkable property of combining likewise with the different acids, and of forming solid compounds, which for the most part crystallize. When dropped into sulphuric acid, a solid substance falls, which, when heated, melts, and assumes, on cooling, the form of yellow-coloured rhombs. According to Sir H. Davy's experiments, this compound is composed of 20 oxide and 8 sulphuric acid. He considers it as a hydrate. If we suppose it a compound of one integrant particle of oxide and one integrant particle of sulphuric acid, it appears from the above analysis that there may be present in it two integrant particles of water. The oxide combines also with phosphoric acid, phosphorous acid, nitric acid, and oxalic acid. These combinations are probably all hydrates.

The oxiodic acid dissolves gold and platinum. When heated, the water is driven off, and the acid at last remains in the state of a thick paste. This paste is a hydrate of oxiodic acid. Sir H. Davy tried to obtain a compound of oxygen and iodine containing a less proportion of oxygen than the above-described oxide; but his attempts were unsuccessful. The supposed combination of oxygen

and iodine described by Gay-Lussac was a compound of oxide and sulphuric acid, and contained likewise some barytes.

#### LINNÆAN SOCIETY.

On Tuesday, the 4th of April, a paper by Dr. Leach was read on the classification of the insects called notonectides.

On Tuesday, the 18th of April, a paper by the Rev. P. Keith was read, on the ascent of the sap in trees. Mr. Keith took a view of the different hypotheses hitherto proposed to account for the ascent of the sap, and showed that they were all inadequate to explain the phenomena. One of the latest of these hypotheses is that of Mr. Knight, who conceives that the ascent of the sap may be owing to what is called the *silver grain* of the wood. To refute this hypothesis, it is only necessary to mention that some of the tallest plants known have no silver grain whatever. Yet it is obvious that the sap ascends in them as well as in other plants. Mr. Keith considers the hypothesis of Saussure, that the sap is forced up by the contraction of the vessels, as with certain modifications, most likely to be true.

#### GEOLOGICAL SOCIETY.

*March 17, 1815.*—A communication was read from the Woodwardian Professor; the object of which paper is to describe a remarkable variety of geode, several individuals of which were lately met in digging a well at Oakhampton, Devonshire. They occurred in a bed of clay about ten feet deep. Their figure is nearly that of a compressed spheroid, and each ball consists of a nucleus of ochreous oxide of iron enclosed within a shell of a cavernous structure, the shells of which are externally so regular that the mass might easily be mistaken for a fossil madrepore.

With regard to the mode in which this and similar bodies have been formed, the Professor suggests that the deposition of the beds in which they are formed might have been attended with effervescence, and that this spheroidal figure and cavernous structure might have been occasioned by gas uniformly distending, and at length escaping insensibly through the cellular crust by which it had been confined.

The reading of Mr. Horner's paper on the south-western part of Somersetshire was continued.

*April 7.*—A short notice from Mr. Horner on the locality of some specimens from the island of Tino, presented by him to the Society, was read; also a communication from the Woodwardian Professor, supplementary to his former, on the Cambridgeshire strata.

This paper furnishes a new locality of the flattened Headington oyster, it having been lately found forming a strong bed at Willham, about six miles north of Ely, in the Fen level, a position which ascertains its geological situation to be below the chalk. Another remarkable appearance is stated to occur at Reche, not far from

Swaffham, on the edge of the fen. This place has for ages supplied the surrounding country with the chalky lime-stone called clunch, in consequence of which extensive excavations have been formed, which have laid bare a large mass of a lenticular shape imbedded in the clunch. The greatest thickness of this mass is about eight feet, and its length and breadth from 30 to 40 yards. It is comprised of kidney-shaped masses, about the size of a walnut, of ochre-yellow lime-stone, covered superficially with a thin coat of green oxide of iron. It contains a few *spines* of an echinus completely converted into spar, and also vegetable impressions seemingly belonging to plants of the tribe of gramina.

At this meeting the reading of Mr. Horner's paper on the south-western part of Somersetshire was concluded.

The most elevated ground in the district here described is Exmoor Forest and the Quantock Hills. This tract is composed for the most part of a grey-wacke formation; the strata of which present more or less coarsely-granular mixtures of quartz and clay, sometimes considerably calcareous, alternating with slaty beds, which often are not to be distinguished by external characters from primary clay-slate. Within these latter beds are included thick short beds of lime-stone full of madrepores, and containing veins and nests of grey copper malachite and hæmatite. In the ground of Hestercombe occurs a wall (probably a vein) of fine grained grey granite; the slate rises up towards it at a very high angle, is considerably indurated, and at the plane of junction appears to be more or less penetrated by the granite.

Where the hills of grey-wacke sink down into the lower country their sides are covered with beds of conglomerate passing into red sand-stone, which extend to a considerable distance. These beds appear to consist of the same materials as the grey-wacke formation, but decomposed to a considerable degree. They are traversed by veins of calcareous spar, sometimes of so great a thickness as to be quarried for lime-stone.

The conglomerate, with its accompanying sand-stones, is covered in many places by a red argillaceous sand-stone, containing a variable proportion of calcareous matter, but being principally characterized by spots and stripes of a greenish-grey colour. It is of an uniformly fine texture, never contains any fragments either angular or rounded, is in places traversed by veins of gypsum, and appears to be the same rock as that in which the salt-beds of Cheshire and Droitwich are situated.

To this rock succeeds the strata of Lyas lime-stone, which with their accompanying beds of slate-clay are sometimes seen distinctly resting on the red gypseous rock, and sometimes in very broken and disturbed stratification appear to alternate with it.

On the sea coast about three miles westward of the river Parrett, in the cavities formed by the curvature of the Lyas strata, is a stiff blue clay, covered with peat, in which are imbedded trunks of

trees, often of great size, and with their lateral branches still attached to them. These remains of an ancient forest extend to an unknown distance into the sea.

## ARTICLE XIII.

### SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

#### I. Lectures.

The Summer Courses of Lectures on the Theory and Practice of Physic, by Dr. Roget, and of Materia Medica and Medical Jurisprudence, by Dr. Harrison, will commence, as usual, in Windmill-street, on the first week in May. The Lectures on Chemistry will, in consequence of Dr. Davy's absence from town, be given during the summer by Dr. Granville.

Dr. Clutterbuck will begin his Summer Course of Lectures on the Theory and Practice of Physic, Materia Medica, and Chemistry, on Friday, June 2, at ten o'clock in the morning, at his house, No. 1, in the Crescent, New Bridge-street, Blackfriars, where further particulars may be had.

#### II. Gas Lights.

IN answer to M., who puts some queries on the subject of gas lights in the last number of the *Annals of Philosophy*, p. 318, I have to observe that, in all my experiments on carbureted hydrogen gas I never was able to produce an explosion by firing any mixture of it whatever with common air. It merely burnt rapidly. Such a rapid combustion on a large scale I am sensible would produce an explosion. When mixed with oxygen gas, it will not fire unless it bear a certain proportion to the oxygen gas. The oxygen gas must amount at least to 105 measures (supposing we take 100 measures of carbureted hydrogen gas), and no explosion takes place whenever the oxygen amounts to 227 measures. From these facts, I infer that whenever 11 parts of common air and one part of carbureted hydrogen are mixed together, the mixture will explode when kindled; but if the carbureted hydrogen exceed  $\frac{1}{11}$ th of the common air, it will no longer be capable of exploding. All proportions between  $\frac{1}{11}$ th and  $\frac{1}{227}$ th will explode.

The gas produced by the distillation of pit-coal consists almost entirely of carbureted hydrogen. Hence I conceive that the preceding observations apply to it correctly; of course coal gas, when collected in reservoirs, never can explode unless it be mixed with at least six times its bulk of common air. This I think never can happen except from an unaccountable and culpable negligence of those who are employed. If the reservoir is not air-tight, if it be

filled with coal gas, and the pressure by which it is made to sink in the water as the gas is expended be taken off, I can easily conceive that after an interval of a day or two (according to the size of the lack), five-sixths of the gas may make its escape, and be replaced by as much common air; for as coal gas is much lighter than common air, it will always make its escape when it can. I am persuaded that the explosion at Birmingham, and some others which I have heard of, were produced in this manner. But as nothing is easier than to ascertain whether the reservoir be air-tight, I conceive that with common precaution gas lights may be used with as much safety as any other light whatever.

As to the coal that answers best, it is undoubtedly that variety which contains the greatest quantity of bitumen. Newcastle and Wigan coal will probably answer better than any other coal in the island, except some of the Fife coal, which is absolutely of the same quality.

With respect to the degree of pressure requisite to make it issue with sufficient velocity from the mouth of the pipes, I do not know that any accurate experiments have been made to determine it, nor do I believe that much nicety is necessary. Some Gentlemen have expressed doubts on the subject, because Mr. Wilkinson was not able to force an efficacious draught of air through a pipe a few hundred feet in length; but the cases are by no means parallel. Mr. Wilkinson required a current of considerable velocity; but no such velocity is requisite for the coal gas. Besides, the water generated by the combustion of the coal gas at the extremity of the tube must occasion a diminution of pressure, which will serve to regulate the issue of the coal gas from the reservoir.

### III. *Toads found in Rocks and Trees.*

A correspondent at Bristol, I. B. I., has proposed some queries relative to the many stories in circulation respecting toads found in the middle of solid rocks. There can be no doubt that toads have been frequently found in such positions, though in no one case that I have seen has it been ascertained that the animal was completely excluded from the external air. I have myself conversed with workmen who had found toads both in coal-pits and in quarries; but they were never able to bring decisive proof that the animal was completely surrounded by the coal or the stone. It is generally observed that when a toad is found in this position the creature dies very soon after being brought out of its lurking hole. This seems to me a proof that the animal, if not entirely excluded from the air, must have been at least nearly so; for the sudden death can scarcely be ascribed to any thing else than the change of situation.

### IV. *On the Focal Powers of the Eye.* By Dan. Pring, Surgeon.

It has been presumed from the supposed resemblance of the eye to some optical contrivances, that it is necessary the former should possess a variable focal power, in order to account for the capability of viewing objects at various distances. But this supposition appears

to be founded upon a false analogy. The eye sees with a distinctness which agrees with the distance of the object : a result which can be determined only by the faculty of vision itself, without any correspondent variety of the focal power. - That the focal powers of the eye undergo no change in order to produce vision at different distances, appears from this fact, namely, that we are enabled to see a great variety of objects *at all distances*, within a range of perhaps from three yards to three miles *at the same time*. A thousand objects may be interspersed in this range. Can there, then, exist a correspondent number of distinct focal powers in the eye *at the same time* ?

The eye requires a *determinate focal power* : its only movements are for the purpose of regulating the axis of vision : and perceptions are formed according to the relation which subsists between light at various approximations to a focus, and the faculty of vision allied with the retina.

Bath, April 4, 1815.

#### V. Proposed Road over Hounslow Heath.

(To Dr. Thomson.)

SIR,

AN Act of Parliament has passed for the inclosure of Hounslow Heath, and the commissioners have already begun to act upon it. It may not, however, be too late to induce them to lay down one of their roads in the line on which Gen. Roy measured his base. The plan seems unobjectionable, and it certainly would be attended by circumstances which make the execution of it highly desirable. The suggestion of it in the *Annals* can at least do no harm, and will oblige

A CONSTANT READER.

#### VI. On Mr. Lockhart's Imaginary Cube Roots. By Dr. Tharks.

(To Dr. Thomson.)

SIR,

Having seen in the last number of your *Annals* a paper by Mr. Lockhart, which contains this most extraordinary assertion, that every cubic equation has more than two imaginary roots, I beg leave to state to you that the imaginary expression which Mr. L. supposes to be a root of 4, different from the two well-known roots, is nothing but a different form of one of them.

Mr. L.'s expression is,  $-\frac{3 + \sqrt{-3}}{2} + \sqrt{-\left(\frac{1}{2} - \frac{3}{4}\sqrt{-3}\right)}$ . Now it will be easily seen that  $-\left(\frac{1}{2} - \frac{3}{4}\sqrt{-3}\right)$  is the square  $-\left(\frac{1 + 3\sqrt{-3}}{2}\right)$ . The above expression, therefore, is,  $\left(\frac{3 + \sqrt{-3}}{2} + \frac{1 + 3\sqrt{-3}}{2}\right) = -(2 + 2\sqrt{-3})$ , which is

really one of the two imaginary roots of 4,  $-(2 \pm 2\sqrt{-3})$  resulting from the equation  $x^2 + 4x + 16 = 0$ .

I am, Sir, your obedient servant,

5, Bateman's Buildings,  
Soho-square.

J. L. TIARKS.

VII. *Another Communication on the same subject.*

(To Dr Thomson.)

SIR,

My attention was arrested by the seventh article of scientific intelligence in your last number (p. 315). Some mathematicians have denied the universality of the doctrine of there being as many roots to an equation as it has dimensions, but none have been able to maintain that there are more. I therefore examined Mr. Lockhart's proof with some attention; and I conceive that he cannot take it ill if I endeavour to point out the source of his mistake. I think, likewise, that you will be indebted to me for doing so; since the "method for approximating towards the roots of cubic equations belonging to the irreducible case," has justly given some weight to the author's opinions; and you must be desirous of not being the means of propagating an error which can only be supported by the authority of his name.

I must begin by laying down that  $-\frac{3 + \sqrt{-3}}{2} + \sqrt{-\left(\frac{1}{4} - \frac{3}{4}\sqrt{-3}\right)}$  is not a cube root of 64, but of 8. To show this in the simplest manner, we will substitute  $\frac{1}{4} + \frac{3}{4}\sqrt{-3}$  in the place of  $\sqrt{-\left(\frac{1}{4} - \frac{3}{4}\sqrt{-3}\right)}$ , for these two quantities may easily be shown to be equal by the rule for extracting the roots of binomial surds; and then  $-\frac{3 + \sqrt{-3}}{2} + \frac{1}{4} + \frac{3}{4}\sqrt{-3} = -\frac{2 + 2\sqrt{-3}}{2} = -1 + \sqrt{-3}$ , which, when cubed, will be

found to make up 8. But it may be asked how a mathematician so well acquainted with algebraical processes, and especially with cubic equations, can have made such a mistake, and in what part of his reasoning the fallacy lies? This question I think admits of a complete answer; for the error will be found in his manner of bringing out the value of  $(-2 + 6\sqrt{-3})\sqrt{-\left(\frac{1}{4} - \frac{3}{4}\sqrt{-3}\right)}$ . It is perfectly clear that  $a\sqrt{b}$  is equal to the square root of  $a^2b$ ; but it escaped Mr. L. that by squaring his quantity he introduced an ambiguity, since the square root of  $a^2b$  is  $\pm a\sqrt{b}$ ; and in this instance he ought to have taken the negative instead of the positive root; the value then would have been  $36 - 28 = 8$ , instead of  $36 + 28 = 64$ . To show that this is so, we have only to take the value above assigned to  $\sqrt{-\left(\frac{1}{4} - \frac{3}{4}\sqrt{-3}\right)}$ ; and then we shall find that  $(-2 + 6\sqrt{-3}) \cdot \left(\frac{1}{4} + \frac{3}{4}\sqrt{-3}\right) = -28$ .

There are some particulars in your last number which would not

deserve a separate communication, and on which I may yet take this opportunity of offering some short remarks. In your account of Count Rumford you have omitted to mention the memoirs which he published abroad, and the medal which he entrusted for distribution to the Royal Society. To the last query of M. (p. 314), it may be answered that he will find the subject mentioned at p. 31 of Smith's Optics, and at p. 148 of Dr. Jurin's Essay on Distinct Vision (annexed to that work); and that a number of curious experiments, which give greater precision to the inquiry, will be found in Harris's Optics, and in a paper of Dr. Herschell's in the Phil. Trans. for 1786 (vol. lxxvi.).

N. R. D.

### VIII. *Effect of Sulphuric Acid on Agates.*

What can be the reason that agate heated, or rather boiled, in concentrated sulphuric acid, becomes in its different layers differently coloured; the white strata becoming milk-white, and the greyish ones as black as pitch? I heard this from Mr. Banks, in Bath, and would not believe it; but Mr. Banks was so good as to make the experiment in my presence, and I was convinced of the fact. Saxon agate is more affected than the Scotch is: half an hour or one hour is generally sufficient for producing the effect. The colours penetrate to a considerable depth. I think this fact fully merits the attention and investigation of chemists and mineralogists.

Glasgow, March 15, 1815.

J. HAMEL, M.D.

### IX. *Constituents of the Ribes Grossularia, (Green Gooseberry.)*

We know from Scheele that the juice of this fruit contains citric and malic acids. Dr. John has lately subjected it to a more detailed analysis. This juice has a greenish, and somewhat thick consistence; but it does not gelatinize when exposed to the air. John could find in it hardly any traces of sugar, and therefore is disposed to doubt the possibility of converting it into wine; but this is often and successfully put in practice in this country. Indeed the taste of this gooseberry is very distinctly sweet. The following are the constituents found in this juice by John.

Much water.

Uncrystallizable sugar.

Supercitrate of potash.

Supermalate of potash.

Supercitrate of lime.

Supermalate of lime.

A little resin.

Prunin or cerasin.\*

Insoluble modified gum.

A salt with base of magnesia.

Traces of phosphates of lime and magnesia.

Trace of muriate of lime?

A little phosphate? of iron.

Ammonia, probably combined with citric and malic acids.

Fibrin.

\* This is a substance similar to gum in appearance, but it does not dissolve in water, only swelling up and becoming gelatinous in that liquid.

X. *Constituents of Angelica or Changelica.*

This plant, one of the greatest ornaments of cold countries, has been analyzed by John. The following are the constituents which he obtained from 300 parts of the dried plant.

Colourless and very volatile oil .....	
Gum.....	100.5
Inulin .....	12
Bitter extractive .....	37.5
Sharp tasted resin .....	20
A peculiar substance, soluble only in potash....	22
Woody fibres .....	90
Water and loss .....	18
	<hr/> 300

The earthy constituents were

Phosphate of lime.	Phosphate of magnesia.
Phosphate of iron.	Silica?

## ARTICLE XIV.

*New Patents.*

ROBERT DICKINSON, Great Queen-street, London; for certain improvements in the art of sadlery. Nov. 28, 1814.

ROBERT DICKINSON, Great Queen-street, London; for certain improvements in the manufacture of barrels and other packages made of iron or other metals. Dec. 10, 1814.

ROBERT SALMON, Woburn, Bedford; for improved movements and combinations of wheels for working of cranes, mills, and all sorts of machinery, either portable or fixed. Dec. 10, 1814.

EDWARD GLOVER, Penton-place, Walworth, Surrey; for an apparatus for drawing or extracting bolts, nails, &c. and for various other useful purposes. Dec. 10, 1814.

HENRY JULIUS WINTER, Dover; for a method of giving effect to various operating processes. Dec. 12, 1814.

JOHN FRANCIS WYATT, Furnival's Inn, engineer; for a new kind of bricks or blocks, one of which is particularly adapted for the fronts of houses and other buildings, giving to them the appearance of stone; another is applicable to a new method of bonding brick-work; also a new kind of blocks or slabs for paving floors, and facing or lining walls, instead of ashler, which will resemble marble or stone, and which may also be applied to steps or stairs, and other parts of buildings. Dec. 15, 1814.

JOSEPH C. DYER, of Boston State, America, now residing in

Camden Town, Middlesex; for certain additions to, and improvements on, machinery to be made and applied in manufacturing cards for carding wool, cotton, silk, and tow, and other fibrous materials of the like description. Communicated to him partly by a foreigner residing abroad. Dec. 15, 1814.

JAMES SMITH, Newark-upon-Trent; for a self-acting sash fastening. Dec. 20, 1814.

WILLIAM EVERHARD, Baron von Doornich, Sun-street, Bishopgate-street, London; for improvements in the manufacture of soap. Dec. 20, 1814.

JOHN VALLANCE, jun. Brighthelmstone; for an apparatus and method of so constructing and securing brewers' vats or store-casks as to prevent the vat falling to pieces, or even breaking, though every one of the hoops on it should be broken asunder, and consequently preventing the liquor from being lost; and also for preventing the loss of liquor, even if a cock or all the cocks of the vat should be broken off. Dec. 20, 1814.

ROBERT DICKINSON, Great Queen-street, London; for certain improvements in implements applicable to the purposes of navigation, namely, an improvement or improvements in the ship's nun-buoy and beacon-buoy. Dec. 20, 1814.

EDWARD JORDEN, Norwich, and WILLIAM COOKE; for an apparatus for the detection of depredators, which they denominate *The Thieves' Alarm*. Dec. 24, 1814.

FREDERICK KOENIG, Castle-street, Finsbury-square; for certain further improvements in his method of printing by means of machinery. Dec. 24, 1814.

JOHN WHITE, New Compton-street, Soho; for a method of making candles. Dec. 27, 1814.

JOSEPH HARRIS, Shire-lane, Middlesex; for an improvement or improvements in the necessities of clothing used for the military in general. Jan. 4, 1815.

JOHN CATTLER, Great Queen-street, Lincoln's Inn-fields; for certain improvements applicable to fire-places, stoves, &c. Jan. 6, 1815.

CHRISTOPHER DIHL, Brewers-street, Golden-square; for a method or means of making a mastic cement or composition, which he denominates *Dihl's Mastic*. Jan. 6, 1815.

JAMES COLLIER, Grosvenor-street West, Pimlico; for an apparatus, machine, or instrument, intended to be called a *Creopyrite*, by means of which power will be very economically obtained, and advantageously applied to the raising of water, and other useful purposes. Jan. 16, 1815.

FREDERICK Marquis de CHABANNS, Thayer-street, Manchester-square; for a method of extracting from fuel a greater quantity of caloric than hath hitherto been acquired, and applying it to the purpose of warming the room in which the operation is conducted, and also other rooms by one single fire. Jan. 16, 1815.

## ARTICLE XV.

## METEOROLOGICAL TABLE.

1815.	Wind.	BAROMETER.			THERMOMETER.			Evap.	Rain.
		Max.	Min.	Med.	Max.	Min.	Med.		
3d Mo.									
Mar. 2	Var.	30.22	30.17	30.195	48	35	41.5		S
3	N W	30.20	30.17	30.185	49	37	43.0		
4	S W	30.17	30.11	30.140	54	41	47.5		
5	N W	30.17	30.06	30.115	55	40	47.5		
6	W	30.17	29.90	30.035					
7	S W	29.90	29.35	29.625	52	38	45.0		—
8	W	29.50	29.35	29.425	52	32	42.0		.45
9	N W	29.46	29.16	29.310	49	34	41.5	.30	.15
10	W	29.32	29.16	29.240	48	29	38.5		6
11	S W	29.48	29.32	29.400	47	30	38.5		—
12	S	29.32	28.86	29.090	49	35	42.0		.39
13	N W	29.52	28.86	29.190	52	39	45.5		.10
14	S W	29.96	29.52	29.740	49	32	40.5		5
15	Var.	29.96	29.68	29.820	55	37	46.0		.19
16	S W	29.83	29.68	29.755	62	43	52.5		
17	N W	29.97	29.83	29.900	62	42	52.0	.35	
18	N W	29.99	29.88	29.935	55	46	50.5		—
19	N W	29.91	29.88	29.895	57	45	51.0		.13
20	W	29.91	29.75	29.830	60	41	50.5		
21	S W	29.75	29.5	29.645	59	45	52.0		6
22	S W	29.54	29.31	29.425	61	45	53.0		6
23	W	29.42	29.14	29.280	59	40	49.5		.15
24	S W	29.42	29.25	29.335	57	40	48.5	.35	8
25	S W	29.70	29.25	29.475	56	31	43.5		
26	S W	29.70	29.42	29.560	53	46	44.5		—
27	S W	29.46	29.34	29.400	60	49	54.5		.12
28	S W	29.95	29.46	29.705	60	44	52.0		.26
29	S	29.93	29.77	29.850	62	43	52.5		
30	S W	29.93	29.88	29.905	59	43	51.0		
31	Var.	29.88	29.65	29.765	73	46	59.5	.43	
		30.22	28.86	29.672	73	29	47.44	1.43	2.33

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

## REMARKS.

*Third Month.* — 2. Small rain at intervals. 3. Misty morning: fine day. 4, 5. *Cumulostratus*. 6. Fine day: *Cirri* appeared, much elevated, and coloured at sun-set. 7. Fine morning: p.m. cloudy and windy, with some rain: night very stormy. 8. Windy, wet, a.m.: showers by inoculation in the evening. 9. a.m. Hoar frost: turbid sky: rain: p.m. fair. 10. Snow early, after which various modifications of cloud, ending in showers of rain and snow, p.m. 11. Hoar frost: *Cirrostratus* and *Cumulostratus*: p.m. *Nimbi*, with large hail. 12. Dull misty day: at night very stormy, with rain. 13. a.m. Cloudy, with a gale at S. W., and rain at intervals: p.m. several dense *Nimbi*, thunder, hail, and hard rain: much wind, with distant lightning, at night. 14. *Cirrostratus* and haze: then *Cirri*, passing to dense *Nimbi*: gusts of wind, hail, and rain. 15. The barometer has risen, with an almost uniform motion, about an inch and a quarter in 36 hours; yet the air has not become clear: it should be observed, that there had been much previous depression: a wet forenoon, with a breeze at E.: p.m. *Cirrostratus*: at night much wind. 16. a.m. High wind at S. W., with *Cumulostratus*: fair and pleasant. 17. a.m. Much dew: *Cirrostratus*, with *Cirrocumulus*: the light clouds after sun-set beautifully tinted with lake and purple. 18. After a few drops, the *Cumulostratus* prevailed, followed by rain in the night. 19. Some rain, a.m.: then *Cumulostratus*: and at evening *Cirrostratus*, with a lunar corona. 20. a.m. Dew: a light veil of *Cirrostratus*: at evening, the clouds passed to the N. 21. *Cumulus*, beneath *Nimbi*form *Cirrus*, both elevated: about five, p.m. during the approach of a squall, the wind was very noisy among the branches (now covered with opening buds), producing an almost vocal modulation of sound: as soon as the trees became wet, this was exchanged for the usual hoarse noise, resembling that of the sea-shore. It is probable that the former effect requires a peculiar sonorous vibration in the branches, the effect of *class friction* by the air, which the interposition of water does not permit to take place. The night was bolsterous. 22. Much wind: showers: two strata of cloud: borne very high, as for some days past. 23. Heavy squalls, with some hail in the showers: p.m. a singular combination of clouds in the E.: it was a *Nimbus*, with *Cumuli* adhering and entering at the flanks, while a very lofty columnar *Cumulus* shot up through the midst of the crown, and this again was capped with a small *Cirrostratus*. 24. Various clouds: squally, p.m. 25. The same: a brisk evaporation: at sunset, *Cumulus* at a considerable height, inoculated with *Cirrus* above, after which two distinct *Nimbi* in the S., which went away eastward. 26. Driving showers: at evening a lunar corona, followed by much wind and rain at intervals. 27. Stormy: showers. 28. Fair. 29. Large *Cirri*, which passed chiefly to the *Cirrocumulus*, p.m. 30. Misty, a.m.: overcast, p.m.: little wind. 31. A very fine day: large *Cirri* formed alone at a considerable elevation, and passed in the evening to the N. W.: much dew followed.

## RESULTS.

Winds Westerly.

Barometer: Greatest height.....	30.22 inches;
Least .....	28.86 inches;
Mean of the period .....	29.672 inches.
Thermometer: Greatest height .....	73°
Least.....	50°
Mean of the period .....	47.44°

Evaporation, 1.43 inch. Rain, 3.35 inches.

TOTTENHAM, Fourth Month, 1, 1815.

L. HOWARD.

# ANNALS OF PHILOSOPHY.

JUNE, 1815.

## ARTICLE I.

*A Memoir on Iodine.* By M. Gay-Lussac.

(Continued from p. 302.)

### *Hydriodate of Zinc.*

THIS salt is easily obtained by putting iode into water with an excess of zinc, and favouring their action by heat, as I have already explained. I have frequently attempted, but always without success, to make this salt crystallize, because it is extremely deliquescent. Heat first deprives it of its water, then melts it, and sublimes it in fine prismatic crystals, similar to those obtained when antimony is oxidized. It is not decomposed by this operation, if performed in close vessels; but if air be admitted, iodine is disengaged, and oxide of zinc remains. When this hydriodate is dried, it does not differ from ioduret of zinc.

By taking the mean of three experiments, differing very little from each other, I find that ioduret of zinc is composed of

Iodine .....	100
Zinc .....	26·225

Consequently the hydriodate is composed of

Acid .....	100
Oxide of zinc .....	32·352

When a solution of hydriodate of potash or soda is mixed with a solution of metallic oxides, no precipitate is obtained with those of manganese, nickel, and cobalt, which proves that the hydriodates of these metals are soluble. Perhaps we may say that all the com-

binations of iodine with the metals that decompose water possess the same property.

On the contrary, the metals that do not decompose water have given me precipitates with the hydriodate of soda. The precipitate of copper is whitish-grey; that of lead, a fine orange-yellow; that of protoxide of mercury, greenish-yellow; that of peroxide of mercury, orange-red; that of silver, white; that of bismuth, chesnut-brown.

I consider all these precipitates as metallic iodurets, and with so much the greater reason, that the hydriodates of the very oxidable metals are changed into iodurets merely by drying them by a gentle heat. Now the force which has determined the insolubility of all these precipitates ought to be considered as much more energetic than a small change of temperature which is sufficient to convert a hydriodate into an ioduret.

It will not be useless, in order to settle our opinions respecting the nature of the combinations of the metals with sulphur, iodine, and chlorine, when in contact with water, to show the analogy which they have to one another.

Among the sulphurets, those only of the metals, which have a much greater affinity for oxygen than hydrogen has, are soluble in water, and may be considered, with some probability, as hydro-sulphates. Such are those of potassium, sodium, barium, &c.

Though iron and zinc decompose water, they have not so superior an affinity for oxygen above hydrogen, that the united affinities of the metal for oxygen, and the sulphur for hydrogen, are stronger than that of the oxygen for hydrogen, and the metal for sulphur. I neglect here the affinity of the oxide for hydro-sulphuric acid, because it must be very weak relatively to the others. The metals which readily yield their oxygen to hydrogen will form *a fortiori* sulphurets, which will not decompose water, and which will be insoluble in that liquid.

When we compare the iodurets with the sulphurets, we must attend to this circumstance, that iodine has a stronger affinity than sulphur for hydrogen, and that from this there ought to result an augmentation of intensity in the forces which tend to decompose water.\* We see, in fact, that all the metals which give soluble compounds with sulphur form equally soluble ones with iodine; and further, that the iodurets of the metals which decompose water possess the same property. As to the iodurets of the metals, which have less affinity for oxygen than hydrogen, they are insoluble, as well as their sulphurets.

Pursuing the same comparison with the chlorurets, we ought,

\* It may be objected, that if the forces which tend to decompose water have increased because iodine has more affinity than sulphur for hydrogen, those which tend to prevent its decomposition have also increased, because iodine has more affinity than sulphur for potassium and the other metallic bodies. But we may suppose, with sufficient probability, that the first have increased in a greater ratio than the second.

according to the same principles, to find a greater number which are soluble than of the sulphurets and iodurets, because chlorine has a much stronger affinity for hydrogen than sulphur or iodine. This accordingly is the case. All the chlorurets of the metals which form soluble iodurets are likewise soluble; and besides them those of lead, bismuth, gold, platinum, the deutochlorurets of copper and mercury possess the same property.\* We see, then, by the comparison which we have just made, that it is the most oxidable metals, and the radicles that have the greatest affinity for hydrogen, which have the greatest tendency to form combinations soluble in water, and which probably decompose it.

I have attempted to decompose several hydriodates by acids in which the oxygen is very much condensed; but I have not obtained any satisfactory result. The hydriodate of strontian, and that of potash, treated by concentrated phosphoric acid, gave me very deep coloured hydriodic acid. Boracic acid produces no sensible decomposition, because it is too weak as long as there is any water mixed with it; and when there is none the hydriodate is changed into ioduret. Liquid hydro-chloric acid does not decompose the hydriodates, because it is more volatile than hydriodic acid; but in the gaseous state it decomposes the iodurets in an elevated temperature. I passed slowly through a glass tube containing ioduret of potash that had been melted, a current of hydro-chloric gas. There was no decomposition while cold. When the temperature was raised nearly to a red heat, I obtained hydriodic gas containing but very little hydro-chloric gas. With the iodurets of strontium and calcium, the decomposition takes place much better. This method may be employed with advantage in order to procure hydriodic gas.

#### *Iodureted Hydriodates.*

All the hydriodates have the property of dissolving abundance of iodine, and by this they acquire a deep reddish-brown colour. They keep it in solution by a very weak force: for they let it go when boiled, or when exposed to the air after being dried. The iodine does not change the neutral state of the hydriodates; and the reddish-brown colour of the solutions, similar to the other solutions of iodine, is a new proof of the weakness of the combination. We cannot compare these compounds to the sulphureted sulphites, in which the sulphur appears to act the part of an acid. They have rather the characters of a simple solution. I am aware that combi-

\* The protochlorurets of copper and mercury are insoluble, while the deutochlorurets are very soluble. Though we may explain this difference on the hypothesis that chlorurets do not dissolve in water but in as much as they decompose it, these facts seem to me more favourable to the other hypothesis, that the chlorurets may dissolve in water without decomposing it. I have called the first combination of copper and mercury with chlorine *protochloruret*, because it corresponds with their protoxides; and the second *deutochloruret*, because it corresponds with the second degree of their oxidation.

nation and solution depend upon the same force, and that it is difficult to draw a limit between them. But we may distinguish them from each other by defining a solution to be a combination in which there is no saturation of properties. At any rate, there is no inconvenience in adopting the expression *iodureted hydriodate* to denote the combination of iodine with a hydriodate, provided we entertain an exact idea of that combination.

### *Of the Iodates.*

When speaking of the action of the alkaline oxides on iodine by means of water, we have shown that there form at the same time a hydriodate and an iodate, and we pointed out the method of separating them from each other. We may obtain the iodates of the other oxides either by double decomposition, or by saturating directly iodic acid, or the acid liquid from chloruret of iodine, which we have considered as a mixture of iodic and hydro-chloric acids, and which, at any rate, possesses exactly the properties of such a mixture.

There are but very few of the iodates that fuse on burning coals; that of ammonia is fulminating.

They are all soluble in hydro-chloric acid, with the disengagement of chlorine. The solution contains subchloruret of iodine.

Sulphurous and hydro-sulphuric acids decompose them, and separate the iodine. Chlorine does not decompose them. Sulphuric, nitric, and phosphoric acids, have no action on them at the ordinary temperature, except as far as they seize a portion of their base.

At a faint red heat all the iodates are decomposed. Some give oxygen only; others, oxygen and iodine.

They are all insoluble in alcohol of the sp. gr. 0.82.

### *Iodate of Potash.*

I have only obtained it in small crystals, which have nearly the cubic form. It deflagrates on burning coals like nitre. It is not altered by exposure to the air. 100 parts of water at 58° dissolve 7.43 of this salt. It is decomposed at a temperature somewhat higher than is required by the chlorates. Oxygen is disengaged, and ioduret of potassium remains, which forms a neutral solution in water. It is easy to see that the residue is ioduret of potassium. If the metal were in the state of an oxide, iodate and hydriodate of potash would be formed on dissolving it in water, and sulphurous acid would occasion a precipitate of iodine. If, then, we wish to obtain a pure hydriodate by treating iodine with potash, we must evaporate the solution to dryness, and melt the residue. On redissolving it in water, we are certain to have only hydriodate; but there will always be an excess of base.

From various experiments on the decomposition of iodate of potash by heat, I have found it composed of

Oxygen .....	22.59
Ioduret of potassium .....	77.41
	<hr/>
	100.00

But from the composition of ioduret of zinc given above, namely, iodine 100, zinc 26.225, and admitting, according to my experiments, that 100 zinc combine with 24.41 oxygen, and according to Berzelius, that 100 potassium unite with 20.425 oxygen in order to be converted into potash, we find that ioduret of potassium is composed of

Iodine .....	100
Potassium .....	31.342

According to this proportion, the 77.41 of ioduret of potassium are composed of

Iodine .....	58.937
Potassium .....	18.473
	<hr/>
	77.410

The 18.473 potassium take 3.773 oxygen to be converted into potash. There remain, then,  $22.59 - 3.773 = 18.817$  to acidify the iodine; so that iodic acid is composed of

Iodine .....	100
Oxygen .....	31.927

The ratio of oxygen to iodine is, then, 10 to 31.321. If we multiply this last number by 5, we have the ratio 10 to 156.605, which differs but little from the ratio formerly obtained, 10 to 156.21, from the hydriodate of zinc.

If we convert the iodine and oxygen into volumes, we find that iodic acid is composed of

Iodine .....	1
Oxygen .....	2.5

It is now easy to determine how much ioduret of potassium we ought to obtain, relatively to the iodate of potash which is formed at the same time, when iodine is dissolved in potash. Since 100 iodate of potash contain 22.59 oxygen, of which 3.773 belong to the potash, it is evident that the remainder 18.817 which belong to the iodine has been furnished either by the potash, which after giving it out has formed ioduret of potassium, or, which comes to the same thing, by the water the hydrogen of which formed hydriodate of potash; but 18.817 oxygen correspond with 92.127 potassium, and this last with 298.940 iodine. Therefore, for every 100 of iodate of potash there are formed 386.067 of ioduret of potassium; that is to say, five times as much as the iodate would give by its decomposition. This ratio may be deduced immediately

from the ratio of the oxygen of the iodine to that of the potassium; for  $\frac{18.817}{3.773} = 5$  very nearly.

If we wish to have the quantity of hydriodate of potash which corresponds to 100 of iodate, we must add to the 92.127 potassium 18.817 oxygen, and to the 293.940 iodine 2.497 hydrogen, which saturate the oxygen of the potassium. Thus we obtain 407.381 of hydriodate.

#### *Iodate of Soda.*

The iodate of soda crystallizes in small prisms, usually united in tufts. I have likewise obtained them in small grains seemingly cubic. They deflagrate on burning coals like nitre, at a heat somewhat below a red heat; but at the same time, a very small quantity of iodine is disengaged. On this account the solution of the residual ioduret of sodium in water is slightly alkaline. 100 parts of water at 58° dissolve 7.3 of this salt. It contains no water of crystallization, and is not altered by exposure to the air. When decomposed by heat, it gives out 24.45 of oxygen, a result which does not sensibly differ from the 24.43 of oxygen deduced from the proportions of iodate of potash, and from the ratio of the oxidation of potassium and sodium. I shall adopt this last number in preference, and conclude from it that iodate of soda is composed of

Oxygen .....	24.432
Ioduret of sodium .....	75.568
	<hr/>
	100.000

On putting iodine into a solution of soda, so that the liquid was upon the point of becoming coloured, I obtained by evaporation fine crystals in six-sided prisms, with bases perpendicular to the axis, very alkaline, and deflagrating strongly upon burning coals. These crystals are very soluble, and contain a great deal of water of crystallization. As they were formed in the midst of a liquid containing hydriodate of soda, chlorine produces in them a precipitate of iodine. I consider them as a subiodate of soda. On adding soda to a neutral solution of iodate of soda, I converted the whole into crystals; but instead of being bulky, like the preceding ones, they were in small silky needles, united in tufts. They were not altered by exposure to the air, though they were very alkaline. Potash forms likewise an iodate with excess of base, and crystallizable. In general, the iodic and hydriodic acids have a great tendency to form subsalts.

The iodates of potash and soda detonate by percussion, but very feebly, when they are mixed with sulphur. It might be thought that they could be employed with advantage in the manufacture of gunpowder. But a very simple calculation will show that nitre is much preferable. 100 parts of nitre, when decomposed by heat, give 53.62 of gas, while 100 of iodate give only 22.55. Therefore, neglecting here the difference of density of steam and oxygen, and

observing that oxygen in the detonation of gunpowder is chiefly employed in the production of carbonic acid gas, the volume of which is equal to its own, nitre has the advantage, every thing else being equal, of giving 2·3 times as much gas as it. It is possible, however, that the detonation of powder made with the iodate may be more rapid than that of powder made with nitre.

#### *Iodate of Ammonia.*

We can only obtain this salt by saturating with ammonia iodic acid, or the solution of chloruret of iodine. It is in small granular crystals, the shape of which I have not been able to ascertain. When thrown upon burning coals, or upon a hot body, it detonates with a hissing noise, giving out a weak violet light and vapours of iodine. I endeavoured to decompose it by heat in a glass tube, but it broke the apparatus. I collected, however, enough of the gas evolved to ascertain that it was a mixture of oxygen and azote. When we calculate its composition from the preceding data, we find it composed of:

Iodic acid .....	100·00
Ammonia .....	10·94

But 100 iodic acid contain 75·80 of iodine; and if we reduce this quantity to volumes by dividing 75·80 by 8·6195, the specific gravity of iodine in vapour we find 8·794. If we divide 10·94 by 0·59669, the specific gravity of ammonia we find 17·587, which is the double of 8·794. Hence iodate of ammonia is composed in volume

Ammoniacal gas .....	2
Vapour of iodine .....	1
Oxygen gas .....	2·5

The two volumes of ammoniacal gas give, when they are decomposed, one volume of azote and three of hydrogen. The hydrogen requires for its saturation 1·5 oxygen. There then remains one volume of oxygen, or a bulk equal to that of the azote. On detonating iodate of ammonia, I have obtained the two gases in nearly that proportion.

#### *Iodate of Barytes.*

We obtain it very easily, either by double affinities, or by putting iodine into barytes-water. It precipitates in powder, which is obtained pure after being several times washed. This salt, while drying, concretes into lumps, and becomes mealy. Though kept for a long time in the temperature of 212°, it gives out water before being decomposed by heat; so that it seems to retain water in combination. The products of its decomposition are oxygen, iodine, and barytes, sensibly pure. This barytes dissolves very slowly in water; and as it retains no iodine, though that which is obtained by the decomposition of the nitrate combine with it, I presume that the

difference proceeds from the water which the iodate contained being retained by it. Iodate of barytes is the least soluble of all the alkaline iodates. 100 parts of water dissolve only 0.16 at  $212^{\circ}$ , and 0.03 at  $64.5$ . It is composed of

Iodic acid .....	100
Barytes .....	46.340

It does not deflagrate on burning coals, but only gives out now and then a weak light. The difference in this respect between iodate of potash and iodate of barytes depends on two causes: first, the barytes not being reduced like the potash by iodine, it gives out less oxygen than the iodate of potash; but the second, and principal cause, is, that the iodate of barytes and the product of its decomposition being infusible, only a very small portion of the iodate comes in contact with the coal, and all the rest of the salt is decomposed, without its oxygen serving to the combustion at all. The iodate of potash, on the contrary, being fusible, as likewise the ioduret of potassium, all the parts of the salt come successively in contact with the coals, and support the combustion in a powerful manner. Among the sulphates, we shall find several that detonate, if that name is to be given to those which are decomposed by fire and give out oxygen. This is the case with alum, and with sulphate of zinc. They do not support combustion, for the same reasons that iodate of barytes does not.

Iodate of strontian is obtained like the preceding salt. It is in small crystals, which viewed through a glass appear to be octahedrons. It gives out water before it is decomposed by heat, and the products which it gives out are perfectly similar to those of iodate of barytes. 100 parts of water dissolve 0.73 of it at  $212^{\circ}$ , and 0.24 at  $59^{\circ}$ .

Iodate of lime is usually pulverulent; but it may be crystallized in the hydro-chlorate or hydriodate of lime, which augment its solubility. It then assumes the form of small quadrangular prisms. 100 parts of water dissolve 0.98 of it at  $212^{\circ}$ , and 0.22 at  $64^{\circ}$ . The quantity of water which it holds in combination appears to me to amount to about three per cent. The products of its decomposition by fire are the same as those of the iodates of barytes and strontian. These three salts require a higher temperature for their decomposition than the iodate of potash.

To obtain the other iodates I employed double decompositions. Nitrate of silver gives with the iodate of potash, and even with iodic acid, a white precipitate, very soluble in ammonia. If we saturate the alkali with sulphurous acid, the precipitate appears again; but it has lost its solubility in ammonia, because it has been changed into ioduret of silver. This experiment furnishes us with the means of distinguishing in a combination hydro-chloric, hydriodic, and iodic acids, and of separating them from each other. If we treat the precipitates obtained by nitrate of silver with ammonia, those containing iodic acid, or hydro-chloric acid, will be dis-

solved. If we saturate the solution with sulphurous acid, and treat the precipitate again with ammonia, the chloruret of silver will be alone dissolved.

On digesting in iodic acid oxide of zinc recently precipitated and well washed, I obtained a pulverulent salt, but little soluble in water, which deflagrates on burning coals, but much more feebly than iodate of potash. We may obtain the same salt by mixing a solution of sulphate of zinc with that of a soluble iodate. No precipitate appears at first; but after some hours, small crystals are deposited, sometimes in grains perfectly spherical, which are iodate of zinc. It is necessary for success in this experiment that the sulphate should not be very much concentrated, for its viscosity would oppose the motions of the molecules, and of course the formation and separation of the iodate of zinc.

The solutions of lead, of pronitrate of mercury, of pernitrate of iron, of bismuth, and copper, give with the iodate of potash white precipitates soluble in acids. The solutions of peroxide of mercury and of manganese were not altered.

There do not exist any iodureted iodates; at least I have not been able to form them. Iodates and iodic acid do not dissolve more iodine than water.

To complete the history of the saline combinations of iodine, it remains to be determined whether, when a base acts upon this substance in water, the two salts which may be obtained exist separately in the solution, or if they are formed at the instant that any cause whatever determines their separation.

If we mix together the neutral solutions of iodate and hydriodate of potash, they do not mutually decompose each other; but if we add any acid whatever to the mixture, even carbonic acid, which is driven from all its combinations by hydriodic acid and iodic acid, iodine is precipitated, owing to the mutual decomposition of the two salts.

To render the solution of iodate and hydriodate entirely the same with that which we obtain in making iodine, potash, and water, act upon each other, and which is always alkaline, it is sufficient to add to the former the quantity of potash necessary to bring it to the same degree of alkalinity. In that case we can no longer distinguish the one from the other. It would appear, then, that the iodate and hydriodate of potash are formed at the instant that the iodine acts on the alkali in water; but that the oxygen of the iodic acid and the hydrogen of the hydriodic acid preserve a great tendency to combine together, and that it is sufficient to favour this tendency in order to cause it to take effect. Iodic and hydriodic acids, and in general all acids produced at once by the two elements of water, destroy each other when mixed together.\* This is the

\* M. Berthollet has observed that sulphurous acid and hydro-sulphuric acid may exist together when dissolved in a great deal of water. The same thing holds with the two acids of iodine, which if concentrated give a copious precipitate of iodine as soon as mixed, but do not decompose each other when diluted.

reason why a very weak acid precipitates iodine from a mixture of iodate and hydriodate; for how weak soever its action should be, it will always decompose a small portion of each salt, as Berthollet has shown, and the acids separated producing immediately iodine which precipitates, the decomposition may continue, and make great progress, without, however, becoming complete.

The solution of neutral iodate and hydriodate, and the solution of iodine in potash, present, however, this remarkable difference, that the first always preserves its neutral state, while the second is constantly alkaline; but if the two salts be really formed at the instant iodine is mixed with solution of potash, it would appear that we ought either to obtain a complete saturation of the alkali, or that the mixture of neutral iodate and hydriodate ought to become alkaline as soon as it is made. The reason why this is not the case is, that we cannot suppose that in a mixture of various bodies each element acts rigorously, as if it were merely mixed, and as if the mobility of its particles were perfect. We ought rather to admit, that to destroy a compound stronger forces are necessary than those that formed it. On this supposition it is easy to conceive that the alkalinity of the solution of iodine in potash, and the constant neutrality of a mixture of neutral iodate and hydriodate, may take place at the same time; and of consequence, that the two salts of which we have been speaking may be formed, and exist separately, in a solution of iodine and potash.

#### *Hydriodic Ether.*

I have paid but little attention to the action of iodine on animal and vegetable substances; yet it may produce several new combinations. MM. Colin and Gaultier have described that which iodine forms with starch; and I shall now make known an ether formed by hydriodic acid and alcohol.

I mixed together two parts in volume of absolute alcohol and coloured hydriodic acid of the specific gravity 1.700. I then distilled the mixture in a water-bath. I obtained an alcoholic liquid, perfectly neutral, colourless, and limpid, which, when mixed with water, became muddy, and let fall in small globules a liquid at first milky, but which gradually became transparent. This liquid is hydriodic ether. What remained in the retort was very dark coloured hydriodic acid. Thus in this experiment a portion of the alcohol had united with the hydriodic acid, and formed an ether, which distilled over with the rest of the alcohol, and there remained hydriodic acid very dark coloured, because it held in solution all the iodine which had coloured it at first. It was probably the action of the iodine and the water which prevented the whole hydriodic acid from combining with the alcohol.

Hydriodic ether, after having been washed two or three times with water, in which it is but very little soluble, is perfectly neutral. Its odour is strong, and, though peculiar, is analogous to that of the other ethers. After some days it acquires a red colour, which does

not afterwards increase in intensity. Potash and mercury destroy this colour on the spot by uniting with the iodine to which it is owing. Its specific gravity at  $72.5^{\circ}$  is 1.9206. It boils (as determined by its tension) at  $148.6^{\circ}$ . By direct experiment, I found it to boil at  $148.1^{\circ}$ . It is not inflammable; it exhales only purple vapours when thrown upon burning coals. Potassium may be preserved in it without alteration. Potash produces no immediate alteration on it. The same is the case with nitric and sulphurous acids and chlorine. Concentric sulphuric acid renders it speedily brown. When passed through a red-hot tube, it is decomposed. I obtained an inflammable carbureted gas, very brown hydriodic acid, and a little charcoal. Besides these products, I obtained, by putting a solution of potash into the tube in which I produced the decomposition, a substance in flocks which refused to dissolve in the alkali and in acids. This substance, after being several times washed in cold water, preserved an ethereal odour, but not so strong as that of the liquid ether. In boiling water the flocks united together, and melted into a matter, which, after being cooled, resembled white wax in appearance. When put upon a burning coal, it gave out without flaming vapour of iodine in greater abundance than the hydriodic ether. It volatilizes, but much more slowly than the ether.

From these properties I consider the substance of which I have been just speaking as a peculiar ether, formed without doubt by the combination of hydriodic acid with a vegetable matter different from alcohol. I have not analyzed hydriodic ether; but on comparing it with hydro-chloric ether, which, according to Thenard, is composed of a volume of hydro-chloric gas and half a volume of pure alcoholic vapour, I consider its composition as analogous. According to this hypothesis, hydriodic ether is composed in volume of

Hydriodic gas .....	1
Alcoholic vapour .....	0.5
or in weight, of	
Acid .....	100
Alcohol .....	18.55
and hydro-chloric ether, of	
Acid .....	100
Alcohol .....	64.67

Supposing this composition correct, it is astonishing that hydriodic ether is not inflammable, while hydro-chloric ether possesses that property in a great degree. We cannot suppose that this difference depends on the proportions in weight of the acids to alcohol. I rather think that hydriodic ether is not inflammable, because its acid is decomposed by oxygen without producing flame, and hence the oxygen becomes too small in quantity to support the combustion of the alcohol. It would be easy to verify this conjecture by burning

the ether in oxygen gas; for if it is well founded, the combustion in this gas should be attended with flame.

On calling to mind the different experiments in this memoir, we shall see that there is not one which authorizes us to consider iodine as a compound body, or as a substance containing oxygen. On the contrary, we must be struck with the resemblance which it bears in some cases to sulphur, and in others to chlorine. Like them, it forms two acids, one by combining with oxygen, the other by combining with hydrogen: and we may have remarked that the acids formed at once by the combination of chlorine, iodine, and sulphur, with the elements of water, present this remarkable property, that when the acid formed by oxygen has its elements very condensed, that formed by hydrogen has them very weakly united.

Thus sulphur takes oxygen from iodine, and iodine takes it from chlorine; but, on the other hand, chlorine takes hydrogen from iodine, and iodine from sulphur.

If we follow this analogy still further, we find that it holds likewise with respect to carbon; for sulphur deprives it of hydrogen, but yields to it oxygen. Thus it would appear that the more a body condenses oxygen, the less it condenses hydrogen.\* This is, without doubt, a cause why the very oxidable metals, as iron, manganese, &c. do not dissolve in hydrogen. I say one of the causes; for if it were the only one, we could not see how mercury, silver, and gold, do not combine with hydrogen, though they have a very weak affinity for oxygen.†

The other analogies which iodine has with sulphur and chlorine are very numerous. Some iodates resemble the chlorates exactly; but most of them have a greater analogy with the sulphates. The iodurets, sulphurets, and chlorurets, in general exhibit the same phenomena with water; and the action of sulphur, iodine, and chlorine, upon the oxides, with or without water, is exactly similar. In short, all the properties of iodine may be classed between those of sulphur and chlorine. It is not necessary to remark, that though

\* From these considerations I do not hesitate to class azote with oxygen, iodine, chlorine, and sulphur. Nitric acid has a considerable resemblance to iodic acid and chloric acid by the property which it has of being easily decomposed; and because azote takes, like chlorine and iodine, two and a half times its volume of oxygen. The nitrates are decomposed by the fire, like the iodates. But we do not know any oxide from which azote disengages oxygen, from which we can conclude only that it has much less energy than this last body. Azote forms likewise with chlorine and iodine combinations which are easily decomposed, which shows that it has little affinity for them, and that it approaches them by the nature of its energy. If it does not form an acid with hydrogen, this is doubtless because in ammonia there are three volumes of hydrogen for one of azote; and in all probability, in order to produce an acid, only equal volumes are requisite. The acid combination of azote and hydrogen appears to me realized in prussic acid, which, from some experiments that I have made, and shall soon publish, I am induced to consider as an acid analogous to the combinations of chlorine, iodine, and sulphur, with hydrogen; only that its radicle is a compound of azote and carbon. Oxygenated prussic acid corresponds to chloric and iodic acids.

† See note B at the end of this memoir.

I have restricted myself to compare iodine with sulphur and chlorine, we might find analogies, though less numerous indeed, between it and phosphorus, and several other bodies; but I thought it better to compare it only with those bodies to which it bears the greatest resemblance, and among which I conceive it ought to be classed. I have been led by this to show that sulphur possesses all the general properties of chlorine, and that we ought in consequence to place it among those bodies which form acids by combining with hydrogen.

(To be continued.)

## ARTICLE II.

*Analysis of the Excrements of the Boa Constrictor.* By Dr. Prout.

THE substance sent me as the excrements of the boa constrictor was solid, of a white colour inclining to yellow. Fracture earthy. When rubbed on a hard surface it left a white mark like chalk. Its feel, however, was rather more dry and harsh than that of chalk, and it was more friable than that substance. Smell faint and mawkish. Sp. gr. 1385.

I.—A. After making a few general experiments to ascertain its nature and composition, 50 grains, well dried and in a state of fine powder, were digested for 24 hours in dilute muriatic acid; the mixture being occasionally, during that time, gently heated and well shaken. On adding the muriatic acid no effervescence was perceived. The acid was then poured off, and in order to remove the whole of it, the substance was repeatedly well washed with distilled water, which washings were added to the acid. It was now carefully dried and weighed, and was found to have lost 4.92 grs., which, of course, were taken up by the muriatic acid, and remained in solution in it.

B. On the substance which had been left undissolved by the acid, and which had become of a bluish tinge, a solution of pure potash was poured, which, on the application of heat, completely dissolved it, forming a solution transparent and nearly colourless. To this solution, while still warm, was added muriatic acid in excess, which occasioned a copious white precipitate in the state of very minute but distinct grains, which, after having been well washed, was collected and dried. The following were its properties. It existed, as before stated, under the form of very minute grains of a beautiful bluish white pearly appearance. No smell. Nearly insipid. Very sparingly soluble in cold water; but rather more soluble in warm, and its solution faintly reddened litmus. Insoluble in alcohol. It combined with all the alkalis, earths, &c., forming compounds very little soluble in water. When nitric acid was poured upon it and heat applied, it was ex-

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tirely dissolved; the solution, after some time, acquired a beautiful deep rose or crimson colour, which stained the skin and other animal matters of the same tinge. The colour, however, of the solution, after some time, vanished irrecoverably. In short, it had all the properties of *uric acid* in the greatest state of purity. Its quantity, as before stated, was  $50 - 4.92 = 45.08$ ; that is to say, 100 pts. of the excrement contained 90.16 of uric acid.

C. The dilute muriatic acid (A) which had acquired a yellowish colour was divided into three equal portions:—

1. To the first ammonia was added, so as just to neutralize the acid. This occasioned a very faint cloudiness. Carbonate of ammonia was now added, which produced no further precipitation.

2. To the second portion ammonia was added as before; but instead of afterwards adding carbonate of ammonia, the oxalate of ammonia was substituted, which produced a copious white precipitate.

3. The third portion of the dilute muriatic acid was carefully evaporated to dryness. The residuum, after having been accurately weighed, was exposed to a strong heat over a lamp. White dense fumes arose in great abundance, which were muriate of ammonia. After these had ceased to appear, the residuum was again weighed; and in a mean of two experiments, it was found to have lost .9 gr.

Now as  $\frac{50}{3} : .9 :: 100 : 5.4$  parts of muriate of ammonia in 100 parts which contained 1.7 of pure ammonia.

II.—A. From I. c. 2, it appeared that the muriatic acid held some lime in solution, but that it was prevented from being precipitated by the carbonate of ammonia (I. c. 1.) on account of the presence of animal matter, which was in a state of intimate union with it. To determine, therefore, the quantity of this lime more particularly, and also whether any other saline matters were present, 20 grs. of the excrements were burnt in a platinum crucible. The residuum was a small quantity of saline and earthy matters, which weighed 1.36 gr. On this residuum was poured a little distilled water, and heat applied. The water took up the alkaline salts, and was then poured off, and evaporated to dryness. These salts, in a mean of two experiments, weighed 1.20 gr. Acetic acid, on being poured on them, produced effervescence. After this addition, they were again dried, and digested in alcohol. This took up the acetate formed, which, on being decomposed at a red heat, left a quantity of subcarbonate of potash, which weighed 1.01 gr. Now  $1.01 \times 5 = 5.05$  subcarbonate of potash in 100 parts, equal to about 3.45 parts of pure potash.

B. The residuum left by the alcohol was neutral, and weighed .19 gr. It was found to consist chiefly of sulphate of potash, with a trace of a muriate, probably of soda. Now  $.19 \times 5 = .95$  parts of this mixture in 100 parts of the excrements.

C. Muriatic acid was now added to the residuum (II. A.) not taken up by the water, which dissolved the whole of it. To this

solution was added pure ammonia. A precipitate fell, which was phosphate of lime. On adding carbonate of ammonia, there was a slight precipitate of carbonate of lime. After this phosphate of soda produced a very distinct crystalline precipitate, indicating the presence of magnesia. The quantities of these were too small to be weighed separately, but altogether their weight must have been  $\cdot 16$  gr. for  $1\cdot 36 - 1\cdot 20 = \cdot 16$ . Now  $\cdot 16 \times 5 = \cdot 8$ , the quantity of these substances in 100 parts.

D. Lastly, the quantity of animal matter, independently of the uric acid, destroyed by combustion, was estimated at 2·94 per cent. For  $100 - 90\cdot 16 + 1\cdot 7 + 3\cdot 45 + \cdot 95 + \cdot 8 = 2\cdot 94$ .

Hence 100 parts of these excrements contained

Uric acid .....	90·16
Potash .....	3·45
Ammonia .....	1·70
Sulphate of potash, with a trace of } muriate of soda ? .....	·95
Phosphate of lime .....	
Carbonate of lime .....	·80
Magnesia .....	
Animal matter, consisting of mucus } and a little colouring matter. ....	2·94

100·

*Observations.*—I. c. 2. Oxalate of ammonia will often throw down lime when in a state of union with animal matter, together with the animal matter itself, when other tests will scarcely indicate its presence. This may be verified by almost any secretion from a mucous membrane, most of which contain lime united to an animal matter. For this interesting observation I am indebted to Mr. Wilson. The lime, therefore, in the present instance, was probably in union as an aliment with an animal matter analogous to mucus, and which was doubtless derived from the internal passages of the animal. It was insoluble in a weak alkaline solution, and hence could easily be separated by its means. The acid also, as before observed, took up a small quantity of a yellow colouring matter. The quantity of these, however, as estimated above, was probably a little too great, since a small portion of the uric acid was doubtless removed by the repeated washings, which of course caused these matters to appear more abundant than they really were.

I. c. 3. It is extremely difficult to ascertain the quantity of ammonia contained in a substance. I had before observed that this alkali was present, and the above mode occurred to me of estimating its quantity. Perhaps it is not very far from the truth, though it can hardly be relied upon as indicating the exact amount.

II.—A. The alkaline matter present was considered as potash, on account of the facility with which its subcarbonate and acetate attracted moisture from the air, and also from its precipitating

416 *Analysis of the Excrements of the Boa Constrictor.* [JUNE, muriate of platinum. This alkali, as well as the ammonia, I considered as combined with the uric acid, because no effervescence was produced on the addition of muriatic acid.

I obtained these excrements from Mr. Lean, jun. and on ascertaining their composition, went to see the animal said to be the boa constrictor, exhibited near Exeter Change, in order to learn a few particulars of its mode of feeding, &c. This animal is a young one, and, according to the keeper's statement, about 16 feet long. It is fed about once a month only, when a living rabbit, or sometimes two small ones, are exposed within its reach, which, after some time, it seizes, destroys, and swallows, in the manner peculiar to these serpents. The excrements are discharged at periods of about 8 or 10 days, in a softish state, about the consistence of stiff dough; but soon become hard and friable on exposure to the air. They are in the form of roundish scybalæ, and on their surface there is often a yellowish substance, sometimes in a crystallized state, which crystals I have found to be urate of ammonia, tinged with a little colouring matter. These are the only kind of fæces, or excrements, observed to come from the animal. I obtained a portion of them a few hours after they had been discharged, which had all the characters and properties of those above described, which I received from Mr. Lean.

As far as I know, uric acid has not been observed to be formed by this class of animals. Dr. Wollaston has found that birds produce more of this principle when fed on animal matters; and Fourcroy, Vauquelin, and Klaproth, found a large proportion of it in the excrements of a bird called guano, brought from the South Sea Islands.\* It is certainly a very extraordinary thing, that nearly the whole of the food taken by this animal should be converted into this substance. May it not proceed from disease arising from the unnatural state in which it is kept?

#### *Liquor Amnii of a Cow.*

This had been taken from the uterus of an animal slaughtered in an early period of her gestation. It was of a yellowish colour, and had the appearance of very minute shining particles floating in it. Smell fragrant, something like that of new milk or butter. Taste bland and sweetish, like fresh whey. Foamed a good deal when shaken. Did not affect litmus or turmeric papers. Sp. gr. 1.013.

My attention was particularly directed to the principle found in this fluid by Vauquelin and Buniva, and called by them *amniotic acid*. I could not, however, discover the least traces of a similar principle; but it contained a very sensible quantity of the sugar of milk, which separated in crystals from it, when it had been concentrated by evaporation. It coagulated partially by heat, and

\* I quote from Berzelius's View of the Progress and present State of Animal Chemistry. P. 103. 108.

some flakes fell, leaving the liquor nearly transparent and colourless. Acetic acid produced no coagulation, and this acid, moreover, prevented its coagulation by heat; hence it contained albumen. Muriate of barytes produced a very copious white precipitate.

From experiments, which it would be unnecessary to detail here, it was found that 1000 parts contained

Water .....	977
Albumen .....	2.6
Substances soluble in alcohol .....	16.6
Substances soluble in water, chiefly sulphate of } soda? and other salts. Also sugar of milk .. }	3.8
	<hr/> 1000.0

The principles soluble in alcohol were of a brown colour, and seemed to consist in part of the lactates, &c., as usual; but chiefly of a peculiar substance quite different from every other that I had examined, and which has a considerable resemblance in its sensible properties to the external brown parts of roasted veal.

My want of leisure prevented me from examining this fluid more particularly. It may not, however, be improper to observe, that the liquor amnii described by Vauquelin and Buniva differed very considerably from the above in its sensible qualities, as well as in its chemical ones. This dissimilarity probably arose from the different circumstances under which it was obtained, theirs, most likely, having been procured at the full period of gestation. These chemists, however, describe a principle obtained by them, soluble in alcohol, and having peculiar properties, which, though it appeared to resemble in some respects that above mentioned, differed from it in others. See *Ann. de Chim.* N° 99. *Johnson's Animal Chemistry*, vol. i. p. 274. *Dr. Thomson's System of Chemistry*, vol. v.

### ARTICLE III.

#### *On the colouring Matter, or Ink, ejected by the Cuttle Fish.*

By Dr. Prout.

THE substance, of which an account is given below, was sent me in the original cyst in a perfectly dry state. The following were its properties.

It was hard and brittle. Fracture imperfectly conchoidal. When solid, of a brownish black colour, and exhibiting a slight pavonine lustre, on exposure to a strong light. In a state of powder, of a beautiful velvet black. No smell. Taste rather saltish. Sp. gr. in powder, 1.640.

A. 25 grains were digested in distilled water, and allowed to

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subside in it spontaneously, which took nearly a week to effect, so very slow was the process. This aqueous solution was brownish. It was divided into three equal parts, two of which were spent in ascertaining the effects of the following agents.

- 1, 2. *a.* Heat. This produced no apparent change.
- b.* Muriatic acid with heat. No apparent change.
- c.* Acetic acid produced a faint opacity not increased by heat. Prussiate of potash added to this solution produced no precipitate.
- d.* Oxymuriate of mercury produced a very faint precipitate after some time.
- e.* Subacetate of lead, no precipitate.
- f.* Nitrate of lead, slight brown flaky precipitate.
- g.* Infusion of galls, no precipitate.

Hence it contained no albuminous matter; but a small portion only of a principle analogous to mucus.

3. The remaining portion was evaporated to dryness in a glass capsule. It left a residuum, which, when strongly dried, weighed .25 grs. Distilled water was poured on this residuum, which took up some neutral saline matter which proved to be a muriate, with a little sulphate, most probably of soda, though the quantity was so small that I could not pretend to determine this. These saline matters weighed about 0.18 gr. The rest was animal matter, apparently analogous, as before observed, to mucus, and probably derived from the membrane being the cyst in which it was contained. Now as  $\frac{1}{2} : .18 :: 100 : 2.16$  parts of these salts in 100 parts; and as  $\frac{1}{2} : .7 :: 100 : .84$  parts of animal matter in ditto.

B. Diluted muriatic acid was now poured on the black matter, and digested with it for some time. To this dilute acid, which was perfectly transparent and colourless, was added pure ammonia. This occasioned no precipitate. Neutral carbonate of ammonia was now added, which produced a very copious white precipitate. This was carbonate of lime. It weighed 2.6 grains. Now  $2.6 \times 4 = 10.4$  parts per cent.

C. To the above solution was next added phosphate of soda. This occasioned a very copious precipitate of the triple phosphate of magnesia and ammonia. It weighed 6 grains = 24 per cent. Now this contained 3.3\* of magnesia, which combined with 3.7 of carbonic acid, formed 7 of carbonate of magnesia.

\* This is founded upon the supposition that the triple sulphate of magnesia and ammonia is composed of one atom of phosphate of magnesia, one atom of phosphate of ammonia, and five atoms of water. Such a supposition will constitute 100 parts of this triple phosphate, according to Dr. Wollaston's scale, as about

Phosphate of magnesia.....	35
Phosphate of ammonia.....	33
Water .....	32

100

Now, according to Fourcroy, it is composed of equal weights of each, which comes very near the above.

D. The black residuum was now carefully dried and weighed, in order to constitute a check to the above. Its weight was 19.5 grains = 78 per cent., indicating a loss of 1.60 in the above-mentioned processes. This is considered as the pure colouring principle. It was of a fine full black colour, and possessed the shining appearance of powdered charcoal. It was insoluble in the muriatic and sulphuric acids, even when assisted by heat. Also in the acetic. Concentrated nitric acid acted on it readily, and with considerable energy, abundance of red fumes being emitted; and at length a partial solution, being formed of a very deep reddish brown colour. A solution of pure potash added to this solution produced no precipitate; but a solution of the subcarbonate of potash produced a slight one. A solution of caustic potash, assisted by heat, likewise effected a partial solution of this substance. Also caustic ammonia in a slighter degree. The colour of these solutions was of a deeper brown than that in nitric acid. The muriatic and sulphuric acids produced a slight precipitate when added to this alkaline solution, but not the nitric acid.

It burnt, without melting, with considerable difficulty, emitting the usual smell of burning animal matters, somewhat modified by a fishy odour. It left a very minute portion of reddish ashes, which proved to be a mixture of red oxide of iron, lime, and magnesia, the quantities in the order mentioned, that of the oxide of iron being greatest. Hence 100 parts of this substance contained

Peculiar black colouring matter .....	78.00
Carbonate of lime .....	10.40
Carbonate of magnesia .....	7.00
Muriate of soda? .....	} 2.16
Sulphate of soda? .....	
Animal matter analogous to mucus .....	84
Loss, .....	1.60
	<hr/> 100.00

The carbonates of magnesia and lime, from their being so readily extracted almost entirely by the muriatic acid, appear to have been in a state of mechanical mixture only in this substance. The iron undoubtedly formed a component part of it, as it does in the colouring matter of the blood. The quantity I possessed was too small to enable me to make the investigation so complete as could be wished; though, from what has been said, it will appear to be chiefly characterized by its negative properties.

Mr. G. Kemp\* has made some experiments on this substance in its recent state. He appears to have considered it as consisting of, or at least containing, albumen; but apparently without any very good reason. It appears to me, that all the effects of coagulation, precipitation by alcohol, mineral acids, metalline solutions, &c.,

\* Nich. Journal, vol. xxxiv. p. 24.

described by him, did not depend upon albumen, but upon a species of mucus, which, probably, had he tried it, would as readily have been precipitated by acetic acid and heat, as by any of the mineral acids. If albumen had really existed in it, I see no reason why I should not have met with, at least, traces of it in the aqueous solution, (A) since it is well known that albumen may be dried at a low temperature, without injuring its properties of dissolving in water, or, as far as I know, any of its properties.\* I cannot say, indeed, how the specimen I obtained was dried; but it had the appearance of having been dried spontaneously by simple exposure to the air. The properties of the colouring matter, as described by Mr. Kemp, do not differ materially from those above described.

After all, however, it would be desirable to examine this substance in its recent state, as it appears to have undergone some changes in drying.

This substance, from the length of time which it takes to subside in water, appears admirably contrived for the purpose of concealing the animal from his enemies, &c. A property also which, added to the permanent nature of its colour, must, as Mr. Kemp observes, render it valuable as an ink, or water colour.

#### ARTICLE IV.

*Refutation of Mr. Walker's Claim to the Discovery of the Uses of the Cerebellum: with further Observations on Respiration.* By Dr. Cross.

(To Dr. Thomson.)

SIR,

Glasgow, April 8, 1815.

IN the 27th number of your *Annals of Philosophy* there appeared a letter from one Dr. Leach, in which it is peremptorily asserted that Gall and Spurzheim have anticipated me in the discovery of the function of the cerebellum, and of the structure of the "*spinal mass of nerves*."

\* I may observe in addition to the above, that as far as my observation extends, albumen, such as it exists in the blood, is not found as a product of secretion, either by a mucous membrane, or glandular apparatus. I am aware that many substances have been called albumen by different chemists, which, as Berzelius first showed, were not entitled to the name; as for example, the mucus of the gall bladder, which appears to possess many of its properties. But this is precipitated by acetic acid, even without heat. It cannot therefore be albumen, since it is well known that a solution of albumen in acetic acid may be boiled without coagulation, and that prussiate of potash precipitates it from this solution. On the other hand, as was first shown, I believe, by the above mentioned excellent chemist, Berzelius, phosphoric acid also precipitates albumen from its solution in acetic acid, but the great advantage of re-dissolving the precipitate, if added in excess. Hence phosphoric acid and prussiate of potash may perhaps be considered as the best tests of albumen at present known. The phosphoric acids, also, and, I believe, most of the other vegetable acids, like the acetic, do not coagulate albumen.

In the 28th number, Mr. Alexander Walker comes forward, strikes Dr. Leach off the field by giving a flat denial to his most unfounded assertion, and thrusts in a claim for himself to the discovery. Mr. Walker, in quoting from my letter, begins at the middle of a sentence, and thus makes me appear to deduce a conclusion from most insufficient data. Moreover, he merely quotes my first conjectures on the subject without giving the smallest hint of the decisive experiments to which they led. Genuine philosophy ought to expand the breast with candour.

I never before saw Mr. Walker's speculations on the nervous system, or knew that they existed. Had I seen the third volume of the Archives before I wrote to you, I would not certainly have claimed the discovery of the quadripartition of the spinal marrow. This discovery, although quite original on my part, belongs, from priority of publication, to Mr. Walker. Mr. Walker, however, has not anticipated me with respect to the sacral termination of the spinal marrow. I am the first, so far as my reading has gone, to lift off the cauda equina, and show the marrow terminating at the sacrum in a sharp point like the quill of a porcupine. This discovery rather militates against the old doctrine that the spinal marrow is just a bundle of nerves proceeding to and from the brain, which doctrine Mr. Walker has adopted. "The spinal marrow," he asserts, "serves no other purpose than a nerve would have done in the same situation, although from its being protected by the canal of the vertebræ, and the productions of the cerebral membranes, it requires not the strong and more close investments which the nerves possess in order to protect them in their passage among moving organs." (Archives, vol. iii. p. 142.) I, on the contrary, view the cerebrum, cerebellum, and spinal marrow, down to the very point of this porcupine extremity, as one continuous organ, which may be styled the animal brain, while the cauda equina, and all the other animal nerves, are merely derivative.

I do not know what Mr. Walker means by quoting from the Archives about the cerebellum. His hypothesis regarding the cerebellum is, that it is the organ of volition; and he arrived at this same hypothesis by the following logical ratiocination. Because the situation of the cerebellum is opposite to the situation of the face, therefore the function of the cerebellum must be opposite to the function of the face; and it being an understood maxim in physiology that *sensation is just directly opposite to volition*, and as sensation resides in the face, so volition must reside in the cerebellum. This doctrine is said to be corroborated by this sapient consideration, "that as the organs of sense and the cerebellum are the *first* and the *last* portions of the nervous system, so sensation and volition are the first and the last of its functions." Although here the onus probandi lies with Mr. Walker, yet, to put this absurd and groundless hypothesis at rest, I may mention that volition ranks among the faculties of mind, whose organ is the cerebrum; and that affections of the cerebrum, while the cerebellum remains sound, produce

palsy, which I humbly submit is just a loss of volition. Mr. Walker's rude, indigested hypothesis regarding the cerebellum must therefore fall to the ground. There is not one word from him about the cerebellum supplying the face with nervous energy. On the contrary, he makes out a direct opposition between the face and cerebellum. Had Mr. Walker, in his loose, dashing, conjectural way, thrown out a hint that perhaps the face might derive its nervous supply from the cerebellum, yet he would have had no claim to the discovery; for a discovery is not made until some kind of proof has been adduced; but it happens unfortunately for Mr. Walker that there is not even the smallest hint, from the beginning to the end of his tract, that could at all lead in the smallest degree towards the discovery. On the contrary, Mr. Walker has kept his face right away from the true direction, and looks earnestly down through the foramen magnum after the posterior column of the spinal marrow as far as his eye can reach. Mr. Walker's volition is just about as far from the function of the cerebellum as Dr. Gall's amativeness.

My letter to you does not announce that I intend to make these discoveries the subject of my promised work. My announced subject is physiology and physiognomy, in which there are yet vast regions of terra incognita.

Notwithstanding the grand eulogy bestowed by Mr. Walker upon Dr. Spurzheim's work, I cannot help viewing it, with the exception of a little anatomical discovery, as a most fanciful production. The alchemists, as a friend of mine lately remarked when talking on this very subject, did actually improve the science of chemistry, although they have never yet found out the philosopher's stone. But enough on this subject at present, as I intend to take a future opportunity of making a few critical remarks on the Gallian doctrine.

In order to render this letter more worthy of insertion, allow me to say a little in amplification of the theory of respiration broached in my former letter. Of the three fundamental functions, respiration, nutrition, and propagation, only two, respiration and nutrition, are immediately connected with the life of the individual. The third is prospectively concerned with the continuation of the species, and indeed is not evolved till an advanced period of life.

What becomes of the food which is swallowed? Some goes to the growth of the body in youth, some in certain constitutions to obesity; some passes off in alvine, urinary, and cutaneous excretions, in cerumen, snot, &c.; but all these excretions, with the largest allowance, do not nearly balance the quantity of food devoured. When it is moreover considered that some animals do not grow at all from birth till death; that all animals during a great part of life do not grow; that many great eaters never become fat; that in a state of health the excretions are trifling, and consist more of noxious than of nutritive materials, and that vegetables, which absorb such an immense quantity of sap, have no alvine, urinary, or such other excretion whatever, the inquirer becomes quite dis-

tified, and looks about for some more important function upon which the great quantity of chyle absorbed may be expended. Physiologists have traced the food through the alimentary tube and lacteal vessels into the subclavian vein; the chyle having got fairly into the circulation, is hurried, along with the blood and lymph returning from all parts of the body, to the lungs. All the blood, and all the chyle, and all the lymph, must perform the pulmonary circulation before they be admitted into the great systemic circulation. Now let us attend to what takes place in the lungs. According to your own calculation, as stated in the 737th page of the 5th vol. of your *System of Chemistry*, 3d edit. there are thrown out of the lungs by ordinary respiration in 24 hours no less than 40 000 cubic inches of carbonic acid, a quantity which contains about  $\frac{1}{4}$  lb. avoirdupois of solid carbon. Whence is this carbon derived? The food is the only source from whence such a supply of carbon can be derived; while the blood is at once the grand reservoir of carbon to the lungs, and the vehicle of vitality to the body. By this conjoint view of respiration and nutrition, two mysteries are cleared up at once—the source of the carbon, and the primary purpose of the food. The life of man has often been poetically compared to the burning of a fire, taper, &c. This poetical turns out a scientific analogy. As long as there is a supply of fuel, and a free admission of air, the animal fire continues to burn; the carbon of the fuel combining with the oxygen of the atmosphere, and forming carbonic acid. Whenever the supply of fuel, or of atmospheric air, is interrupted, the fire declines; if the interruption is momentary, the fuel may rekindle; if the interruption has been too long, the fire goes out for ever. Thus we see that respiration is the great primary function for whose sake digestion was instituted; while all the excretions, like the ash-pits of a furnace, are things of merely secondary moment in the animal economy. To ensure a supply to respiration, carbon has been made the great substratum of vegetable and animal fabric—the chemical skeleton; so that when chyle is deficient, the very substance of the body is carried off to the lungs, and sacrificed on the shrine of respiration. No living organized body, from the primitive germ up to the adult stature, from the microscopic animalcule up to the whale, from the rudest lichen up to man, has ever been seen without organs of respiration. The cotyledons of vegetable seed must emit carbonic acid gas ere the radicle begins to send down its fibres into the earth; and if these cotyledons cease their respiration, and fail to become seminal leaves before the plumula rises and spreads its foliage to the heavens, the plant dies. If the pores of a fecund egg are stopped up, the hen may hatch upon it while she has heat in her body without bringing forth the chick. Nor does the chick, after it is fully formed, delay a moment to drive its bill through the shell for the free admission of air. The embryo, ere it begins to evolve, is connected to the uterus by means of a placenta as an intermediate agent between the foetus and the maternal lungs; and whenever the function of the placenta

and the foetal circulation cease, that moment do the thorax and diaphragm commence operation; and if the placental function ceases for but a few minutes, before the mouth or nostrils get into atmospheric air, the child is irretrievably gone. That thorax and that diaphragm, having once begun, must continue their motion till death. The first sign of death is the disappearance of respiration; and the first sign of recovery from asphyxia is a renewal of respiration. It is not until God breathes into the nostrils the breath of life that man becomes a living creature; and no sooner does the breath cease, than man becomes a mere lump of organized clay.

Upon this view of respiration and nutrition, and upon the reciprocity of their functions, or rather upon the subordination of nutrition to respiration, I have formed a new theory of diseases, and more rational and successful methods of cure. To enter upon this subject would be encroaching in kind, as well as quantity, on your Journal. That purgation is a cure for melancholia and mania, I in the mean time take this opportunity of announcing; both to secure to myself the discovery, and the sooner to remove the most afflicting and the most horrible of all the sufferings of humanity. The rationale and cases shall be brought forward in detail elsewhere. I expect shortly to see many who had recommended or administered a few doses of physic to these diseases start forth as claimants of this discovery.

I remain, very respectfully, Sir,

Your most obedient servant,

JOHN CROSS.

## ARTICLE V.

*Experiments showing that in Hepatitis the Urine contains no Urea.*  
By Mr. C. B. Rose.

(To Dr. Thomson.)

SIR,

*Ego, April 6, 1815.*

PERMIT me, through the medium of your *Annals*, to transmit to its chemical and medical readers the knowledge of the absence of urea from urine not being confined to the urine of diabetes only; for, while examining the urine of a girl labouring under a chronic inflammation of the liver, I could discover no trace of urea: indeed, its absence was as complete as in cases of diabetes mellitus; and I have ascertained this to be the case by a repetition of my experiments on the urine in several cases of acute as well as chronic inflammations of the liver. The urine operated upon in acute hepatitis was rather high coloured; in the chronic disease, it was pale; its odour not so urinous; its specific gravity less than that of healthy urine; and consequently left a smaller quantity of extract when evaporated.

Whether the above state of the urine is dependant on the

dyspeptic stomach from hepatitis only, or on every case of dyspepsia, I have not yet satisfied myself; but I intend pursuing the subject with that view.

Unavoidably drawn by this discovery to turn my thoughts on the nature of secretion and digestion, I have dared to indulge in speculative ideas. That there subsists a great connexion between the stomach and the kidneys, not merely sympathetic, and that the state of the urine depends very much upon the state of the digestive organs, has long been observed; but that there existed this decided concatenation between the liver, stomach, and kidneys, was not, I think, before known. As this concatenation is demonstrated by the above circumstance, is it not probable that the kidneys have a similar consent of action with all the chylopoetic viscera? And is it too sweeping a conclusion to draw, from our present knowledge of secretion and digestion, if we say that it is probable in diabetes mellitus the saccharine quality of the urine is dependent on the morbid action of the stomach, and the absence of urea from the urine on a deranged state of the hepatic function? Or to suggest the probability that some one or other of the proximate elements of the urine is lost, or a new one added, by a morbid action of one of the chylopoetic organs? For instance, what change in the urine may a schirrhous pancreas produce? These inferences must not be tolerated until more data are produced; nor should I have hazarded either of them, had I not some reason to believe, from the examination of the urine of two dyspeptic patients, that the want of urea is observed in cases of hepatitis only.

Might not the want of urea in the urine of persons attacked with hydrocephalus, be a diagnostic mark between hydrocephalus *idiopathicus*, and hydrocephalus *from altered function* of the liver?

It is, I trust, the general opinion of the physiologists of the present day that the kidneys are not *merely separating but secreting organs*: and the absence of urea in hepatitis I consider as an additional fact in support of the latter opinion; for if that state of the stomach consequent on a morbid action of the hepatic system is productive of a want of urea in the urine; and if urea has never been found in the blood (which it never has); must we not infer that its elements are the products of digestion; that they, in a peculiar state of combination, are presented to the cryptæ of the kidneys, where it is presumed the secretory power resides, and there recombined. I have communicated this fact, and my reflections on it, to you, in its unexplored state, merely with the view to excite an inquiry into, and an examination of the subject, by persons far more equal to the task than myself.

You may insert the following also, if you think it worth notice. Perhaps to those of your medical readers who are but little versed in chemical analysis, and not furnished with any chemical apparatus, and yet would be desirous of examining the urine in hepatitis, not only with the intent to prosecute the above inquiry, but also to assist in establishing the diagnosis of that disease, a brief relation of the

method I have followed in my experiments, and a description of the apparatus I have invented to operate with, may not be unacceptable.

Dr. Henry, in the first number of the *Annals of Philosophy*, after having related some experiments on the urine discharged in diabetes mellitus, in which he employed the nitric acid with the extract as a test of urea, says, "There is one property, however, of this substance, originally pointed out by Fourcroy and Vauquelin, which enables us to detect urea, even when present in such minute quantities as to escape discovery by nitric acid. Amidst the great variety of animal products, this appears to be the only one which is decomposed, when in a state of solution, by the temperature of boiling water. At this low degree of heat its elements, held together by a balance of affinities which is easily disturbed, arrange themselves in a new order; ammonia and carbonic acid are generated; and carbonate of ammonia is composed, equivalent in weight to about two-thirds that of the urea. It is in the fluid, therefore, condensed during the evaporation of diabetic urine, that we are to look for traces of the existence of urea; and in this fluid I have invariably found a sufficient quantity of carbonate of ammonia to restore the colour of reddened litmus paper, and to precipitate muriate of lime." In my search for urea I have generally chosen this method by distilling the urine, and applying the tests to the condensed fluid, in preference to the more tedious process of evaporation for the extract, &c. In Dr. Henry's paper, (*Annals of Philosophy*, No. 1.) and in his Elements of Experimental Chemistry, may be found information sufficient to enable any one to accomplish the examination of urine for the above purpose with the necessary precision.

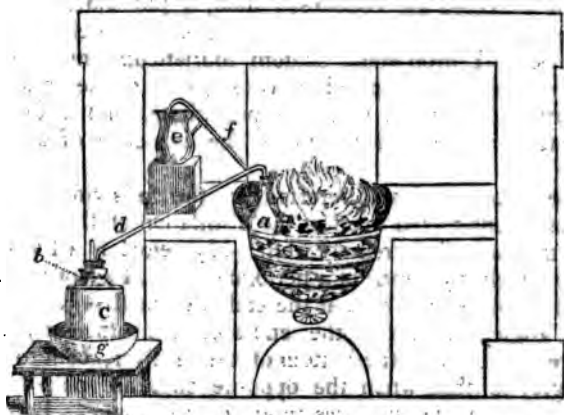
Having collected the condensed fluid, I put some of it into a wine-glass, to which I add some solutio muriatis calcis; if a precipitate subside, I drop some acidum muriaticum into the liquid, which, from its greater specific gravity, passes to the bottom of the glass, comes in contact with the precipitate, and a brisk effervescence follows, the decisive test of the existence of urea in the urine.

Not being provided with retorts, &c. the following apparatus is what I have constructed to effect the distillation of the urine, and it answers the purpose extremely well. My retort is a Florence oil-flask; to which, by means of a perforated cork, I adapt a barometer tube, which has a small part at the end next the flask, bent to rather an acute angle, and at the other end a portion sufficiently long that it may reach nearly to the bottom of the receiver, bent to nearly a right angle, so that when the opposite ends are placed in their respective places the tube lies in an inclined plane. By means of a leather collar fastened to the top of a jar, I fix in it a wide mouthed bottle for a receiver. The lower end of the tube passes into this, through a perforated cork, and a small short tube is passed into the receiver to admit of the exit of air. This cork should be

luted into the bottle; perhaps putty is the best lute. The jar is filled with cold water as a refrigerator; but the principal agent in condensing the vapour is a glass syphon (and so efficient is it, that the condensed vapour falls guttatim into the receiver). It is placed so as to convey cold water on to the tube which connects the retort with the receiver, and this, trickling along it, is received by a large bason, in which the jar and bottle stand. The tube may be luted to the flask and bottle with a stiff dough of flour and water. If the lute is not carefully, and in sufficient quantity, applied around the tube where it passes into the cork of the receiver, the water from the syphon will wash it away, and into the receiver, making the liquid cloudy, and oblige you to repeat the process. The longer the tube is to convey the vapour, and the larger its calibre, the better it is for your purpose. The calibre of the syphon may be between a twelfth and a sixteenth of an inch in diameter. These tubes may be bent in any form, with the assistance of a lamp and blow-pipe, or a blacksmith's fire. Fearful that my description of the still which I have been in the habit of using is not sufficiently clear to enable any body to construct one by it, I have subjoined a rough sketch of it as it appears when in use. The readiness with which its materials may be had, the ease with which it may be constructed, and the convenience of such a simple apparatus to the juvenile experimentalist, are things which I hope will in some measure repay you for giving the above tedious description, and the following sketch, a place, which might have been occupied by something of more importance.

I remain, Sir, yours most respectfully,

C. B. ROSE.



*a*, the Florence oil-flask; *b*, the bottle, as the receiver; *c*, the jar in which the bottle is fixed; *d*, the barometer tube to convey the vapour; *e*, the jug of cold water; *f*, the glass syphon; *g*, the bason in which the jar stands.

## ARTICLE VI.

*Reply to Mr. Phillips's Animadversions.* By Mr. Hume.

(To Dr. Thomson.)

SIR,

WHEN your correspondent, Mr. Phillips, shall have finished his consultation with Klaproth's Essays he will find me ready to proceed in my reply. Mr. Phillips has practised the art of garbling most dexterously in his last letter to you, it is therefore incumbent on him, in order to satisfy your readers at least, that he turn once more to the Analytical Essays, and read and explain many passages which are more immediately connected with the subject.

If Mr. Phillips be unwilling to perform the task, or ashamed to admit the truth, I shall mention a few of the items to which I refer, as proofs that Mr. Klaproth never applied nitrate of silver as a test for arsenic, and that he never, on any occasion, combined it with the oxide of arsenic, where alone it is so eminently useful; for I never heard of a single instance where the acid of arsenic had been exhibited as a poison from sinister motives. But I shall not take up your time and space further than to offer the following references to Mr. Klaproth's work.

Vol. i. p. 566. "Both the solutions of the arsenical oxide in water showed exactly the same appearances which are exhibited by any other aqueous solution of arsenic." Here is an example, where Mr. Phillips must acknowledge there can be no quibbling about the author's acquaintance with the superior efficacy of silver, for none was used. Mr. Klaproth then proceeds. "By combination with lime-water, they (the two solutions) yielded arseniate of lime: with sulphuret of ammonia, yellow sulphuret of arsenic (orpiment): and the green pigment of Scheele, with ammoniacal oxide of copper."

If the English translation here be correct, I may remark, that arseniate of lime and the green pigment of Scheele are incompatible products from a solution of white oxide of arsenic in water; an arseniate of lime must require the acid of arsenic, and Scheele's precipitate certainly is composed with copper and the white oxide or arsenious acid.\*

In every case where great exactness was required to detect and appreciate the quantity of arsenic, I do not perceive that Klaproth ever applied silver as a test. Thus, p. 140, vol. i. no silver test was used, although at p. 142, "a weak arsenical smell was perceived." He speaks of "a slight trace of arsenic;" and some of it combined with the copper, and some with the iron." Page 158. "The muriate of silver emitted some arsenical vapours." Page 160. "It is then evident that this ore consists of silver, iron, arsenic, and antimony." In the same place he notices "arseniated iron." Page 526. "A grey-yellow sublimate" and "a faint odour of

\* The English translation is inaccurate. "The words used by Klaproth are arsenikalische kalkerde.—T.

arsenic was perceived;" yet this article is not included in the list of component parts. Page 548. The solution of variegated copper-ore in nitric acid "was tried by the proper *re-agents* for silver, lead, zinc, *arsenic*, &c." Here I am anxious to know what reagent or test Mr. Klaproth took for the arsenic, for, a more suitable and altogether palpable case for exercising the silver test does not occur in either volume. Page 567. "One grain of sublimed arsenic" is mentioned; "the arsenic escaped in vapours;"—"the cobalt (after calcination with charcoal) was not yet entirely deprived of all portion of arsenic." In this case, I must observe, arsenic seems to have been *deposited* in place of being dissolved in the nitric acid. Indeed the whole of this essay, the analysis of the white cobalt-ore, offers many opportunities which admit of the silver test; yet it was never employed, certainly from no other cause than that the author was not aware of the value of the silver as a test, and this appears so evident through the whole of the two volumes as to admit of no contradiction.

In the second volume, page 150, the nitric acid is used to dissolve the olive copper-ore, and the *acetate of lead* is preferred to collect and separate the whole of the arsenic acid; and this *precedes* the next step the author takes, which Mr. Phillips, to serve his own purposes, has separated from the whole process of Mr. Klaproth, viz. merely to regenerate what the author supposed to be the native compound, the arseniate of silver, described in the first volume, page 125: and this was evidently done to prove that the arseniate of lead contained no *sulphate* of lead, and that the arsenic found in the ore is in the state of an *acid*. I need not say that such conclusion must be very uncertain in all cases where an ore has been subjected to the action of *nitric acid*. At page 153, the author states that "not only the crude ore itself, but also the precipitate obtained from its nitric solution by means of acetated lead, when reduced by the blow-pipe upon charcoal, give out a vapour, which, by its garlic smell, sufficiently shows to be arsenical:" query, the *metal* or *oxide* of arsenic? The last example which Mr. Klaproth gives us in his very excellent and most valuable essays, in which the arsenic is found, is in the *pharmacolite*. Here it will be seen that, after all the author's experience, including that analysis quoted by Mr. Phillips, he fixes upon the *acetate of lead* alone for detecting, separating, and ascertaining the arsenic acid; not a word, or the slightest allusion to a silver test; nor is there a single syllable, or the most trifling hint from Mr. Klaproth, in any part of his work, to induce an operator to prefer silver to lead as a test for white arsenic.

It is with much diffidence and hesitation that I offer any observation on the works of such a celebrated master as Klaproth. It appears, however, that on many occasions the true state in which the arsenic previously existed in the ore was not completely demonstrated by subsequent experiments; for, by employing nitric acid in almost every instance, the metal became acidified, and, consequently, its primitive and more

simple form entirely changed. I have occasionally analyzed many minerals in which arsenic made a prominent ingredient, and these were first treated by the following process, which I found to be very successful as a preliminary operation. I boiled the pulverized ore in a solution of subcarbonate of potash, and when the solution was filtrated and allowed to cool, a sufficient quantity of acetic acid was added to saturate the solution. In all these examples I never discovered any sign of arsenic acid; for, on presenting a piece of dry nitrate of silver (lunar caustic) to the surface of the solution, I invariably procured a bright yellow, and not a red or brick-coloured precipitate. This method, as far as I can learn, has not been practised by any other person, I may therefore take this occasion to claim it as peculiarly my own. I have had no opportunity to analyze the red arseniate of silver, or any other arseniate, but I have little doubt that, if such a compound be submitted to the same process, the result will be equally satisfactory, and that a brick-red coloured precipitate would confirm my anticipation of one component part of such a mineral substance being the acid of arsenic.

In regard to the nitrate of barytes, that its solution is immiscible with nitric acid, and that the method employed by many chemists, before my paper was published, namely, to purify the nitric acid by adding the solution of this salt, is absurd and ineffectual, I repeat, that all I have said on that subject will defy Mr. Phillips's best efforts. Dr. Duncan, jun. has read my papers, and evidently draws a very different inference from my communication, and this Mr. Phillips may see in the "Edinburgh new Dispensary;" it therefore remains with Mr. Phillips himself to explain what M. Botillon Lagrange and Dr. Swediaur mean by *acid nitrique*. Before I close this letter, allow me to observe, that the word *barytic* is omitted by Mr. Phillips in the quotation from me, in one part of his last letter; and I may here add, that, to serve his own cause, he made a material and very gross omission of this kind, in his observations respecting the *compound extract of colocynth*, in what has been termed his *acute* work upon the pharmacopœia, where he confounds proof spirit with the tincture expressed from the colocynth, which must be charged with vegetable mutilage and other principles, and therefore it can no longer be considered as a *proof* spirit.

I am truly sorry, Sir, to have occupied your Journal by such an unprofitable subject, I trust, however, that the present communication will be not altogether uninteresting to your readers. The only apology I can make is founded upon the very illiberal, and, I may add, unfriendly language in the latter part of Mr. Phillips's first letter, which is evidently written with a view to injure my character. I remain, Sir, your very obedient servant,

Long Acre, April 12, 1815.

JOS. HUME.

[The editor hopes that neither of the gentlemen concerned will continue this dispute any longer, as it is evidently fast sinking into a verbal invective.]

## ARTICLE VII.

*Remarks on the Phenomena of Galvanism.*

IN the present fluctuating state of galvanic theories, when such a variety of opinions are offered for our consideration ; it may fairly be concluded that every hypothesis will be noticed in proportion to its plausibility. I shall therefore endeavour to place the subject in a different point of view from any yet published ; after having made a few remarks on several opinions that have been brought forward to account for the various galvanic phenomena.

When Volta offered his simple, yet ingenious, electrical theory, to illustrate the effects of the galvanic pile, or battery, it appeared tolerably satisfactory : but as galvanic facts became more numerous, and exhibited such a novelty of character and principles, the views of this eminent philosopher were too circumscribed to embrace the great number of results that were brought to light by the rapid advance of this branch of philosophy. Although the opinions of Volta were too circumscribed to keep pace with the discoveries in galvanism, still they were too ingenious to remain unnoticed, and therefore have been remodelled by several philosophers in this country, as well as in other parts of Europe. In these alterations the ideas of Volta have been generally retained, as far as respects the electrical character of the galvanic fluid ; but some writers have differed from him, by supposing that the galvanic fluid is brought into action by the chemical agency of the acid, or saline solution, used in the pile or battery, and not merely by the contact of dissimilar metals, as Volta had supposed ; consequently they have been obliged to ascribe different laws to this fluid from those which were maintained by that philosopher.

In pursuing the subject of galvanism, it is extremely difficult to follow the many peculiar laws of action that this electrical view of the science enjoins, and still more difficult to reconcile all the galvanic results to those principles.

To any one well acquainted with the laws of electricity, it must appear very improbable that the negative and positive, or resinous and vitreous, state of the opposite wires of a battery, on which the most interesting phenomena are said to depend, can be obtained when they are placed in a solution that is a conducting medium : and that these phenomena, which are supposed to depend on the contrary electrical states of the wires, should become more powerful as the conducting power of this medium is increased ; is a result that cannot be reconciled to any well known electrical principle. By considering the galvanic phenomena agreeably to the popular theories of electricity, we must infer that many results depend on laws the very reverse of simplicity ; for according to Dr. Franklin's hypothesis, we must suppose when the battery is in action that there

is a redundancy at one end, and a deficiency, or less than the natural quantity of this fluid, at the opposite end of the galvanic arrangement; and we are likewise required by the same authority to believe, that the individual particles of this fluid are violently repulsive of themselves; and yet, with this repulsion towards each other, they crowd and condense themselves at one end of the battery, although every part of this galvanic combination is a conducting medium. The other popular electrical theory admits of two distinct fluids, the particles of which are supposed to be strongly repulsive of those of their own kind; but at the same time are violently attractive of the particles of the contrary fluid.

In the application of this theory to illustrate the action of the voltaic battery, we are led to believe that the electricities of the battery are separated, that the particles of the vitreous fluid are collected at one, and the particles of the resinous fluid are collected at the opposite, end of the battery. Therefore if we follow the changes that must take place according to this theory, they will appear truly extraordinary; for these contrary fluids, which are supposed to have so great an attraction for their opposite kind, must have passed each other in their progress from one part of the battery to the other, and have collected in great quantities at the contrary ends, although the particles of each fluid thus collected are violently repulsive of themselves.

The above results, agreeably to this theory, take place in the battery while every part is a conducting medium, which, according to all the well-known laws of electricity, would counteract any such unnatural accumulation. These, as well as other complicated difficulties, must have been evident to every one who has paid attention to electrical theories; and several writers have substituted a phraseology that gives a more specious illustration to certain results, but at the same time involves the whole subject in greater obscurity; for an attempt has been made to explain some of the most interesting effects in galvanism, by what are termed "*electrical energies*," without supposing the existence or action of any distinct galvanic or electric matter.

This vague and general notion may give a plausible solution to the transfer of small bodies through a fluid medium, by supposing that the electrical character of the wires from the battery influences and gives a similar character to the particles of the fluid medium that come in contact with them, and that these portions of the medium influence or give a corresponding electrical character to their adjoining particles. That this communicating principle of the electrical energies may extend itself through the whole of the medium, and produce the transfer in question, does not appear very improbable: but this principle will not readily explain the heating and melting of steel wire when placed in the circuit of the battery; for we can scarcely imagine that the particles of a steel wire ten or twelve inches long, which are evidently held together by a stronger cohesive force than the particles of any other body in nature, can be

heated red-hot and torn asunder merely by the wires from the battery being in contrary electrical states, without imparting any active matter to the wire, which must be the case if we do not admit the existence and circulation of the galvanic fluid or fluids.

Several other difficulties might be mentioned that must attend this electrical view of the science; but as the chief object of this communication is the introduction of views which appear more consistent with the general phenomena of galvanism, and not the refutation of previous opinions, it will not be necessary to dwell on any other particulars. When we impartially reflect on the many important facts offered for our consideration by the present advanced state of galvanism, and observe the great degree of ingenuity displayed in the application of various theories to account for the numerous galvanic phenomena, we must feel disappointed at the little theoretic satisfaction that has been obtained from the knowledge of so many interesting facts, and the united efforts of so much ingenious labour, and be led to infer that the chief cause of this disappointment has been the adoption of principles that are incompatible with the physical operations of Nature.

The speculations of the celebrated Franklin, perhaps, unfortunately gave too specious a facility to his electrical illustrations, as they appeared to explain in a tolerable manner all the electric facts that were known at the period he lived; but they ascribed laws of action to the electric matter that involved the subject in complete mystery; for results, that seemed to open an extensive field for inquiry, were illustrated by principles that were beyond the limits of human comprehension, and in a great measure superseded the necessity of an experimental investigation. After a view of these difficulties, our attention is naturally invited to the increasing probability that these galvanic agents have characters and qualities less mysterious than has been generally imagined, and that their principles of action are uniformly consistent with the general laws of matter. Although it is evident, from our present imperfect knowledge of galvanism, that every hypothesis must be incomplete, still there seems to be a number of well-known facts that are worthy of particular attention; and if an opinion is formed, having a judicious reference to these circumstances, it may hasten the developement of that chain of experimental results, on which a correct and complete system of galvanism will be ultimately established. By attentively considering the action of the voltaic battery, I have been induced to suppose that two galvanic fluids are generated during the process, consisting of a large portion of caloric, and two distinct and highly attenuated bases, that partake of an oxygenous and hydrogenous nature; and in this communication I shall endeavour to point out the reasons that led to this supposition, hoping they will stimulate some other person to extend the investigation agreeably to this view of the subject. In pursuing this investigation, our illustration must in a great measure depend on deductions drawn from various analogies; and as these remarks have a particular allusion to

the similarity of character there is between some of the gaseous bodies and the galvanic fluids, a general view of those analogies will be found of considerable advantage throughout the most obscure and difficult part of the inquiry.

If a united stream of oxygen and hydrogen gases be lighted, and blown upon bodies that are even difficult of combustion, they will be quickly consumed by the continued action of these gases; and it is also a fact, that when the two galvanic fluids are thrown upon the same class of bodies they will produce similar effects. In the common and regular process of combustion the presence of oxygen is indispensably necessary; but when the bodies to be burnt are placed in a gas that contains no oxygen, or even in an exhausted receiver, they may be consumed by the agency of the galvanic fluids.

When a galvanic battery acts in the most powerful manner, particularly in the display of its chemical energies, there is a singular coincidence of results during its action that is worthy of notice, for a part of the water in the battery is decomposed, and at the same time two galvanic fluids appear to be liberated from the battery, whose character, in many other instances besides those just mentioned, bears a strong resemblance to the two gases obtained from the decomposition of water.

At present we may not be able to comprehend clearly the law by which these newly discovered galvanic combinations are generated; but if we follow the evident changes that water undergoes by its union with different portions of caloric, we may perhaps venture to infer that these galvanic agents are generated during the action of the battery by a principle somewhat similar.

Water in its solid form, or character, of ice, is united comparatively with a small quantity of caloric, when combined with a larger portion of this agent, it obtains the liquid state: but if united to a still greater portion, it assumes the character of steam, and often displays a mechanical force that is as singularly striking as the power of the galvanic fluids. By the application of a greater degree of heat than what is necessary for the changes already stated, the constituent parts of water become separated, and assume the state of two invisible gases, whose peculiarities approximate much nearer the character of the galvanic fluids: for the fact that oxygen and hydrogen gases mixed in proper quantities enter into combustion, when sufficiently influenced by heat or a mechanical force, presents a strong feature of those agents; as it is more than probable, from the velocity with which the galvanic fluids move, that in many instances they produce a high degree of temperature by their mechanical action upon each other, and the bodies that impede their free circulation, which gives rise to their combustible energies.

Although a variety of circumstances already mentioned appear to support the idea that the bases of the galvanic fluids are derived from the liquid in the battery, and partake of an oxygenous and hydrogenous nature; still it is extremely probable that these newly

discovered compounds are formed by a very different union with caloric from that which takes place in the formation of oxygen and hydrogen gases, as we have not the least evidence that the greatest possible attenuation we could obtain by the application of heat to these gases would impart to them any electrical energies.

If we take into consideration the subtle and active nature of caloric, and compare it with the most attenuated gaseous bodies yet known, we shall see the probability that other compounds of this nature may be formed by the action of various chemical and mechanical forces, that must hold a rank between gaseous bodies and caloric; and, according to the established rules of analogy, are likely to be endowed with energies strongly resembling the powers of the galvanic fluids. The idea that a class of highly attenuated compound bodies holds a place between well-known gaseous bodies and caloric, deserves perhaps more attention from its reasonableness, than any direct evidence we at present possess; as we cannot fairly suppose that this agent, which in its active state converts the hardest and closest metals into vapour, and can by its influence change a solid mass of ice into two combustible gases, should lose this transforming power exactly at the point where it gives these bodies this gaseous character, when there is evidently such a wide difference between the comparatively dense nature of these gases and caloric.

If we admit the probable existence of these highly attenuated compounds, no results can be more reasonably expected from their action, than some of the effects we obtain by galvanism; as combinations partaking of a small portion of oxygen and hydrogen, intimately combined with a large quantity of caloric, must partake in a great degree of its active and penetrating nature, while their bases are calculated to produce many similar effects to those which are obtained by the energies of the galvanic fluids. This appears a simple mode of accounting for the production of what are termed the galvanic fluids; and the principle by which they seem to obtain their energies appears consistent with the idea that they are generated by the action of the battery.\* When a galvanic combination produces a strong chemical action, these fluids are given out in great abundance, and it is not improbable that they are propelled along the conducting circuit by the high degree of elasticity which every succeeding portion obtains at the point of generation. In some experiments made to show the transfer of acid and alkaline bodies by the galvanic influence, the results obtained seem intimately connected with this part of the inquiry, as they evidently point out two opposite galvanic currents, and support the above opinions.

The experiments were made by using two small gold conical cups; in one was placed a solution of the sulphate of potash, and

\* In these remarks we have supposed the galvanic fluids to be produced by a strong chemical action; but when obtained without this violent action, their character approaches much nearer to common electricity, and will be considered with greater propriety under a more general view of the subject.

in the other pure water, and the liquid in the two vessels was united by moistened amianthus; when the cup in which the saline mixture was put was galvanized positively, and the other cup negatively, the solution in the positive cup was soon found to have acquired a considerable degree of acidity, while the potash of the solution was transferred to the cup that contained the pure water. But when that cup which contained the saline mixture was galvanized negatively, and the other positively, the potash was left, and the acid portion transferred to the other cup, clearly showing, when a solution which contains an acid and an alkali is decomposed by the action of the galvanic fluids, that the positive fluid transfers the alkaline, and the negative fluid the acid, part of the solution.

This transfer of an acid substance through a fluid conducting medium, from the negative to the positive wire, appears the most conclusive fact ever published in favour of two distinct electric or galvanic fluids; as we cannot, on any rational principle, explain this transfer of matter, if we suppose the wire called the negative wire to be deficient in galvanic electricity, and supplied by a current passing from the opposite or positive wire.

The various results attending the heating and melting of steel wire when placed in the galvanic circuit also support the idea of two opposite galvanic currents. These effects most probably arise from the two opposite currents not having room to pass each other freely along the small steel wire; and their action upon each other gives birth to that combustible energy which their constitutional character is calculated to support. Although we are furnished with such strong evidence that there are two different and opposite galvanic currents in most experiments; yet it must seem rather extraordinary that these fluids should pass each other in contrary directions, when each fluid is generally supposed to have so great an attraction for the opposite kind, as in most instances to produce combustion by the violence and rapidity of their union. These remarks naturally introduce another very interesting question, viz. What evidence have we that the galvanic or electric fluids have such a strong attraction for each other? But as an inquiry into this question is not necessary for my present purpose, I shall defer it until another opportunity. In the investigation of any difficult subject, when we have no clear and positive evidence to direct our conclusions, we must apply to the aid of analogy, and take advantage of the most appropriate facts which the present state of the subject furnish for our consideration. By the rapid union of oxygen and hydrogen gases, we obtain results that resemble most of the combustible effects of galvanism; still we know the two gases show no strong signs of attraction for each other, without the application of an extra portion of heat, or a mechanical force; may we not therefore infer that it is by a similar law that the galvanic fluids unite and produce their combustible effects; for although their constitutional nature will allow them to pass each other in a complete galvanic circuit, yet when they rush upon each other from the

opposite wires of a battery, which are the terminating points of a broken circuit, the mechanical action resulting from their contrary forces may induce them to unite with such rapidity as to render manifest all their combustible energies. This principle of action, and the probability that the bases of the galvanic fluids partake of an oxygenous and hydrogenous nature, will enable us to form a tolerably correct idea of the combustible effects of galvanism; but the most perplexing results attending the galvanic phenomena are said to be the invisible transfer of different bodies through various fluid media.

From the view we have taken of the subject there must be at the same time a distinct fluid quitting the end of each wire that proceeds from the battery, when they are placed in an imperfect conducting fluid medium; and to keep up the evident circulation, each fluid must endeavour to gain the wire opposite to the one it has quitted; it is therefore highly probable that the opposing forces of these contrary currents of the galvanic fluids, give rise to their powers of decomposition, rendered so manifest at the end of each conducting wire of a battery.

These general conclusions give us a new hypothetical view of the galvanic phenomena, the truth or correctness of which, will be the best ascertained by its application to explain what are termed the most perplexing results in galvanism.

In attempting to account for the invisible transfer of acid and alkaline matter through various fluid media, and the appearance of oxygen and hydrogen gases at the opposite wires of a battery, when separated by a column of water some feet in length, the correctness of this hypothesis will be put to a tolerably fair trial.

If we consider the characters we have attributed to the galvanic fluids, the invisible transfer of this acid and alkaline matter in opposite directions appears consistent with the view we have taken of the subject; for if the base of the positive fluid partakes of an oxygen nature, this fluid will probably convey to the negative wire, by the influence of affinity, the alkaline part of any saline solution which is decomposed at the positive wire, and deposit the greater portion of this transferred matter at the negative wire, when it enters that metallic part of the circuit.

We have supposed also that the base of the negative fluid may partake of an hydrogenous or alkaline nature, consequently this fluid may by the force of affinity convey the acid portion of any saline solution decomposed at the negative end of the battery, towards the positive wire; and there deposit this acid matter, when it enters the metallic part of the circuit; and this exchanging process most probably goes on, until the alkaline part of the solution is collected round the negative wire, and the acid portion of the same compound is collected round the positive wire of the battery. On this principle we may account for the appearance of oxygen and hydrogen gases at the opposite wires of a battery, though separated by several feet of water.

When water is decomposed by the galvanic action, and two distinct gases appear at the opposite wires, although separated by such a body of water, one of these gases must have been transferred in an invisible manner from one wire to the other, or the water must have been decomposed at each wire, and the constituent portions which do not appear at the point of decomposition must have become so far changed and influenced by the fluids from the battery, as to have passed with them through the water in an invisible state. This latter opinion, though rather novel, is agreeable to what has been advanced, and is strongly corroborated by the transfer of other substances, as well as those just mentioned. By keeping in view the preceding illustration of galvanic effects, it will appear that the hydrogen of the water decomposed at the positive end of the battery will be transferred by the positive fluid towards the negative wire, and there liberated: and that the oxygen of that portion of water decomposed at the negative end of the battery will be transferred by the negative fluid to the positive wire, and be there liberated; and ascend through the water in the character of oxygen and hydrogen gases.

These inferences are supported by the fact, that all the bodies collected and liberated round each wire of a battery possess such characteristic properties as are likely to be influenced by the attractive affinity of the galvanic fluids; if we admit that these fluids possess the constituent nature ascribed to them in this communication.\*

The positive evidence we have that the most dense bodies can be transformed by the agency of caloric to assume so many characters, naturally suggests the idea, that a great variety of combinations may take place by its union with the constituent parts of water, which are still unknown; and no products are more likely to be among this class than those elastic compounds which, in all probability, form the galvanic fluids; as they seem a link between well known gaseous bodies and caloric, by partaking of the constitutional character of the one, and the action and subtle nature of the other. Nor is it improbable but both the electric and galvanic fluids will, at some advanced period of these sciences, be considered merely as a newly discovered class of peculiar gaseous bodies, sufficiently attenuated by various degrees of caloric to give them different electrical energies.

It is not the results mentioned in this paper only that support this mode of reasoning, for the whole series of regular galvanic

\* In a small essay I lately published on Electricity, I have endeavoured to show the probability that the electric fluids excited by the machine consist of a large quantity of caloric intimately united to a small portion of oxygen and nitrogen obtained from the atmosphere by the mechanical action of the cylinder and rubber. Perhaps if the machine was so constructed that the cylinder could be surrounded and worked alternately in different kinds of gas, the electric fluids excited under these circumstances might display a variety in their chemical action that would lead to some interesting results; and it is not extremely improbable, but that the galvanic fluids would also manifest some variety of character, if they were excited by different agents properly calculated for such a purpose.

effects, as well as several anomalies, seem to point at something of this nature; and as opinions formed agreeably to this view of the subject will account for most of the galvanic phenomena in a simple and plausible manner, without the aid of mysterious principles, the subject assumes an highly interesting character, by the increasing probability, that the phenomena of galvanism are most intimately connected with many other important branches of natural philosophy.

## ARTICLE VIII.

*Defence of the Opinion that all Numbers have Four Imaginary Cube Roots.* By James Lockhart, Esq.

(To Dr. Thomson.)

SIR,

I AM much obliged to Dr. Tiarks, and to your Correspondent N. R. D., for their attention to my late communication. The disagreement of these Gentlemen in respect of the value of the imaginary quantity gives me encouragement to hope, that some doubt of the error which they suppose I have made will be excited. Dr. Tiarks affirms that the quantity is nothing but a different form of a well-known root of 64; whereas N. R. D. insists that it is a cube root of 8, and *not* of 64; and thus it would appear that the quantity is the square, and the square root of itself also. If impossible expressions, only a little complicated, universally lead to such difference of sentiment, it will be wise to abandon them altogether. Nevertheless, it now becomes me to endeavour to show that I have not made a hasty assertion, and that I was duly acquainted with the nature and construction of the quantity in question; and for this purpose I resort to the following remarks and demonstration.

In the general equation  $x^3 - b x = c$ , there are three roots,  $x$  the greater,  $-t$  the middlemost, and  $-v$  the least. The rule promulgated by Cardan gives all the three values, which, however, is denied by some eminent algebraists of the present day. I shall place the cube roots in their order under Cardan's binomials; and I believe that it is the first time of their being so exhibited.

$$\begin{aligned} & \sqrt[3]{\frac{c}{2}} + \sqrt{\frac{c^2}{4} - \frac{b^3}{27}} & \sqrt[3]{\frac{c}{2}} - \sqrt{\frac{c^2}{4} - \frac{b^3}{27}} \\ & \frac{x}{2} + \sqrt{\left(\frac{x^2}{4} - \frac{b}{3}\right)} & \frac{x}{2} - \sqrt{\left(\frac{x^2}{4} - \frac{b}{3}\right)} \\ & -\frac{t}{2} + \sqrt{\left(\frac{t^2}{4} - \frac{b}{3}\right)} & -\frac{t}{2} - \sqrt{\left(\frac{t^2}{4} - \frac{b}{3}\right)} \\ & -\frac{v}{2} + \sqrt{\left(\frac{v^2}{4} - \frac{b}{3}\right)} & -\frac{v}{2} - \sqrt{\left(\frac{v^2}{4} - \frac{b}{3}\right)} \end{aligned}$$

By means of these roots, and 28 varieties connected with them, the cube roots of all binomials may be obtained, if such roots admit of a finite expression, even when they are irrational, and without trial or assumption,

The imaginary quantity which I introduced relates only to  $t$ , and to the second cube root in the column on the left hand, which cube root is thus demonstrated to be exact;—

Let  $b t - t^3 = c$

$$\frac{b t - t^3}{2} = \frac{c}{2}$$

$$\frac{b^3 t^3}{4} - \frac{b t^3}{2} + \frac{t^6}{4} = \frac{c^3}{4}$$

$$\sqrt{\left(\frac{b^3 t^3}{4} - \frac{b t^3}{2} + \frac{t^6}{4}\right)} = \sqrt{\left(\frac{c^3}{4}\right)}$$

subtracting  $\frac{b^3}{27}$  internally to the parenthesis

$$\sqrt{\left(\frac{b^3 t^3}{4} - \frac{b t^3}{2} + \frac{t^6}{4} - \frac{b^3}{27}\right)} = \sqrt{\left(\frac{c^3}{4} - \frac{b^3}{27}\right)}$$

$$\text{or } \left(t^3 - \frac{b}{3}\right) \times \sqrt{\left(\frac{t^3}{4} - \frac{b}{3}\right)} = \sqrt{\left(\frac{c^3}{4} - \frac{b^3}{27}\right)}$$

adding  $\frac{b t - t^3}{2}$  to one side of the equation, and its equal  $\frac{c}{2}$  to the other side,

$$\frac{b t - t^3}{2} + \left(t^3 - \frac{b}{3}\right) \times \sqrt{\left(\frac{t^3}{4} - \frac{b}{3}\right)} = \frac{c}{2} + \sqrt{\left(\frac{c^3}{4} - \frac{b^3}{27}\right)}$$

extracting the cube roots

$$-\frac{t}{2} + \sqrt{\left(\frac{t^3}{4} - \frac{b}{3}\right)} = \sqrt[3]{\frac{c}{2}} + \sqrt[3]{\frac{c^3}{4} - \frac{b^3}{27}}$$

No other value can be used in this case for the cube root of the binomial, which the algebraist may readily prove by adapting it to an irreducible equation where there is no ambiguity in respect of the square root. Such is the equation  $x^3 - 63x = 162$ , where the binomial is  $81 + \sqrt{-2700}$ , and the cube root for  $t$  is  $-3 + \sqrt{-12}$ .

To obtain the imaginary quantity which is the subject of consideration, I employed the reducible equation  $x^3 - 24x = 72$ , where  $x = 6$ ,  $t = 3 + \sqrt{-3}$ ,  $u = 3 - \sqrt{-3}$ ; and by Cardan's rule the roots of the equation are thus expressed:—

$$\sqrt[3]{36 + \sqrt{784}} + \sqrt[3]{36 - \sqrt{784}}$$

and by the previous demonstration, the cube root of the binomial on the left hand connected with  $t$  is the quantity I gave; namely,

$$-\frac{3 + \sqrt{-3}}{2} + \sqrt{-\left(\frac{13}{2} - \frac{3}{2}\sqrt{-3}\right)}.$$

Algebraists universally give precedence in magnitude to the binomial on the left hand; and in this they follow the old masters,

It would be strange indeed to call the first binomial  $\sqrt[3]{8}$ , and the latter  $\sqrt[3]{64}$ .

The binomial on the left hand being, then, by common consent and usage, equal to  $\sqrt[3]{36 + 28}$  or  $\sqrt[3]{64}$ , it follows that my number is a true cube root of 64, and not of 8, which your correspondent N. R. D. affirms it to be, and that I have properly, and in conformity with the practice of algebraists, taken the positive square root of 784.

I conceive, therefore, that I have now only to show that the quantity is different from the known forms of the cube roots of 64. Dr. Tiarks has divided  $x^3 - 64$  by  $x - 4$ , and by means of the quotient he obtains  $-2 \pm \sqrt{-12}$ , which are the cube roots connected with the equation  $x^3 - 48x = 128$ , where by Cardan's rule the roots are represented by  $\sqrt[3]{64 + \sqrt{4096 - 4096}} + \sqrt[3]{64 - \sqrt{4096 - 4096}}$ , and where, by the roots previously exhibited depending on  $t$  and  $v$ , the cube roots become  $-2 \pm \sqrt{-12}$ ; but these are the cube roots of binomials in their vanishing state, in which state they have functions and connexions widely different from those deduced from binomials which are not evanescent.

The means taken by Dr. Tiarks to prove my quantity to be equal to  $-2 - 2\sqrt{-3}$  is by no means sufficient.

This, as well as the correctness of my assertion, may be sufficiently evidenced by the nature of vanishing fractions; and on this evidence, and not on any ambiguity of expression, I entirely rest my opinion.

If binomials are not in a vanishing state, one of the roots of the equation from which the binomials are deduced will, by a simple operation, become extinct; but all the roots will be preserved if the binomials are evanescent.

$$\text{Thus let } 3x - x^3 = 2$$

$$x - x^3 = 2 - 2x$$

$$\text{or } x - x^3 = 2 \times \frac{1-x}{1-x}$$

$$\therefore \frac{x-x^3}{1-x} = x^2 + x = 2$$

Here the roots are preserved, because the binomials connected with the given equation vanish. But

$$\text{Let } 7x - x^3 = 6$$

$$x - x^3 = 6 - 6x$$

$$\text{or } x - x^3 = 6 \times \frac{1-x}{1-x}$$

$$\therefore \frac{x-x^3}{1-x} = 6$$

Here the value of unity is extinct, because the binomials

$\sqrt[3]{-3 + \sqrt{-\frac{109}{27}}}$ ,  $\sqrt[3]{-3 - \sqrt{-\frac{109}{27}}}$ , do not vanish. The cube roots in respect of the root unity are  $\frac{1}{3} - \sqrt{-\frac{1}{3}}$  and  $\frac{1}{3} + \sqrt{-\frac{1}{3}}$ ; but if, under this conception, we should assimilate the sum of these roots to the root of the equation  $3x - x^3 = 2$ , a greater mistake, in my opinion, could not be made. In the same manner may my quantity be divided by 4, and it will be a cube root of unity, but never can it be conceived to be a root of the equation  $3x - x^3 = 2$ ; but if Dr. Tiarks's number be so divided, it will be, together with  $-\frac{1}{4} + \sqrt{-\frac{3}{4}}$ , a root of the equation  $x^3 - 3x = 2$ . The equations  $7x - x^3 = 6$  and  $3x - x^3 = 2$  have a similar root unity; but it is seen that all equality is lost when they are converted into fractions, and this is precisely the case of our two numbers. It is the province of the lovers of the science to decide on the question.

I am, Sir, your obedient servant,

May 9, 1815.

JAMES LOCKHART.

*Another Communication on the same subject.*

(To Dr. Thomson.)

SIR,

May 5, 1815.

As the subject proposed by Mr. Lockhart on the algorithm of imaginary quantities is one of considerable importance in a variety of analytical investigations, you will be induced probably to admit a few remarks on the two answers published in your last number.

The first thing which appears singular is, that one of your correspondents has shown Mr. Lockhart's expression to be the cube root of 64, but under a different form to that usually given; and the other, that it is not the square root of 64, but of 8.

The fact is, that Mr. L.'s expression,  $-\frac{3 + \sqrt{-3}}{2} + \sqrt[3]{-\left(\frac{1}{2} - \frac{1}{2}\sqrt{-3}\right)}$ , the same as all other quantities in which the sign of the square root enters, admits of two values; and as there is no previous condition, either of them may be employed; and the quantity will be accordingly either the  $\sqrt[3]{64}$  or  $\sqrt[3]{8}$ . R. N. D. is therefore too positive when he says, "it is not the cube root of 64, but of 8." He is also wrong in stating that by squaring  $a\sqrt{b}$  an ambiguity is introduced; for the ambiguity has place in the  $\sqrt{b}$  before the operation of squaring; in fact, the only case in which there is no ambiguity is when we know the origin of the quantity whose root is to be extracted, as is shown in one of the latter numbers of Nicholson's Journal, where the object was to explain why  $\sqrt[3]{\frac{1}{2} - \frac{1}{2}\sqrt{-3}} + \sqrt[3]{\frac{1}{2} + \frac{1}{2}\sqrt{-3}}$ , which is known to be equal to  $1.87938$ , or  $2 \sin. 70^\circ$ , is not (when squared by the usual process) equal to the square of the same number. The

square of  $\sqrt[3]{\frac{1}{2} + \frac{1}{2}\sqrt{-3}} + \sqrt[3]{\frac{1}{2} - \frac{1}{2}\sqrt{-3}} = -$   
 $(\sqrt[3]{\frac{1}{2} - \frac{1}{2}\sqrt{-3}} + \sqrt[3]{\frac{1}{2} + \frac{1}{2}\sqrt{-3}}) + 2$ , which is cer-  
 tainly not equal to  $1.879381^2$ .

Query the cause of this anomaly, if we admit the ambiguity to arise as stated by your Correspondent R. N. D.?

Yours, &c.

MATHEMATICUS.

## ARTICLE IX.

### *Astronomical and Magnetical Observations at Hackney Wick.* By Col. Beaufoy.

Latitude,  $51^{\circ} 32' 40.3''$  North. Longitude West in Time  $6^{\text{h}} \frac{33}{100}$ .

April 26, Emerision of Jupiter's	11 <sup>h</sup>	8'	50"	Mean Time at Hackney Wick.
1st Satellite .....	11	8	57	Ditto at Greenwich.
May 4, Immersion of Jupiter's	10	1	49	Ditto at Hackney Wick.
3d Satellite .....	10	1	56	Ditto at Greenwich.
May 4, Emerision of Jupiter's	12	41	07	Ditto at Hackney Wick.
3d Satellite .....	12	41	14	Ditto at Greenwich.
May 12, Emerision of Jupiter's	9	26	14	Ditto at Hackney Wick.
1st Satellite .....	9	26	21	Ditto at Greenwich.

### *Magnetical Observations.*

1815.

Month.	Morning Observ.			Noon Observ.			Evening Observ.		
	Hour.	Variation.		Hour.	Variation.		Hour.	Variation.	
April 18	8 <sup>h</sup> 25'	24°	15' 33"	—	—	—	6 <sup>h</sup> 45'	24°	20' 06"
Ditto 19	8 35	24	15 26	1 05	24 25 58	—	6 40	24 18 37	—
Ditto 20	8 35	24	17 53	1 25	24 29 16	—	—	—	—
Ditto 21	8 30	24	17 56	—	—	—	6 25	24 19 31	—
Ditto 22	8 40	24	16 54	1 45	24 25 46	—	—	—	—
Ditto 23	8 35	24	16 05	1 40	24 27 06	—	6 25	24 19 02	—
Ditto 24	8 30	24	15 50	—	—	—	6 25	24 20 15	—
Ditto 25	8 35	24	16 04	—	—	—	—	—	—
Ditto 26	8 35	24	17 32	1 05	24 28 49	—	—	—	—
Ditto 29	8 20	24	19 27	—	—	—	6 35	24 22 18	—
Ditto 30	8 20	24	16 30	1 35	24 29 21	—	6 50	24 28 00	—

## Magnetical Observations continued.

1815.

Month.	Morning Observ.			Noon Observ.			Evening Observ.		
	Hour.	Variation.		Hour.	Variation.		Hour.	Variation.	
May 1	8 <sup>h</sup> 40'	24° 14' 50"		1 <sup>h</sup> 55'	24° 29' 12"		6 <sup>h</sup> 50'	24° 20' 22"	
Ditto 2	8 30	24 20 00		1 20	24 27 24		—	—	—
Ditto 3	8 35	24 17 28		—	—		—	—	—
Ditto 4	8 35	24 17 02		1 55	24 22 12		6 25	24 18 24	
Ditto 5	8 40	24 16 00		1 05	24 25 37		6 36	24 19 11	
Ditto 6	8 40	24 14 56		1 10	24 26 46		6 40	24 18 41	
Ditto 7	8 35	24 14 59		1 35	24 28 12		6 35	24 19 08	
Ditto 8	8 40	24 16 56		—	—		—	—	—
Ditto 10	8 35	24 20 55		1 35	24 26 26		6 25	24 20 55	
Ditto 11	8 25	24 14 30		—	—		—	—	—
Ditto 14	8 35	24 16 55		1 40	24 24 55		7 10	24 19 26	
Ditto 15	8 35	24 16 05		1 25	24 28 33		—	—	—
Ditto 16	8 45	24 17 28		—	—		7 10	24 18 21	
Ditto 17	8 30	24 17 12		1 40	24 26 58		7 10	24 20 37	

		1813.	1814.	1815.
April.....	{ Morning .....	24° 09' 18"	24° 12' 53"	24° 16' 01"
	{ Noon .....	24 21 12	24 23 53	24 27 42
	{ Evening.....	24 15 25	24 15 30	24 17 48

*April.*—The variation of the weather this month has been equalled by the uncommon variation of the needle.

Rain fallen { Between noon of the 1st Apr. } 1.735 inch.  
 { Between noon of the 1st May }  
 Evaporation during the same period ..... 1.90

## ARTICLE X.

## ANALYSES OF BOOKS.

*Transactions of the Geological Society, Volume 2d. London, William Phillips, 1814.*

This volume contains 24 papers, and is accompanied by a volume of plates.

I. *On certain Products obtained in the Distillation of Wood, with some Account of Bituminous Substances, and Remarks on Coal.* By J. Macculloch, M.D. F.L.S., Chemist to the Ordnance, and Lecturer on Chemistry at the Royal Military Academy at Woolwich, and Vice President of the Geological Society.—

When wood is exposed to destructive distillation there comes over a thick black fluid like tar. Great quantities of this substance are obtained at the powder works from the distillation of willow and alder. It was this substance that Dr. Macculloch examined. It is very inflammable, and may be burnt like oil in a lamp. When it is washed in water, that liquid separates a considerable proportion of acetic acid, coloured by an oily matter, which has an empyreumatic smell and taste. When triturated, or boiled with carbonate of potash, it acquires a pitchy consistence; but does not seem to combine fully with the alkali. It is soluble in alcohol, ether, caustic fixed alkaline lees, acetic acid, and the mineral acids. Fat oils and fresh essential oils dissolve it imperfectly; but the drying oils and inspissated essential oils act on it more readily. Coloured oil of turpentine dissolves a good deal of it. Naphtha has scarcely any action on it. When exposed to a heat just sufficient to keep it boiling, an oil comes over, at first light coloured, but becoming darker as the process advances. If the heat of the retort be gradually increased to redness, nothing remains but a spongy charcoal. There is found in the receiver an oil and acetic acid, combined with a little ammonia. No gas is evolved in this process, if the heat be carefully managed. When a gas is formed, the oil has been exposed to too high a temperature, by letting the fire act too much on the upper part of the retort. If the heat be continued for a certain time, what remains in the retort resembles petroleum; if longer, maltha; if longer, bitumen; and if still longer, only coal remains. But these resemblances are merely external. Bitumen and our substance are different in their composition; since the first is soluble in naphtha, the second insoluble. Dr. Macculloch conceives it probable that vegetables may have been converted into bitumen or jet, by the action of water, and the bitumen afterwards converted into coal by heat. He shows that heat is incapable of bituminizing wood, but that it converts jet into coal. Many other very ingenious hypotheses respecting the origin of the various species of coal and plumbago occur in this paper; but they are of so bold a nature, and so little supported by the present state of our knowledge, that I am afraid to enter upon them, lest they should lead to a tedious controversy about a subject, the decision of which is at present obviously beyond the reach of our faculties. The valuable part of the paper is the chemical description of the new substance obtained by distilling vegetables, which Dr. M. proposes to distinguish by the name of *bistre*.

II. *Mineralogical Account of the Isle of Man.* By I. F. Berger, M. D. M. G. S.—The Isle of Man, the situation of which is too well known to require specification here, is rather more than 30 miles long from north to south, while its breadth varies from 15 to eight miles. The northern extremity is tolerably low for about five miles, where a range of mountains commences that proceed to the southern extremity. This group of mountains consists of three

chains, separated from each other by high table lands. Three very narrow openings cross the group from east to west. The furthest north of which lies between Douglas Town and Peel Town; and the furthest south is the narrow channel between the island and Calf of Man. The first of these is highest, and the last lowest, being under the level of the sea. The highest mountain in Man is Snowfield, which is 2000 feet above the level of the sea. Dr. Berger has given the elevation of 89 mountains and spots of this island, which has enabled him to give interesting sections of the island in various directions. The Calf of Man is 206 feet high. The mean annual temperature he reckons from the heat of 31 springs, at  $49.99^{\circ}$ , or  $50^{\circ}$ , Fahrenheit.

Very little granite has been observed in Man. It occurs, however, in two or three spots; but most likely only forming beds in the clay-slate, which constitutes the central and highest part of the island. Grey-wacke covers the lower parts of this clay-slate, almost surrounds it, and constitutes the sea-coast both on the east and west side of Man. This grey-wacke sometimes contains beds of grey-wacke slate and flinty slate. It has not been observed to contain any animal remains. A floetz lime-stone containing madrepores, and some univalve shells, occupies the south-east coast; except a peninsula of old red sand-stone, which occurs likewise at Peel Town. The Calf of Man is grey-wacke. Veins containing galena occur in three different parts of the island, and always in the grey-wacke. Sulphuret of copper likewise occurs. These veins have been wrought more than once, but are at present abandoned. Dr. Berger thinks that the southern part of the island has at some former period subsided. Hence he accounts for the gradual declivity, the dip, and the narrow valleys. The population is reckoned at 28,000; but Mr. Curwen thinks it does not exceed 23,000.

III. *On the Granite Tors of Cornwall.* By Dr. Macculloch.—The Logging Rock, and Cheese Wring in Cornwall, and the Vixen Tor in Dartmoor, exhibiting curious configurations of granite rocks, are described and figured; and Dr. Macculloch shows very clearly that they have resulted from the natural effects of the disintegration of the rock, and that they have not, as some supposed, been formed artificially. He terminates the paper with some speculations on the origin of granite, and rather inclines to its igneous origin. Into these speculations I think it unnecessary to enter; because I conceive them to be entirely beyond the reach of the human faculties. Supposing a man to spend his life in guessing how the Almighty brought the earth into its present state, and supposing him even to guess right, (no very probable supposition) I ask by what means he could prove his conjecture? The only conclusive evidence in such a case is historical evidence, and where could it be found? *Geology* does not consist in speculating about the origin or creation of the earth, it consists simply in determining the number, nature, and position of all the rocks.

which constitute the surface of the earth; and till it is confined to this, it can never become a useful nor correct science. If you can demonstrate by petrifications the epochs of the formation of rocks, or the changes they must have undergone, do so; but never reason or speculate from the mere love of hypothesis.

I may just observe for the consideration of Dr. M., that nothing is more common than to find sand-stone having exactly the structure of his Cornish granite, I mean decaying ultimately into balls. As examples, I may mention a sand-stone in the Isle of Skye, a sand-stone in Fifeshire, and another in Dumfries-shire. It follows, therefore, that if the Doctor's reasoning about the igneous origin of granite be accurate, he must admit likewise the igneous origin of sand-stone.

IV. *Notes on the Mineralogy of the Neighbourhood of St. David's Pembrokeshire.* By John Kidd, M. D. Prof. Chem. in the University of Oxford, M. G. S.—It would appear from Dr. Kidd's description, that the fundamental rock of this district is a syenite, which seems in some cases to pass into granite, in others to porphyry; but, unfortunately, the descriptions are not sufficiently minute to make us sure of these transitions. Beds or rocks of these three different species, however, occur here. Over the syenite, and, in general, covering the flat country, lie beds of clay-slate, which vary a good deal in their appearance. This slate is often black, has little lustre, and contains quartz veins running through it; hence it is probably a transition slate. Dr. Kidd calls it *grey-wacke*; but that term never can be applied correctly to a slate. Indeed it is quite obvious from Dr. Kidd's observations, that the term *grey-wacke* has no fixed meaning in his mind: yet it is as well defined a rock as any other, and any mineralogist in five minutes might make himself so well acquainted with its characters, as afterwards to run no risk of confounding it with any other. The following definition may be of some use.

"*Grey-wacke of Werner is a conglomerated rock with a basis of clay-slate. It contains portions or fragments of clay-slate, flinty-slate, quartz; and sometimes grains of felspar and scales of mica.*"

If Dr. Kidd dislikes the term *grey-wacke*, he may substitute *transition sand-stone*. The word *grey-wacke* was invented by the miners at Freyberg, where the rock abounds. Werner adopted it from them, and drew up an accurate description of the rock. Very good specimens of it are to be found in the Pentlands and Lamermuirs, near Edinburgh. About 100 yards east of the fourth milestone from Truro in Cornwall, there is a quarry of excellent *grey-wacke*, which is employed in mending the road. Many other British localities might be given; but from either of these two it would be easy to bring up a sufficient number of specimens to London, to make every mineralogist familiar with the appearance of this rock. I conceive that the structure of the country described in this paper by Dr. Kidd is similar to that of the

Criffe in Galloway, of which an account will be found in the *Annals of Philosophy*, vol. iii. p. 465.

V. *An Account of the Brine Springs at Droitwich*. By Leonard Horner, F.R.S. M.G.S.—Salt has been made at Droitwich in Worcester for above a thousand years. The structure of the country, as far as it has been ascertained, is this. Uppermost a bed of sand-stone, usually red, but sometimes greenish, and then containing veins of gypsum. Mr. Aikin and Mr. Horner consider it as old red sand-stone. Under this lies a bed of gypsum about 150 feet thick: below this a river of brine 22 inches deep. Lastly, there lies a bed of rock salt which has never been penetrated. If the sand-stone be old red sand-stone, the position of the gypsum and rock salt is uncommon, as these beds have hitherto been observed only above the red sand-stone and never below it. There are four pits at present used at Droitwich, and the quantity of brine which flows out is much greater than can be consumed. The quantity of salt manufactured annually is about 16,000 tons. This is chiefly consumed in England, and pays a duty of 320,000*l.* The present market price of the salt is 31*l.* per ton, 30*l.* of which is duty.

The specific gravity of the brines of the different pits is as follows:—

Walker's pit .....	1.20611
Walwyn's pit .....	1.20383
Romney's pit .....	1.20015
Stuckey's pit .....	1.18467
Farley's pit .....	1.17471

The last pit is not at present in use, and is probably somewhat mixed with surface water. None of them are perfectly saturated; for water saturated with salt at 55° is of the specific gravity 1.21. Walker's pit ought to contain, by my experiments, 28.88 per cent. of salt; or three ounces of it hold in solution 470 grains of salt. Mr. Horner, however, obtained by evaporation only 431.86 grains; but it is well known to chemists, that the whole of the salt dissolved in water cannot be recovered by evaporation. However carefully the evaporation be conducted, a portion of the salt always makes its escape. The quantity of salt obtained by evaporation from four cubic inches of each of the pits, by Mr. Horner, was as follows:—

Walker's pit .....	317.14 grains.
Walwyn's pit .....	313.40
Romney's pit .....	341.00
Stuckey's pit .....	283.50
Farley's pit .....	266.34

The salt was dried in the temperature of 160°. The constituents of this salt Mr. Horner found as follows:—

Common salt .....	96.48
Sulphate of lime .....	1.63
Sulphate of soda .....	1.82
Muriate of magnesia .....	0.07

100.00

I doubt whether Mr. Horner's criterion of the absence of sulphate of magnesia be sufficient. After the removal of the lime he added ammonia without obtaining any precipitate. I think it probable, that a small quantity of sulphate of magnesia, held in solution in a complicated saline liquid, would not precipitate with ammonia, in consequence of the solubility of the triple salt formed. But I have not tried the experiment.

VI. *On the Veins of Cornwall.* By William Philips, M. G. S.—The ore in Cornwall occurs commonly in veins which have an east and west direction, and are called *lodes*. These are traversed by north and south veins called *cross courses*; and sometimes by north-east and south-west veins, called *contres*. The relative antiquity of these veins appears in general to be in the order in which they have been named. The lodes are the most important; they contain tin and copper ore. The tin ore is commonly towards the upper part, and the copper towards the lower part of the lode. No one of these lodes has been traced further than two miles; yet there can scarcely be a doubt that they traverse the whole county, and even pass through Devonshire, till they lose themselves in the channel. No vein has been followed to its greatest depth. Their most common breadth is from one to three feet. They are very rarely 30 feet wide. When a cross course cuts a lode it generally changes its direction, or *heaves* it, as the miners term it. The *contres* do the same thing to both the others. The *cross courses* are usually filled with quartz; though sometimes likewise they contain ore.

There is a low range of hilly country that traverses Cornwall just on the south of the principal veins. This range, as far as I observed, was granite. On the north and south side this granite is covered by clay-slate, which goes to an unknown depth. The veins usually traverse the clay-slate, and when they are followed far enough, they are found likewise entering the granite, which spreads itself below the clay-slate. Horn-stone porphyry likewise passes through the clay-slate, but whether in beds or veins has not been determined. It is called *elvan* by the miners. The top of a lode is usually called *gossan*, being filled with a yellow ochrey matter to which that name is given; and lodes have received the following names, according to the prevalent substance with which they are filled:—*Gossany*, when abounding in gossan: *sparry*, when abounding in quartz, or fluor spar, both of which are called spar, in Cornwall. The latter commonly green spar or sugar spar: *mundicky*, when abounding in iron pyrites: *peachy*, when abounding in chlorite: *fleuckany*, when filled chiefly with a kind of clay.

*scovany*, when composed of a mixture of quartz and chlorite; not very hard: *calpy*, when composed chiefly of hard quartz, coloured by a little chlorite: *pryany*, when the ore does not occur in a compact state; but mixed with the constituents of the lode: *grauany*, when composed chiefly of granite.

VII. *On the Fresh Water Formations in the Isle of Wight, with some Observations on the Strata over the Chalk in the south-east Part of England.* By Thomas Webster, M. G. S.—One of the most remarkable additions which geognosy has of late years received, is an accurate description of the beds which cover the chalk in the neighbourhood of Paris. These beds abound in petrifications; by a careful examination of which it has been ascertained that some of these beds have been formed at the bottom of the ocean, while others, placed above them, have been formed at the bottom of a fresh-water lake; and these alternations are repeated more than once: so that the sea appears to have twice covered the environs of Paris, to have twice retreated while its place was supplied by a fresh-water lake. Immediately over the chalk lie the following beds: 1. plastic clay; 2. coarse lime-stone and sand-stone; 3. siliceous lime-stone, which have been formed at the bottom of the sea. Next comes a bed of gypsum, marl, &c. containing only bones of land animals and fresh-water shells, and therefore formed at the bottom of a fresh-water lake. It constitutes the lower fresh-water formation. Then come beds of marl, containing only sea shells, and therefore of marine formation. It constitutes the upper marine formation. The sixth bed consists of sand and sand-stone, without shells; the seventh a sand-stone containing sea shells; the eighth, the buhr or mill-stone formation, without shells, and argillaceous sand. Then comes the upper fresh-water formation, comprehending marls and buhrs, with fresh-water shells. This last formation is covered by alluvial soil. For a particular description of these formations we are indebted to Brogniart and Cuvier.

Though the south-east of England bears this striking resemblance to the north of France, where the Paris basin is situated, that its basis is chalk, which is covered in various places by different beds, yet Mr. Webster was the first person who pointed out a similarity in the formations in certain parts of the south-east of England and those which fill up the Paris basin. The two places where the resemblance is greatest are the Isle of Wight basin and the London basin. In the Isle of Wight Mr. Webster traced an alternation of sea and fresh-water formations similar to those near Paris, and characterized by the very same fossils. The beds, indeed, are not exactly the same in both, though there is a certain degree of resemblance between them. These different formations can be distinctly seen at Headen Hill, upon the north side of Alum Bay, not far from the Needles, on the west coast of the Isle of Wight. Sir Henry Englefield first discovered a range of chalk-hills running west through the middle of the Isle of Wight. These hills

terminate on the south side of Alum Bay, and on the north side of the chalk there occur a great number of perpendicular beds of clay and sand, often containing abundance of loose pebbles. Mr. Webster conceives it impossible that these beds could have been formed in the position which they now occupy. He supposes that they were at first placed horizontally over the chalk, and that they were thrown into their present position by some unknown convulsion. Immediately to the north of these beds is Headen Hill, composed of beds nearly horizontal. These beds Mr. Webster considers as formerly lying over the perpendicular beds of Alum Bay, before they acquired their perpendicularity.

The perpendicular beds at Alum Bay are analogous to the lower marine formation in the Paris basin; for all the fossils which they contain, though different from those in the chalk, are of marine origin. The lower fresh-water formation is distinctly seen near the bottom of Headen Hill. It consists of a series of beds of sandy, calcareous, and argillaceous marls, sometimes mixed with brown coal. The thickness of these beds is 63 feet. It contains so many fresh-water shells, and so regularly deposited, that we cannot suppose them to have been carried by rivers into the sea. Besides, in that case we should find a mixture of sea shells, which do not occur in this formation. This is covered by the upper marine formation. It consists of clay and marl, is about 90 feet thick, and contains a prodigious quantity of sea shells. This is covered by the upper fresh-water formation, about 70 feet thick, and consisting of alternate beds of sand, lime-stone, and clay. It contains abundance of fresh-water shells, without any admixture of sea-shells. This formation is covered by alluvial soil.

Mr. Webster conceives the Isle of Wight basin to have been formerly filled with these formations. It consisted of the northern half of the Isle of Wight, extended as far west as Dorchester; and as far east as Shoreham, being bounded on the north by the hills which constitute the South Downs.

The London basin is of much greater extent, but not so well defined. Its southern boundary is marked by Deal, Canterbury, Milton, Chatham, Gravesend, Purfleet, from which it runs south-westerly to Leatherhead and Guildford, and then bending northerly it proceeds as far west as Hungerford. Maidenhead, Eton, St. Alban's, mark part of its northern boundary, and it appears to take in the whole counties of Essex, Suffolk, and Norfolk. The bottom of this basin is chalk. Over the chalk lies a bed of sand, or in some places of plastic clay. Over this lies the London clay, well known for the great number of marine remains which it contains. This clay is mostly covered by a bed of gravel of various thickness, obviously alluvial. Mr. Webster considers the London clay as analogous to the upper marine formation in the Paris basin. The formations formerly placed over the London clay he considers as having been removed by some convulsion.

VIII. *Remarks on the Vitrified Forts of Scotland.* By Dr.

Macculloch.—These forts have been observed in different parts of the north of Scotland and in Galloway. Nothing is known relative to the time of their erection, or the people by whom they were built. Two opinions have been advanced: that they were vitrified on purpose, and that they were vitrified by accident. Dr. Macculloch shows, by an examination of the stones of which they are composed, that the latter opinion cannot be correct. He examined two of these forts: Dun Mac Sniochain, in Argyleshire, and Craig Phadric, in Inverness-shire. In both the walls consist chiefly of primitive rocks. These have been mixed with a considerable quantity of a kind of amygdaloid, which easily fuses, and by the fusion, more or less complete, of this material, the vitrification has been brought about. The amygdaloid differs considerably in its nature at the two forts. At Amworth, in Galloway, there is no amygdaloid; the rocks are grey-wacke and grey-wacke-slate. Some portions of this grey-wacke are fusible, from a mixture of calcareous spar. Accordingly this fort is only vitrified in a few detached spots.

IX. *On the Sublimation of Silica.* By Dr. Macculloch.—He had exposed a mixture of the oxides of tin and lead in a crucible covered by another, for some hours, in a furnace at a heat conjectured to be between  $130^{\circ}$  and  $140^{\circ}$  Wedgewood. On cooling, the top of the uppermost crucible contained some small filamentous crystals, white and brilliant, crossing each other in all directions. They were found to be crystals of pure silica. Dr. M. was not able to repeat this experiment with success.

X. *Observations on the Specimens of Hippurites from Sicily, presented to the Geological Society by the Hon. Henry Grey Bennet.* By James Parkinson, M.G.S.—These specimens are so perfect as to enable Mr. Parkinson to determine some things respecting the original structure of the shells. The supposed operculum mentioned by Picot la Peyrouse could not be distinguished. Mr. Parkinson thinks that the hippurites, orthoceratites, ammonites, &c. had the means of elevating themselves to the surface of the sea, like the nautilus; and some things which he observed in these specimens give countenance to this idea.

(To be continued.)

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## ARTICLE XI.

### *Proceedings of Philosophical Societies.*

#### ROYAL SOCIETY.

ON Thursday, the 27th of April, a paper by Mr. Seppings was read, containing further particulars respecting his improved mode of ship-building. As, immediately after his last paper a paper was read to the Royal Society, containing theoretical objections against a mode of building ships to which in his opinion theory was not

competent, it occurred to him that it would be satisfactory to the Society to lay before them the results which have been obtained by an investigation into the ships built according to his mode. He stated the report of Admiral Durham respecting the *Tremendous*. She was the best sailing ship in his fleet. Three years afterwards she was examined by professional ship-builders, and found not to have suffered any deterioration. Other similar reports were mentioned. Dr. Young's allegation, that the oblique braces were imitated from the French, is, according to Mr. Sepings, inaccurate. The French had indeed introduced some pieces of oblique timber, and they were to be seen in a ship which we took from them; but they had not been attended with any benefit, and were abandoned as useless by the French themselves. Mr. Sepings's mode is quite new, and the benefit of it sufficiently obvious.

On Thursday, the 4th of May, a paper by Sir Humphry Davy was read, on the action of acids on hyper-oxy muriate of potash. In consequence of the discovery of a new acid by Gay-Lussac by treating hyper-oxy muriate of barytes with sulphur, Sir H. Davy was induced to examine more carefully than had hitherto been done the action of acids on the hyper-oxy muriate of potash. When sulphuric acid is poured upon this salt in a wine-glass, very little effervescence takes place, but the acid gradually acquires an orange colour, and a dense yellow vapour of a peculiar and not disagreeable smell floats on the surface. These phenomena led the author to believe that the substance extricated from the salt is held in solution by the acid. After various unsuccessful attempts to obtain this substance in a separate state, he at last succeeded by the following method. About 60 grains of the salt are triturated with a little sulphuric acid, just sufficient to convert them into a very solid paste. This is put into a retort, which is heated by means of hot water. The water must never be allowed to become boiling hot, for fear of explosion. The heat drives off the new gas, which may be received over mercury. This new gas has a much more intense colour than euchlorine. It does not act on mercury. Water absorbs more of it than of euchlorine. Its taste is astringent. It destroys vegetable blues without reddening. When phosphorus is introduced into it, an explosion takes place. When heat is applied, the gas explodes with more violence, and producing more light, than euchlorine. When thus exploded, two measures of it are converted into nearly three measures, which consist of a mixture of one measure chlorine and two measures oxygen. Hence it is composed of one atom chlorine and four atoms oxygen. It is not unlikely that euchlorine is a simple mixture of three measures of chlorine and two measures of this new gas; but the point cannot be determined till it be known whether Dutch foil will burn in such a mixture as it does in euchlorine. This experiment the author could not try, because at Rome, where he then was, he could procure no Dutch foil fit for his purpose. The same gas is disengaged from hyper-oxy muriate of potash by nitric acid, and with still greater facility; but it is always mixed with one-fifth of

its bulk of oxygen gas. Sir H. Davy conceives that the chloric acid of Gay-Lussac contains hydrogen, and owes its acid properties to the presence of this principle.

At the same meeting of the Society, a paper by Dr. Philips was read, giving an account of the remains of a *fœtus* found within the abdomen of a child. The child was aged two years and a half, and had a swelling in the belly, which had been considered as dropsical, and aperient medicines had been administered. The swelling did not diminish; and the child at last was obliged to be kept always in a recumbent posture. Dr. Philips, on examining the abdomen, found a circumscribed swelling on the left side, which at first he considered as an enlarged spleen, but afterwards laid aside that opinion, without being able to form any one in the least satisfactory. The child died on the fourth day after having been seen by Dr. Philips. On opening the abdomen, the intestines were all found in a sound state, except the liver, which was indurated. The tumor consisted of a large mass of matter not connected particularly with any of the intestines, weighing eight or ten pounds, and inclosed in a very vascular bag. On cutting into it, some serous liquid oozed out. The dissection, from the situation of the medical men, was necessarily hurried and imperfect; but unequivocal traces of a *fœtus* were found, particularly the bones of the tibia and talus, and some others, which were found adhering together, and covered with muscle.

On Thursday, the 11th of May, a paper by Mr. Porrett, jun. was read, containing experiments to determine the composition of prussiate of mercury and of the prussic acid with the application of the atomic theory to both, and to the constitution of ferrureted and sulphureted chiazic acids. He dissolved 40 grains of prussiate of mercury in water, and decomposed it by hydro-sulphuret of potash. He obtained 37·2 grains of black sulphuret of mercury. To determine the quantity of prussic acid present in this salt, he dissolved ten grains of it in water, and mixed an hydrogureted sulphuret with the solution, which he had previously ascertained to have the property of converting prussic acid into sulphureted chyazic acid. This acid was then thrown down by means of two parts of sulphate of iron and three parts sulphate of copper; and the sulphureted chyazate of copper being dried and weighed, he was able, from his previous analysis of this salt, and of sulphureted chyazic acid, to determine the quantity of prussic acid which it contained. The result of the analysis was, that prussiate of mercury is composed of

Prussic acid .....	13·2
Red oxide of mercury .....	86·8
	<hr/>
	100·0

To analyze prussic acid he employed the methods pointed out by *Thenard* and *Gay-Lussac* and *Berzelius*; but he simplified them considerably. He used prussiate of mercury, and mixed it with a

quantity of red oxide of mercury, constituting a certain multiple of the red oxide contained in the salt. He found by trials that it was necessary to mix the salt with five times the quantity of red oxide which it contained, in order to obtain a complete decomposition. The two substances were intimately mixed together by trituration, and then introduced in three separate portions into a glass tube close at one end, about eight inches long, and of the size of a goose-quill. Each portion was separated from the succeeding one, by a small quantity of green glass. Another tube of the same size, open at both ends, and previously filled with mercury, was firmly tied to the retort tube by means of a piece of caoutchouc. Its extremity was then introduced into a glass cylinder filled with mercury, and placed as near as possible in a horizontal position on the mercurial trough. The mixture was then successively burnt by means of a spirit lamp, and, when cool, the whole gaseous contents were thrown into the glass jar. The azote evolved was just equal in bulk to the prussic acid decomposed, the carbonic acid formed amounted to twice the bulk of the azote. From these data Mr. Porrett concluded that prussic acid is composed of

Azote .....	40·7
Carbon .....	24·8
Hydrogen .....	34·5
	<hr/>
	100·0

Or of	Azote .....	1 atoms
	Carbon .....	2
	Hydrogen .....	8
		<hr/>
		11

Prussiate of mercury is composed of one integrant particle of prussic acid and one integrant particle of red oxide of mercury. Sulphureted chyazic acid is a compound of 1 atom sulphur + 4 integrant particles of chyazic acid. Ferrureted chyazic acid is a compound of 4 atoms black oxide of iron + 1 atom prussic acid.

#### LINNÆAN SOCIETY.

On Tuesday, May 2, a paper by George Montague, Esq. on the *ardea nigra*, or black stork, was read. This bird was shot in England.

On Wednesday, the 24th of May, the anniversary of the Society, the following office-bearers were elected for the ensuing year:—

PRESIDENT—Sir James Edward Smith, M.D.  
 TREASURER—Thomas Marsham, Esq.  
 SECRETARY—Alex. Macleay, Esq.  
 UNDER SECRETARY—Mr. Richard Taylor.

There were retained of the old Council :—

The President,  
The Lord Bishop of Carlisle,  
Aylmer Bourke Lambert, Esq.  
William Elford Leach, M.D.  
Alex. Macleay, Esq.  
Thomas Marsham, Esq.  
William George Maton, M.D.  
Daniel Moore, Esq. F.R.S.  
Joseph Sabine, Esq. F.R.S.  
Thomas Smith, Esq.

The five following Fellows were elected into the Council :—

Thomas Marquis of Bath.  
William Kent, Esq.  
Rev. Thomas Rackett.  
Thomas Thomson, M.D. F.R.S.  
John Walker, Esq.

Since last anniversary the Society has lost nine Fellows and five Foreign Members by death; and eleven new Fellows have been elected into the Society.

#### GEOLOGICAL SOCIETY.

*April 21.*—A communication from Thomas Hare, Esq. entitled *Observations on Basalt*, with eight illustrative drawings, was read. In the opinion of the author of this paper, basalt is a crystallized substance, formed by deposition from an aqueous solution; its real form is spheroidal, and the columns which it usually presents result from those spheroids being heaped one on another, and from the lateral compression to which each heap is subjected by contiguous and surrounding heaps.

*May 5.*—A paper by the Rev. W. Buckland, M.G.S. entitled *Description of an insulated group of Rocks of Slate and Green-stone*, situated on the east side of Appleby, between Melmerby and Murton, in Cumberland, was read.

The group of rocks here described runs nearly N. and S., and consists principally of slate and green-stone, the slate lying for the most part on the east of the green-stone. The order of the superposition of these two rocks appears to be very indeterminate; sometimes they abut abruptly against each other; sometimes the slate is uppermost, but most generally the green-stone. A few thin beds of blackish transition lime-stone occur in the slate; and in some places the slate is intersected by dykes of compact flesh-red felspar with scales of mica. In another place a more perfectly characterized granite makes its appearance surrounded by green-stone; but whether this is a dyke, or a projecting mass of the subjacent rock, it is not easy to ascertain.

On the east side of the range of rocks above described may be seen a conglomerate (being one of the forms of the old red sand-stone) resting immediately on the slate, and itself serving as the basis for the great lime-stone deposit which erops out in Cross Fell and forms a parallel range to the slate and green-stone. On the western side, however, of this latter range, a very different series of rocks displays itself. First occur strata of lime-stone, either by themselves or with their seams of coal interposed, elevated at a high angle approaching to vertical, and much broken. Then comes in a sand-stone in nearly horizontal beds, extremely different from the conglomerate already mentioned, and, in the opinion of the author of this paper, forming a part of the great deposit which overspreads the vale of Carlisle, a large part of Cheshire, and the vale of York, and in which are found the great quarries of gypsum and the beds of rock salt, a deposition more recent than the magnesian lime-stone which is incumbent on the upper strata of the principal English coal-fields.

#### ROYAL INSTITUTE OF FRANCE.

*Account of the Labours of the Class of Mathematical and Physical Sciences of the Royal Institute of France during the Year 1814.*

I. *Physical Department.* By M. le Chevalier Cuvier, Perpetual Secretary.

#### CHEMISTRY.

ONE of the most curious substances lately discovered is *iodine*, a substance long concealed in kelp, which by heat is converted into a beautiful violet vapour, and which, acting on other bodies in a manner analogous to chlorine, has given new force to the notions originating from the nature of sulphureted hydrogen and from that of chlorine—notions tending to introduce into the theory of chemistry that important modification that oxygen is not the only principle capable of producing *acidity*.

In fact, Berthollet had shown about 30 years ago that sulphureted hydrogen, though it contains no oxygen, possesses all the properties of acids; and the German chemists had dwelt very much upon this fact in their disputes with the French chemists. MM. Thenard and Gay-Lussac made experiments in 1809 showing that it is impossible to extract oxygen from what was called oxymuriatic acid; and that, if it contains oxygen, we must suppose, that in all cases when it is converted into common muriatic acid, water is formed, which unites, and cannot be separated from the acid produced, or at least that the elements of the water enter into the acid as constituent parts; while on the other hand, by regarding oxymuriatic acid as a simple substance which forms muriatic acid by combining with hydrogen, these suppositions are rendered unnecessary. But though our two chemists stated these two hypotheses, they adhered to the

former, which appeared more analogous to what takes place in the greater number of acidifications.

Sir H. Davy, who had been led to the same conclusions, made his decision with greater boldness. He adopted the latter hypothesis, and gave to oxymuriatic acid the new name *chlorine*, from which he derived that of the other acids into which it enters. One of these (*muriatic*), in which it is combined with hydrogen, has been called *hydro-chloric acid*; the other, in which it is combined with oxygen, *chloric acid*.

The experiments on the acid, called *fluoric acid*, led soon after to the notion that it was likewise composed of hydrogen and of a simple body of a peculiar nature, distinguished by the name of *fluorine*. This opinion was first entertained by M. Ampere, lately named a member of the section of geometry.

Thus the property of acidifying hydrogen, or of becoming acid by its means, was admitted in three substances; namely, sulphur, chlorine, and fluorine. Iodine has presented itself as a fourth body.

In our analysis of the labours of last year, we stated that iodine was discovered by M. Courtois. This skilful manufacturer appears to have discovered it about the end of 1811; but he mentioned it only to his friend M. Clement, who did not make it known to the public till towards the end of 1813. But this delay was soon compensated. In a few days M. Gay-Lussac and Sir H. Davy determined its principal properties, pointed out its analogy to chlorine, and described the two acids which it forms, like chlorine, with hydrogen and oxygen. Davy presented this analogy as a new argument in favour of the theory which he had embraced.

Since that period iodine has been examined with that interest to which it is entitled. M. Colin has examined its combinations with mercury and ammonia, and ascertained that it forms *iodic acid* (a combination of iodine and oxygen) whenever it is treated with those oxides in which the oxygen is feebly combined. He explained the generation of the fulminating powder of iodine, first discovered by M. Courtois. Ammoniacal gas is absorbed by iodine, and forms with it a viscid liquid, which changes its nature when put into water. The hydrogen of a part of the ammonia forms with a part of the iodine hydriodic acid, which combines with the rest of the alkali, and the azote of that first portion of ammonia forms with the other part of the iodine the fulminating powder.

The same M. Colin has laboured with M. Gauthier Claubry to determine the way in which iodine acts on organic bodies. These two young chemists have ascertained that those substances in which the oxygen and hydrogen are in the same proportions as in water merely mix with iodine: that those in which there is a greater proportion of oxygen combine with it intimately; but that neither of these bodies is altered, unless a heat be employed capable of decomposing them. On the other hand, those substances in which hydrogen abounds convert iodine into hydriodic acid. The same

thing happens to the first class of bodies when they are sufficiently heated to let go their hydrogen. These experiments presented some curious appearances. A mixture of starch and iodine, when triturated, assumes a red, blue, or black colour, according as the iodine is more abundant, &c.

But our associate Gay-Lussac is the person who has made the most extensive and careful set of experiments on iodine. His paper has been printed in the *Annales de Chimie*. He examines iodine itself, its combinations, and those of its two acids with different bodies. These, according to the established rules of nomenclature, are denominated *iodurets*, *iodates*, and *hydriodates*. He treats likewise of chlorine, and makes some new remarks on its combinations, all of which had not been correctly appreciated. Then considering prussic acid as essentially formed of azote, hydrogen, and carbon, he concludes that azote ought likewise to be added to the list of substances capable of producing acids without oxygen. This leads him to consider acidity and alkalinity as properties belonging to certain bodies and certain combinations, without any necessary relation to their composition, as far as can be discovered, and which of consequence makes it approach to the ideas of Winterl and some German chemists. This memoir is full of delicate investigations and ingenious hints, of which it is not possible to give an account, but which will not fail to give a new spring to the most profound and most important department of chemistry.

Our respectable associate M. Sage, who, notwithstanding his age and infirmities, always takes a lively interest in new chemical facts, has likewise made experiments on iodine, and on kelp, from which it is obtained. He has observed the alteration produced by iodine in the silver vessels in which it is heated. Kelp furnished him, by naked distillation, products analogous to those of animals; and by macerating them in weak nitric acid, he obtained a cartilaginous net, similar to that left by bones and by madrepores when deprived of their earthy parts. M. Sage concludes, from these two facts, that the fuci are polypi.

The same chemist has presented likewise a notice on the advantages of reducing galena by the fire. He affirms that by this method much more lead is obtained than by the ordinary way.

M. Theodore de Saussure, correspondent, who in 1807 had read to the Class a memoir on the composition of alcohol and of sulphuric acid, of which we gave an account at the time, and from which it resulted that ether contains more carbon and hydrogen than alcohol, has last year resumed this important object of investigation, and making use of methods at once more simple and more rigorous, has obtained a more exact result. By passing these two liquids through a red-hot porcelain tube, he converted them into water and a gas, the analysis of which presented no difficulty. By this method he ascertained that alcohol and ether contain each an identical proportion of carbon and hydrogen, and in the same ratio that they

are in olefiant gas, but combined with different proportions of water reduced to its elements.

In alcohol the elements of water form a third of the whole, and in ether they form the fifth part; so that the action of sulphuric acid upon alcohol to produce ether seems to consist only in depriving it of a portion of its water; and the same acid, when applied in greater quantity, produces olefiant gas, by removing the whole of the water.

The analytical results of M. de Saussure agree with those obtained by the late Count Rumford, respecting the quantity of heat produced by combustion of alcohol and ether.

One of the great difficulties in the analysis of organic bodies consists in this, that chemistry possesses but a small number of re-actives capable of separating the immediate principles without destroying them. M. Chevreul, Assistant Chemist in the Museum of Natural History, has endeavoured to render them more useful, by employing very different degrees of heat, and thus varying their solvent power.

For this purpose he has contrived a machine, which he calls a *distillatory digester*, consisting in a Papin's digester, shut by a valve attached to a spring. The force of this spring, which is altered at pleasure, determines the degree of heat which the liquid must receive in order to escape. The produce of each degree is successively collected by means of a tube passing into a receiver. The solid matter under examination is retained in the digester by means of a diaphragm, by which it may be pressed, and all the remaining liquid forced out of it.

M. Chevreul has made experiments on cork by this method. He subjected it 20 times to the action of water, and 50 times to that of alcohol. Having thus separated various substances, there remained a cellular body, which he calls *suberin*, and which, when treated by nitric acid, is converted into *suberic acid*. Among the substances thus extracted from cork, there is one which he considers as new, and which he calls *cerin*, because it possesses several of the properties of wax.

The same chemist has applied his method to amber, and ascertained that succinic acid exists in it in a perfect state.

He has likewise continued his researches on saponification, of which we gave an analysis last year. By comparing fat in its natural state with that which has been saponified, he has concluded that the new properties of the last do not proceed from the separation or addition of any constituent, but from a new mode of combination, occasioned by the action of the alkali, which gives to fat an analogy with the acids independent of all oxygenation.

M. Pelletier, the son of our deceased associate, has examined the colouring matters from sandal wood and *alcanetia* (*enchusa tectoria*), hitherto considered as mere resins. The first, besides possessing most of the properties of resins, is soluble in acetic acid,

even when very weak, and then acts upon gelatine like astringent bodies, and forms oxalic acid when acted on by nitric acid. It possesses, besides, some other characters, which seem to raise it to the rank of a peculiar vegetable principle. The colouring matter of alcanette dissolves in ether, alcohol, and expressed oils. When treated with nitric acid, it gives oxalic acid and bitter principle. Alkalies and water make it undergo various changes of colour. These united properties, in the opinion of M. Pelletier, entitle it likewise to be considered as a peculiar vegetable principle.

We have seen formerly that crude platina, as extracted from the mine, contains several foreign substances, and among others, four peculiar metals, which have been lately described. Last year we described the methods employed by M. Vauquelin to separate *palladium* and *rhodium* (two of these metals form a solution of platinum in nitro-muriatic acid); and to obtain them in a state of purity. We mentioned, likewise, that M. Laugier having perceived that this solution contains a notable quantity of a third metal, remarkable for its volatility, on which account it received the name of *osmium*, had pointed out a method of collecting it.

A black powder, which does not dissolve in nitro-muriatic acid, remained to be examined. It forms the residuum after the solution of crude platinum. It is composed chiefly of *osmium*, and of a fourth metal, to which, on account of the various and lively colours of its solution, the name of *iridium* has been given.

These two metals are united in that powder with chromium, iron, titanium, silica, and even with a little alumina. The difficulty was to separate them completely from this mixture, and to obtain them in a state of purity.

Vauquelin has succeeded in this, but by laborious and complicated processes.

Simple washing divides this powder into two parts; one, finer and more brilliant, contains more iridium and osmium, and scarcely any chromium; the other, browner and coarser, contains less of the first two metals and more of the others. As this last portion is the most difficult to analyze, we shall confine ourselves to it.

M. Vauquelin triturates it in the first place with twice its weight of nitrate of potash. The oxygen of the acid oxidates the iridium and osmium, which combine with the disengaged potash. The application of heat disengages a great part of the acid and the osmium, which are received in lime-water. The residue, diluted and saturated with nitric acid, gives a precipitate of iridium, titanium, iron, alumina, and a little oxide of chromium, and there remains a liquid composed of potash united to chromic acid and to osmium. This last metal is separated by adding nitric acid and distilling, receiving the osmium in a flask surrounded by ice. A little muriatic acid is poured into the water containing the osmium. A plate of zinc is then introduced, which precipitates the osmium. To obtain it quite pure it is washed with water acidulated with a little sulphuric acid.

The chromium is next to be separated. For this purpose the liquid is evaporated, the residue dissolved in water, and filtered, to separate the silica that may be present. Pronitrate of mercury is then poured in, which occasions a precipitate of chromate of mercury, which, being dried and heated, gives the green oxide of chromium. There remains for examination the first precipitate of iridium, titanium, iron, chromium, and alumina. There is likewise a little osmium, which is removed by digesting in muriatic acid, distilling, and precipitating by zinc as before. If there remain portions undissolved, they must be triturated with nitre, as at first; and we observe that the oftener this operation is repeated the more blue do the muriatic acid solutions become, because they contain less and less iron and titanium, which, as more easily dissolved, are first acted upon by the acid, and leave behind them a greater proportion of iridium.

Now iridium has this property: when in that state of oxidation that it forms red solutions in acids, it is only precipitated by sal-ammoniac, and that in the state of a triple salt. It is therefore brought to this state by boiling its muriatic solution with nitric acid. The liquid is neutralized by ammonia. By boiling, the iron and titanium are thrown down. The iridium is then precipitated by sal-ammoniac; and the triple salt obtained, when exposed to a red heat, leaves iridium in a state of purity.

This metal, so difficult to separate from the singular alloy which concealed it from all eyes, possesses remarkable properties. Its colour and lustre are very similar to those of platinum. It is more difficultly fusible. It is insoluble in the simple acids, difficultly soluble in nitro-muriatic acid; but potash and nitre oxidize it, and combine with it into black powder, which gives a blue-coloured solution. With boiling nitro-muriatic acid, it forms a red solution. Its blue solutions themselves become red when boiled; but both the blue and the red solutions are discoloured by sulphate of iron, sulphureted hydrogen, iron, zinc, and tin. Oxymuriatic acid causes them to resume their colour. It is iridium which gives a red colour to the last precipitates of the triple salt of platinum, while the first precipitates, into which it does not enter, are yellow.

The properties of osmium are not so easily determined, in consequence of the ease with which it is oxidated and volatilized. Its oxide is white, and very caustic. It exhales an unsupportable odour. It is flexible and fusible, like wax; and as soon as it touches an animal matter, it blackens it. Its solution in water becomes blue by nutgalls, &c.

M. Mongez, member of the Class of Ancient Literature, has read a memoir on the bronze of the ancients, in which he shows, from experiments made by M. Darcet, that it is not by immersion in cold water that bronze becomes hard, as is the case with steel; but that it acquires its hardness by being heated red-hot, and then allowed to cool slowly in the air. M. Darcet has taken advantage of this property to make symbols, instruments hitherto made only in

Turkey, and, it is pretended, by a single workman in Constantinople, who possesses the secret.

The falls of stones from the atmosphere, since the reality of the phenomenon has been constated, are observed so often, that by and by the most astonishing thing will be the long incredulity entertained respecting them. A remarkable fall took place this year in the department of the Lot-et-Garonne. It happened on the 5th of September, and, as usual, in fine weather, with a strong explosion and a whitish cloud. The number of stones was considerable; one of them was said to weigh 18 pounds. They were scattered over a surface of about a league radius. Their external characters and composition are absolutely the same as in other stones of the same origin, only their fracture has tips a little more marbled than common. Excellent reports by two good observers at Agen, MM. de Saint-Amans and Lamouroux, have made us fully acquainted with the details of the phenomenon.

M. le Comte Berthollet has presented to the Class, on the part of Mr. Teanant, one of the stones that fell last year in Ireland, and which resemble all the others, excepting that they contain a little more iron.

It is known, and we have had repeated occasions to mention it, that the stone called arragonite furnished the strongest objection that could be produced against the employment of crystallization in the classification of minerals; because chemists had found no difference between its composition and that of calcareous spar, though the crystalline forms be essentially distinct. This objection is now removed. M. Stromeyer, Professor of Chemistry at Gottingen, has discovered the constant presence of three per cent. of strontian in arragonite, while none exists in calcareous spar. M. Laugier, Professor in the Museum of Natural History, has repeated this analysis, and obtained the same result. It remains to be explained how the addition of so small a quantity of a constituent can change so completely the form of the primitive molecule of a mineral.

(To be continued.)

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## ARTICLE XII.

SCIENTIFIC INTELLIGENCE; AND NOTICES OF SUBJECTS  
CONNECTED WITH SCIENCE.

### I. *River Missouri.*

The river Missouri, which was navigated in 1805 and 1806 by Captains Louis and Clarke from its junction with the Mississippi to its source, runs a course east and south of above 3000 miles. It rises in a very elevated group of mountains situated between north latitude  $44^{\circ}$  and  $45^{\circ}$ , and about west longitude  $112^{\circ}$ . The height

of these mountains is unknown; but as their summits are perpetually covered with snow, we are sure that it at least exceeds 8000 feet. It runs in a northerly direction for nearly three degrees of latitude; then easterly, with still a northerly inclination, for six degrees of latitude; then nearly south; afterwards south-east; and lastly, nearly east, over a space occupying nine degrees of latitude and 13 degrees of longitude. Its size is fully as great above a thousand miles before it joins the Mississippi as at the junction; yet a great number of large rivers join it in the interval. This shows us the great evaporation to which it is subjected. It joins the Mississippi nearly in north latitude  $39^{\circ}$ , west longitude  $90^{\circ}$ , from Greenwich. The Mississippi, after this junction, flows for  $10^{\circ}$  of latitude south, a course, including the windings, certainly not so little as 2000 miles; so that the whole course of the Missouri, from its source to the ocean, exceeds 5000 miles. This is a length, of course, that will not easily be paralleled any where else; and almost the whole of this immense river is navigable. What is still more important, a great part of its banks consist of fertile plains; and from the observations of Lewis and Clarke, it would appear that a coal country occupies about a thousand miles of these plains. What a country is this likely one day to become!

## II. *An Analogy respecting Volcanoes.*

The phenomena of volcanoes are some of those that have hitherto baffled the sagacity of philosophers. Hence I conceive that every analogy which has any tendency to throw light upon their origin, however insignificant in itself, ought not to be neglected. This induces me to venture to state the following one. Some time ago I got a salt from Mr. Trimmer, which is sublimed during the burning of London bricks. Mr. Trimmer informed me that the sand which is mixed with the clay in the bricks is brought from below Woolwich, and is therefore in all probability impregnated with common salt. This will account for the acid in the sal-ammoniac. I was at a loss to account for the decomposition of the common salt; but a correspondent from Bristol (*Annals of Philosophy*, v. 157,) suggests that sulphate of ammonia is generally formed during the combustion of coal. This must no doubt be the case, as pyrites is almost a constant companion of pit coal. Here, then, we have during the burning of bricks containing common salt by common pit coal the sublimation of sal-ammoniac. Now it is well known that sal-ammoniac is sublimed from Mount Vesuvius. Hence is it not likely that the fuel which supports Vesuvius is coal, and that sea water has access to it? This supposition has been made long ago; but the preceding analogy seems to me to strengthen it. Would it not be proper to ascertain whether sal-ammoniac be sublimed from all volcanoes? and if not, whether there be any thing peculiar in the position and geology of those that yield this salt? If all volcanoes yield sal-ammoniac, I should be inclined to suspect that the quantity of coal contained in the bowels of the earth is much

greater, and that it goes much deeper, than has been hitherto suspected.

### III. *Arragonite.*

It may be worth while to state, that Stromeyer's discovery of the presence of strontian in arragonite has been confirmed more than six months ago at Paris by the experiments of Vogel, Laugier, and Vauquelin, all of whom succeeded in obtaining crystals of nitrate of strontian from arragonite.

### IV. *Whether Cast-Iron expands on congealing.*

In answer to the queries and doubts of my Correspondent N. N. on this subject, I have to observe that Reaumur made a set of experiments on the melted metals, and found that three of them expand in the act of congealing; namely, cast-iron, bismuth, and antimony; while all the rest contract. If you throw a piece (of tolerable size) of gold or silver into these metals in fusion, it immediately sinks to the bottom; but a piece of iron will swim on the surface of the melted mass. When melted iron congeals, the surface is not concave, as it would be if it contracted, but convex. The allowance alluded to by N. N. for the shrinkage is in consequence of the diminution of bulk before congealing, partly from contraction, and partly from leakage. If the iron had congealed before the addition was made, it would be in vain to make it, as the addition would not unite with the old portion.

### V. *On the Production of Complimentary Colours by the successive Reflection of Polarized Light from Gold and Silver.*

Dr. Brewster has discovered that if a pencil of polarized light undergoes more than two reflections between two polished plates, either of *gold* or *silver*, it consists of two portions, A and B, of coloured light, the colours being complimentary to each other. The portion A is polarized in the plane of reflection, and B in a plane perpendicular to it. The colours of A and B vary after every two reflections. The 2d and 3d, the 6th and 7th, the 10th and 11th, &c. reflected images are *blue*, or composed of the most refrangible rays; while the 4th and 5th, the 8th and 9th, the 12th and 13th, &c. are *red*, or composed of the least refrangible rays. The angle of incidence upon the plates should be between  $80^{\circ}$  and  $90^{\circ}$ , and the plane of reflection inclined  $45^{\circ}$  to the plane of the primitive polarization. In order to perceive the colours, the reflected pencil must be analyzed by a prism of Iceland spar. This singular property is not possessed by mercury, lead, steel, nor any other metal which has been tried; but all these metals possess another property different from that of the gold and silver. These experiments have been repeated and verified by M. Biot, of the Institute of France.

### VI. *On the Multiplication of Images, and the Colours which accompany them, in some Specimens of Iceland Spar.*

Professor Robison, Martin, Brougham, and Malus, have success-

irely examined the multiplication of images, and the beautiful colours which accompany them, in some specimens of Iceland spar. All these philosophers ascribe the phenomena to  *fissures*  within the crystal, and almost all of them explain the multiplication of the images by internal reflections. Dr. Brewster has discovered the true cause of all these appearances, and can communicate the faculty of producing them to any piece of Iceland spar, or any double refracting crystal. What was supposed to be an accidental fissure, he has shown to be an  *interrupting stratum* , either of the same or of a different substance,  *crystallized in a different manner from the rest of the mineral* , and producing the complimentary colours by depolarizing two of the images; and he has found that this stratum is always perpendicular to the shorter diagonal of one of the rhomboidal faces. The artificial rhomboids which have been constructed upon this principle imitate, in the most perfect manner, all the phenomena which appear in the real crystal.

#### VII. Optical Properties of Iodine.

In attempting to obtain a measure of the refractive power of iodine by ascertaining the angle at which it polarizes light, Dr. Brewster found that it possessed the property of polarizing the light which it reflected in two opposite planes, a property which is peculiar to metallic bodies.

#### VIII. Queries respecting the Visibility of the Stars in the Day-time.

(To Dr. Thompson.)

SIR,

Having for some time past made a considerable number of observations on the celestial bodies in the day-time, the following conclusions, amongst many others, have been established, from a great number of observations and experiments, viz. that in every instance an increase of the magnifying power of the telescope has the principal effect in rendering a star easily perceptible; that the diminution of the aperture of the object glass produces a very slight effect, in some cases none at all, and that when the aperture is contracted beyond a certain limit it produces a hurtful effect, and even prevents the object from being distinguished. These conclusions, I have reason to believe, coincide with the deductions of Mr. Short and others who have made similar observations; so that it may be considered as a fact sufficiently established, that magnifying power is requisite for distinguishing a star in the day-time, and that the more the magnifying power is increased, the more splendid and brilliant the star appears.

*Query*: What is the cause why the magnifying power of telescopes produces this effect?

In regard to the planets, a probable solution might be given from the consideration that the telescope, by enlarging the angle of vision, augments the apparent size of the object, thus presenting a larger surface to the eye, which is partly the reason why the moon is

visible in the day-time. But this reasoning will not apply to the fixed stars; since no telescope is found to augment their apparent size, so as to make them resemble planetary disks. I am aware that it may be said, that the telescope excludes almost all the light except what comes from the object, and thereby prevents the impression made by its light from being effaced; and that by increasing the magnifying power the ground upon which it is seen becomes darker, forming a greater contrast to the light of the star. That the first of these considerations, however, is not sufficient to account for the effect, I am pretty much convinced by the following experiment. I have frequently directed a long tube, blackened in the inside, and furnished with proper apertures to exclude the extraneous light, to a star, about sun-set, when it was easily seen with a very small degree of magnifying power, but could never perceive it through the tube, though I was perfectly certain it was pointed directly to the star. This experiment has uniformly failed of success in detecting a star till it became visible to the naked eye. That the darkness of the ground on which a star is seen is not, of itself, sufficient to account for the effect produced by magnifying power, is also evident from the following circumstance, viz. that by diminishing the aperture of the object glass we may produce as dark a ground as we please; but this contraction of aperture will not produce the effect of rendering a star visible if a small power be applied; nay, by diminishing the aperture beyond a certain limit, we prevent a star from being seen, which would otherwise be quite perceptible, I am therefore induced to conclude that some additional reasons must be assigned why magnifying power produces this effect. To ascertain these reasons is the object of this query.

For similar reasons, I should wish to be informed if the fact is *established beyond all doubt* that the stars are visible in the day-time from a deep well or pit. This fact is generally taken for granted, both by philosophers and by the vulgar, and has been asserted by many respectable writers, both ancient and modern; but none of those whose works I have perused assert that they themselves, or any of their learned friends, have witnessed this phenomenon. Otto Guericke, without questioning the fact, attempts to account for it in the following manner: "It is," says he, "because the light which proceeds from the stars is not overpowered by the rays of the sun, which are lost in the number of reflections which they must undergo in the pit, so that they can never reach the eye of a spectator at the bottom of it." But for the same reason they ought to be visible through a long tube, where the rays of light are prevented from striking the inside, by the interposition of proper apertures, and more especially when such a tube is accurately directed to a particular star, which is contrary to many experiments I have made to ascertain this fact. Have any of your numerous philosophical readers, or their friends, seen the stars from a deep pit in the day-time? Are colliers, miners, or subterraneous surveyors, who are frequently in favourable situations for this purpose, known to have

occasionally observed them? If so, are small stars distinguishable in such situations? or is it only when a star of the first or second magnitude happens to pass near the zenith that such a phenomenon is perceived?

Is it a fact, as has been related by some authors, that the celebrated astronomer Tycho Brahe had an observatory in a deep pit or dungeon, where he frequently sat and contemplated the stars in the day-time, as reflected from mirrors which were placed around him in different positions for this purpose?

An answer to any of the above queries, if they be not deemed too unimportant for discussion in the *Annals of Philosophy*, will much oblige, Sir, yours, &c.

T. DICK.

Methuen, near Perth, April, 1815.

### IX. On the Explanation of the Fluxionary Calculus.

(To Dr. Thomson.)

SIR,

The following ideas owe their origin to the valuable article of Professor Christison in your last number. Their merit is certainly only of the ordinary kind, yet their publication may perhaps do some good.

To facilitate the conception of the generation of fluxional quantities, I conceive that if the line A D (see the Professor's figure) be considered as a cylinder, on which is rolled a sheet of paper, divided into the two parallelograms, A F, C E, the former being coloured black, and the latter red; then, when the paper is unrolled, it will be easy for the student to comprehend the generation of the rectangles, and also their constant ratio to each other, which (Euclid i. 6,) is as A E to E D, or, in the Professor's example, as 5 to 1.

Again, if A C D E, in fig. 2, represent a piece of paper forming a parallelogram, as A D, and a triangle, as A B C, having a series of equidistant lines, as N M L, H G F, &c. &c. drawn parallel to C B D, and on which the respective proportions of C B to B D, of N M to M L, of H G to G F, &c. &c. are written, then if C D represent a cylinder on which the paper is rolled, commencing at C D, it is manifest when the paper is unrolled that A E will be the part first visible, and as it continues to be unfolded, the generation of the parallelogram and triangle, and also the ratio of their rates of increase, by the numbers on the parallel lines, at any instant or position, will be shown in the easiest and most familiar manner.

This simple contrivance will, I conceive, illustrate completely the Professor's idea, as the generation of the quantities can be easier comprehended by this mode than by supposing them to be produced by the motion of a line. I perfectly agree with your learned Correspondent, that the first principles of this science, and indeed somewhat more, may be attained by very young persons; and it is singular that a simple and elementary treatise adapted to their com-

prehension has not yet been published. An author perhaps could not be better employed than in elucidating the principles of those sciences which, from their great and important applications, are so intimately connected with the most profound and interesting departments of human knowledge.

I am not, however, a friend to the introduction of motion into the conception of fluxional quantities, as I conceive it to be foreign to the inquiry. I would rather consider the differential calculus as an extension of the science of algebra.

Your humble servant,

Plymouth, May 5, 1815.

GEORGE HARVEY.

### X. *Milky Juice of Plants.*

Dr. John has lately examined the milky juice of several European plants. I conceive that a short account of the results which he obtained will be acceptable to the readers of the *Annals of Philosophy*.

#### 1. *Leontodon Taraxicum* (*Dentylion*).

The juice of this plant is liquid, and milk-white. It has a sweetish and very bitter taste, with a certain degree of sharpness. Its smell is similar to that of the fresh plant. When exposed to the air, it speedily coagulates, and acquires a violet-brown colour. The coagulated sap being boiled in water, communicates to that liquid a brown colour and a bitter taste, and the property of reddening litmus paper. The undissolved portion is white and elastic, and similar to caoutchouc. It becomes brown by exposure to the air. Alcohol acts but feebly on it, dissolving only a little resin. The caoutchouc burns with a brilliant flame. The watery solution was precipitated by oxalic acid, nitrate of silver, nitrate of mercury, and nitrate of barytes. Hence the constituents of this plant are—

Water.	A trace of gum?
Caoutchouc.	An acid.
Bitter extractive.	Muriate, phosphate, and sul-
A sweet substance?	phate of lime, and of an
A trace of resin.	alkali.

#### 2. *Lactuca Sativa* (*Lettuce*).

This plant yields but little milky juice. According to John, its properties are the same as those of the *leontodon taraxicum*, and its constituents the same. He was not able to determine whether it contained an uncombined acid. From the known soporific powers of the juice of lettuce, there is reason to believe that it contains a substance similar to that which exists in common opium. It would be worth while to examine whether the juice of the *leontodon taraxicum* contains the same principle.

#### 3. *Ficus Carica* (*Fig Tree*).

The twigs and footstalks of this tree yield a milky juice, which

speedily coagulates in the air, and becomes a transparent mass, having the consistence and lustre of wax. The fresh sap has a bitterish and astringent taste, but produces no change upon the solutions of iron. Water dissolves a small portion of the coagulated sap. Alcohol at a boiling temperature dissolves about a third of it; but the resinous substance thus dissolved precipitates again as the liquid cools. The undissolved portion possesses the properties of caoutchouc. Hence the constituents of this sap are as follows:—

Caoutchouc.	A trace of extractive, soluble in
Resin, soluble only in boiling	water.
alcohol.	Salts.

#### 4. *Platinus Occidentalis* (Plane-Tree).

The bark and wood of these trees when young contain a milky juice. This juice, according to John, contains the following constituents:—

Water.	A very small quantity of gummy
Resin, soluble in boiling alcohol	matter.
only.	Phosphoric acid.
Caoutchouc.	Salts.

#### XI. *Account of a dreadful Accident at Heaton Main Colliery near Newcastle.\**

This colliery is situated in the bed of coal called the high main. It is of a considerable depth, about 110 fathoms, and the shaft is situated at the lower extremity of the mine. The shaft is divided by boarding all the way down, so that the same opening served for the *up* and *down* cast shaft. The seam towards the rise had been formerly worked as a colliery, under the name of Heaton Banks, by shafts distinct from the present workings, which shafts, when the colliery was given up, were covered over with boards and earth. In the course of time these old workings had become filled with water; and the managers of the present colliery being well aware of the danger attending so large an accumulation of water, the workings were proceeded in with the utmost caution.

The mine was very much subject to what the colliers call the *creep*, which is a gradual filling up of the horizontal passages. It had been customary for some time past to bore in various directions upon the lines the men were working, in order to ascertain whether any body of water lay concealed in the adjacent cavities. This precaution was about to be put in practice at nine o'clock on Wednesday the 3d of May; but before that time had arrived, (between three and four o'clock in the morning,) a dreadful rush of water came through the roof in the north-west part of the colliery, and continued to flow with such rapidity, that only 20 men and boys were enabled to make their escape. In a very short time, the

\* This account is partly drawn up from private information, and partly from the *Tyne Mercury* for May 9, 1818.

water closed up the lower mouth of the shaft: and that night it rose to the height of 24 fathoms. Some faint hopes being entertained that the men below would retire to the higher parts of the workings, which were said to be above the level of the water in the shaft, every exertion was used to open a communication with them by the old workings. Considerable difficulties, however, presented themselves. The rubbish which covered and choked up the mouths of two old shafts, when deprived of the support of the water, fell in, dragging along with it some trees which had been planted round the spot. An old shaft, in front of Hexton Hall, has not, however, presented a like impediment, and consequently every exertion is using to open a communication by that way. They had uncovered the pit, and reached the scaffolding on Saturday the 6th, which was five fathoms from the surface; and we understand their efforts are likely to be successful, if not prevented by an accumulation of inflammable air, with which the old workings appear to be filled. Ever since the accident, three large engines (one of 130 horse power) have been constantly employed in drawing the water from the pit, at the rate of about 1200 gallons per minute, yet on Friday morning it was found to have attained the height of 31 fathoms up the shaft. In the evening, however, the water had decreased about three feet, and we understand has continued to decrease since that time; so that no doubt is now entertained of the colliery being at some future period again set to work. We now come to state the extent of the calamity. Mr. Miller (the underviewer, who has left a wife and eight children), 92 workmen, 42 boys, and 37 horses, have perished; and 25 widows, with about 80 children, are left to bemoan the sudden death of their husbands and fathers.

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## ARTICLE XIII.

### New Patents.

**JOHN CARPENTER**, Truro; for a knapsack, which prevents the wet coming between it and the back; and a pouch in front suspended from the shoulder straps of said knapsack, so as to counteract its weight. Jan. 20, 1815.

**JEAN RAUDONT**, Oxford-street; for certain improvements in the construction of dioptric telescopes. Jan. 20, 1815.

**JAMES MILLER**, Liverpool; for certain improvements in the construction of stills, furnaces, chimneys, and other apparatus connected with the art of distillation. Jan. 28, 1815.

**JOHN WOOD**, Manchester, clock-maker; for certain improvements in machinery, used for preparing and spinning cotton-wool and various other articles. Feb. 4, 1815.

**JOSEPH TAYLOR** and **PETER TAYLOR**, Manchester, machine-

makers; for certain improvements in the loom to be used in weaving cotton, worsted, silk, or other cloth, made of any two or more of the said materials. Feb. 4, 1815.

JAMES THOMSON, Primrose Hill, Lancashire; for certain improvements in the process of printing cloth made of cotton or linen, or both. Feb. 4, 1815.

WILLIAM GRIFFITH, Giltspur-street, London; for an improved toast-stand. Feb. 7, 1815.

RICHARD JONES TOMLINSON, Bristol; for certain improvements in the method of framing, constructing, or putting together the roofs of buildings, or the parts thereof. Feb. 9, 1815.

WILLIAM MOULT, Bedford-square, London; for a mode of evaporation and sublimation. Feb. 13, 1815.

JOSEPH BURRELL, Thetford, Norfolk; for an invention to be used as a support and safeguard in getting in and out of chaises, and other two-wheeled carriages. Feb. 21, 1815.

JONAH DYER, Wootton-under-edge, Gloucester; for an improved frame or machine for shearing of woollen cloth. Feb. 21, 1815.

SAMUEL BROWN, Mark Lane, London; for a rudder and certain apparatus connected therewith, for governing ships and vessels of all descriptions, with much more certainty and effect, and for producing various advantages not hitherto practised or known. Feb. 28, 1815.

RALPH DODD and GEORGE STEPHENSON, Killingworth, Northumberland; for various improvements in the construction of locomotive engines. Feb. 28, 1815.

WILLIAM MITCHELL, Glasgow, and JOHN LAWTON, King-street, Snow-hill, London; for a lock and key applicable to various purposes. March 7, 1815.

THOMAS DEAKIN, Ludgate-hill, London; for a portable kitchen. March 7, 1815.

DUDLEY ADAMS, Fleet-street, London; for certain improvements in the construction of paper vellum tubes for telescopes, and other optical parts of telescopes. March 7, 1815.

WILLIAM WOOD, Shadwell, Middlesex; for the manufacture of a material or materials, and the application thereof to the more effectually making water tight and sea-worthy ships and all other vessels, which he denominates adhesive felt. March 9, 1815.

ROBERT DICKINSON, Great Queen-street, Lincoln's Inn-fields, London; for certain improvements in the making or fabrication of sundry tools, implements, or articles, used in various arts or manipulations, or the ordinary occasions of life. March 14, 1815.

JOHN MILLS, Holywell-street, St. Clement's church-yard, Strand, London; for improved elastic stays for women and children; and also to give relief to women in a state of pregnancy. March 14, 1815.

ELIZABETH BEVERIDGE, Hatton-garden, London; for an improved bedstead. March 14, 1815.

# ARTICLE XIV.

## METEOROLOGICAL TABLE.

1815.	Wind.	BAROMETER.			THERMOMETER.			Evap.	Rain.
		Max.	Min.	Med.	Max.	Min.	Med.		
4th Mo.									
April 1	S E	29.72	29.55	29.635	70	45	57.5		3
2	W	29.82	29.81	29.815	63	42	52.5		
3	Var.	29.91	29.81	29.860	57	34	45.5		3
4	W	30.18	29.91	30.045	60	35	47.5		
5	W	30.20	30.12	30.160	59	41	50.0		
6	N W	30.12	30.06	30.090	66	43	54.5		
7	E	30.06	29.91	29.985	69	45	57.0		
8	E	29.91	29.81	29.860	61	45	53.0	.68	
9	N E	29.84	29.81	29.825	64	43	53.5		—
10	N E	29.90	29.84	29.870	54	50	52.0		.50
11	Var.	29.90	29.89	29.895	63	47	55.0		—
12	S W	29.89	29.75	29.820	68	40	54.0		4
13	Var.	29.59	29.56	29.575	69	36	52.5		.23
14	N W	30.00	29.59	29.795	46	34	40.0		—
15	N	30.13	30.00	30.065	50	28	39.0	.30	
16	N E	30.13	30.11	30.120	53	30	41.5		
17	N E	30.25	30.11	30.180	54	31	42.5		
18	N	30.25	30.18	30.215	54	30	42.0		—
19	Var.	30.18	29.85	30.015	59	38	48.5		
20	S W	29.85	29.17	29.510	59	36	47.5		—
21	Var.	29.17	28.74	28.955	49	40	44.5		.72
22	N	29.06	28.74	28.900	50	40	45.0	.38	.20
23	N W	29.24	29.06	29.150	52	35	43.5		.16
24	N E	29.66	29.24	29.450	54	36	45.0		2
25	Var.	29.91	29.66	29.785	53	41	47.0		8
26	S E	30.06	30.04	30.050	58	33	45.5		—
27	N E	30.04	29.92	29.980	61	43	52.0		1
28	N E	29.92	29.64	29.780	64	45	54.5		
29	N E	29.64	29.49	29.565	54	39	46.5		—
30	S E	29.65	29.47	29.560	56	40	48.0	.45	7
		30.25	28.74	29.783	70	28	48.56	1.81	2.09

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

## REMARKS.

*Fourth Month.*—A summer-like day, with a breeze of wind: *Cirrus* formed about noon, and continued after sun-set, passing to *Cirrostratus*: some rain followed. 2. Overcast, a. m. apparently with *Cirrostratus* from *Cirrus*: during the middle of the day this gave place to *Cumulostratus*. 3. a. m. Overcast, calm, with dew on the grass: showers followed: after which *Cumulus* and *Cirrus*. 4. Much dew: *Cirrus*, followed by *Cirrostratus*: a few large drops about sun-set. 5. Much dew: *Cirrus*. 6. The same: low *Cumuli* prevailed afterwards, some of them capped with *Cirrostrati*: then a shallow bed of *Cumulostratus*, ending at sun-set in *Cirrostratus*: the evening twilight opaque, dewy, and suffused with red. 8. *Cumulus*, beneath *Cirrocumulus*: a strong breeze: a mixture of *Cirrostratus* and *Cirrus* gave the clouds an appearance of active electricity; but the whole ended in *Cirrostratus*. 9. A confused mixture of the modifications, as yesterday, with the addition of haze: some drops by 10 a. m. and a shower, p. m. 10. Heavy showers, a. m.: cloudy, p. m. 11. Wind, a. m. S. W.: the clouds, mingled with the smoke of the city, came back from the E. at sun-set. 12. a. m. The clouds gathered peacefully, the *Cumulus* capping: p. m. the *Nimbus* appeared, and after some distant thunder to S. W. we had a shower. 13. a. m. *Cirrus*, *Cirrocumulus*: much dew, the large drops of which sparkled in the sun with the prismatic colours: various modifications of cloud followed: about one p. m. it thundered, N.: then nearer, with lightning, S.: the wind shifting to that point, we had a heavy shower from W. with hail: wind and rain at night. 14. a. m. Cloudy, windy: *Cumulostratus*: a little snow: swallows skimming the meadows. 15. *Cumulus*, succeeded by *Cumulostratus*: the wind moderate. 16. The same: there were indications of hoar frost this morning. 17. Hoar frost: *Cumulostratus*: at night *Cirrostratus*, and a lunar corona. 18. a. m. A few drops during the union of some clouds, which became *Cumulostratus*. 19. a. m. Loose *Cumulus*: at night *Cirrus*, and afterwards a very large white lunar halo. 20. Overcast, dripping: a hail shower, p. m.: at night a lunar halo, very small, the prismatic ring touching a corona within. 21. Much cloud and wind: wet forenoon and night: the greatest depression of the barometer that has occurred in the present year. 22, 23. Much cloud, windy: rain at intervals. 24. Windy: cloudy: rain, a. m. with a little hail: in the evening the *Cumuli* dispersed rapidly, with *Nimbus* and *Cirrostratus* in the horizon. 25. Various clouds, ending in rain, of which, however, very little till night. 26, 27. Windy, variable: some dripping. 28. A little rain early: then *Cumulostratus*. 29. *Cumulostratus*: strong breeze: dripping: the *Cirrocumulus* appeared, transiently, as for several days past: a *Stratus* at night. 30. *Cirrocumulus*, which passed to *Cirrostratus*: p. m. (upon the wind becoming S. E.) rain.

## RESULTS.

Winds variable, but for the most part Northerly.

Barometer: Greatest height..... 30.25 inches.

Least..... 28.74

Mean of the period..... 29.783

Thermometer: Greatest height..... 70°

Least..... 28

Mean of the period..... 48.56

Evaporation, 1.89 inch. Rain, 2.09 inches.

TOTTENHAM, Fifth Month, 2, 1815.

L. HOWARD.

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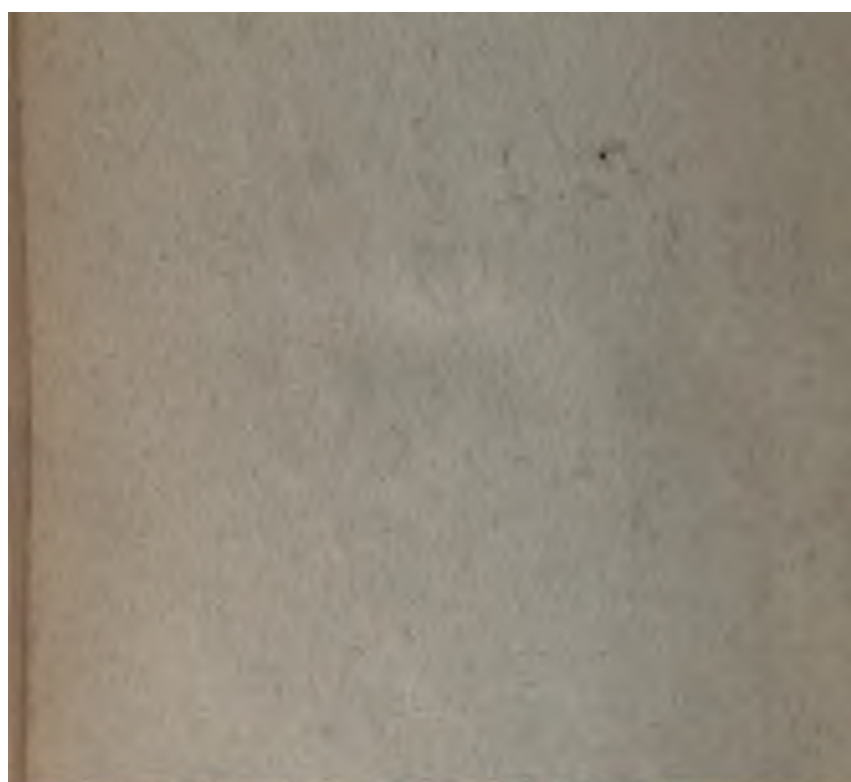
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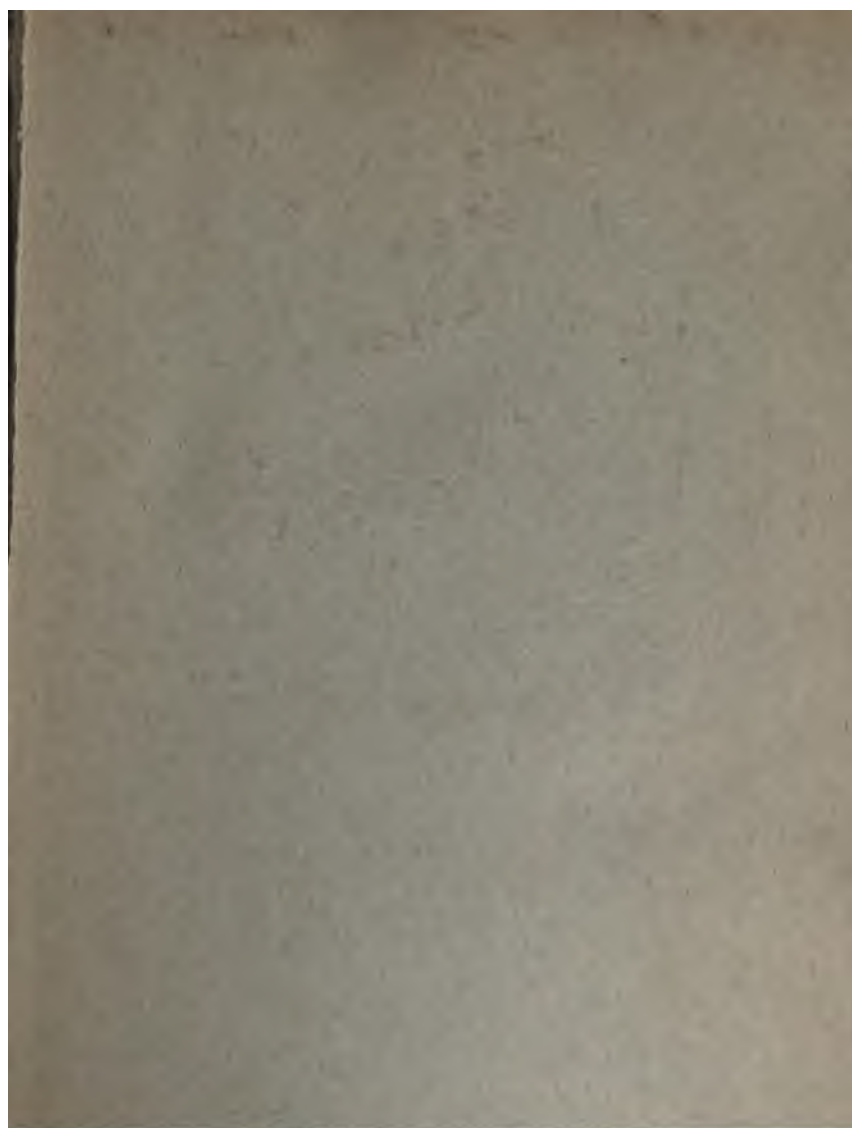


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NEW SERIES.

JANUARY TO JUNE, 1823.



VOL. V.

AND TWENTY-FIRST FROM THE COMMENCEMENT.

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## ERRATA TO VOL. V.

Page 87, line 11, for *M. Von Mons*, read *M. Van Mons*.

91, line 15, for *decompose*, read *precipitate*.

54, line 6, from bottom, for  $\overline{1000000000}$ , read  $\overline{1000000000}$ .

122, line 5, from bottom, for *Coldile*, read *Cottle*.

130, in fig. 2, for  $5\frac{1}{2}$ , read  $5\frac{3}{4}$ .

131, line 6, for as 13 to 15, read as 13 to 5.

233, line 17, and p. 235, line 14, for *M. Eneker*, read *M. Encke*.

305, line 16, for the manner in which the chlorine lies in the water is evinced by,  
read is evinced by the manner in which the chlorine lies in the water.

306, line 22, for *Asalaphus*, read *Ascalaphus*.

line 29, for *Taile*, read *Tailed*.

line 30, for *Fuverca agavephylla*, read *Furcraea agavephylla*.

line 32, for *M. Felix de Avelear Protero*, read *M. Felix de Avellar Protero*.

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JANUARY, 1823.

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ARTICLE I.

*Memoir illustrative of a general Geological Map of the principal Mountain Chains of Europe.* By the Rev. W. D. Conybeare, FRS. &c. (With a Plate.\*)

(To the Editor of the *Annals of Philosophy*.)

SIR,

So rapid has been the progress of geological science within the last ten years (since its cultivators, abandoning the idle endeavour to construct theories without data, have confined themselves to the only true path of philosophy, that of inductive observation), that while at the beginning of this period, amidst volumes of speculation, no accurate description of a single country could be found, a physical map of the entire extent of Europe may now be confidently anticipated at no distant day. The labours of Buckland, Ebel, Freisleben, Raumer, and more especially the highly important work of Beudant† on Hungary, and the memoir of Mr. Strangways on Russia, have already accumulated a vast store of valuable materials, to which a few months will probably add a geological map of France by Omalius d'Halloy, and of Germany by Von Buch;‡ together, as it is

\* The map is partly copied from one in Ebel's *Bau die Erde*.

† This is at present known in this country only by a few unconnected extracts; a full analysis ought to be given in some of our scientific journals. I have no hesitation in pronouncing it the most important geological work which continental science has yet produced.

‡ While this memoir was passing through the press, I have received the four first numbers of a very able geological work now publishing periodically in Germany by *New Series*, VOL. V.

hoped, with Charpentier's long-promised description of the Pyrenees. It is also understood that an individual in this country, eminently qualified for the task, has, for a considerable time, been employed in collecting together all the scattered sources of information, with a view to incorporate them in a general map of Europe.

In this advanced state of the science, a general *coup d'œil* of the ground which has been already gained seems desirable, and in this view I have thrown together the following brief notices, embodying their contents in the accompanying map (Pl. XIX), in which, neglecting the detail (an object precluded both by the scale adopted, and by the yet incomplete state of the requisite observations), it is attempted alone to indicate the *general* distribution of the *principal classes* of European formations, and nothing further than *approximation* is professed. I shall be fully contented if this sketch may be considered as an humble but useful subsidiary to the more extended plans of which I have spoken; as an index map, it may, I hope, accomplish this purpose, for which its small scale particularly qualifies it, allowing the information it contains to be comprehended at a single glance; while those general relations which are the great objects of true science, and which are often almost lost amidst the complications of detail, are brought more prominently forward, and more readily seized under these circumstances.

The map on which these observations are laid down is copied as to its groundwork from a similar map in Ebel's work on the Alps, although the great advances in geology have required an almost total change in the colouring of the secondary countries, and many corrections in that of the older chains, so that in its present state, it may be considered as almost an original document. It will be found to include all those parts of Europe which are most important to the present object; since the southern extremities omitted (Spain, for instance), are generally as yet too imperfectly known to have admitted even an approximate representation; while the Scandinavian chains of the north (including almost the whole country north of the Baltic, as far as the gulf of Finland), presenting only primitive and transition rocks, are too uniform to require any.

The general distinctions which alone are attempted in this map reduce themselves to the following: 1. The more ancient rocks, including the primitive and transition classes. 2. The carboniferous series, including the old red sandstone, mountain limestone, and coal measures. 3. The new sandstone and mag-

C. Kosterstein, the "Teut-schland geognostisch geologisch dargestellt." This contains excellent introductory memoirs on the geology of Germany and the neighbouring countries; and it has greatly gratified me to find the plan of these memoirs nearly coincident with that adopted in the present paper. Maps and sections of Germany, Bavaria, Switzerland, and the Tyrol, have already appeared in this work; and I regard myself as fortunate in having been enabled to avail myself of these new materials in the accompanying map.

nesian limestone. 4. The lias and oolitic series (Jura limestone of the French). 5. Green sand. 6. Chalk. 7. Tertiary formations reposing on the chalk. 8. Overlying trap and volcanic districts.

I shall proceed to give a hasty sketch of the general distribution of these formations, following the order above specified.

### 1. *Primitive and Transition Districts.*

So intimate is the connexion between the schistose rocks of these two distinct aeras (if distinct indeed they be, which some recent geologists appear strongly inclined to question), that it is impossible in any general map (unless the scale were such as to admit a separate colour for each individual rock), to assign any accurate line of division. In tracing, however, the tracts which fall under this description, I shall, as far as materials are afforded, and the nature of so general a sketch will admit, specify the leading character of the constituent formations.

Europe may be considered, when viewed under its most general aspect, as presenting a vast central basin occupied by the deposits of the secondary and tertiary class, surrounded by a border of alpine chains, exhibiting the older rocks of which we are now about to treat.

This border, however, is not continuous, but interrupted by openings, frequently very considerable, separating the constituent chains.

Neither is the included basin exclusively occupied by an uninterrupted series of the more recent deposits; but mountain groupes of the older rocks in places break through the mantle thus thrown over them, and reappear towering above the vicinity.

On the south of the great bounding chains, e. g. in Spain, Italy, &c. the same or similar secondary and tertiary deposits are repeated, forming portions of another great basin which may be designated as that of the Mediterranean.

There are other smaller and local basins comprehended by the tortuous course of the great chains.

The chains and groupes of the primitive mountains appear to present the centres of the greatest disturbances which have affected the surface of the earth, their strata being always elevated under high angles, often indeed in vertical planes, and exhibiting dislocations which can be referred to no cause but mechanical violence.

The extent of these disturbances appears often at least to be in proportion to the height of the chain; so that when the older chains form very elevated mountains (as in the Alps and Pyrenees), the secondary strata, and even those of very recent origin, are thrown into elevated planes, exhibit extraordinary contortions, and themselves form lofty collateral mountain ranges; but where, as in England, the older chains are compa-

relatively low, the secondary formations (those at least which succeed the carboniferous series), are nearly horizontal, and appear to have suffered little derangement, with a few local and limited exceptions, such as are presented by the vertical chalk, &c. of the Isle of Wight. The distinction, therefore, of inclined and horizontal, or floetz\* rocks, does not express any inherent character in the rocks themselves, as deducible from their relative antiquity (any further than that the phenomena must induce us to consider the primitive chains as the scenes of the greatest and most general convulsions), but it depends merely on the circumstance of contiguity to the principal chains.

On the importance of these observations to geological theory, and the support they afford to the hypothesis of the elevation of the mountain chains, it is needless to insist.

I proceed to trace these principal chains in order, beginning from the north.

#### (A.) *Finland and Scandinavia.*

These countries, though not included in the map, must yet be noticed, in order to convey a complete idea of the chains bounding the great central basin of Europe.

They are almost exclusively occupied by primitive rocks, among which gneiss predominates, and characterises the lowest members. Granite is of less common occurrence, and alternates with the gneiss. Mica slate succeeds, on which granite again reposes. Transition rocks, principally limestone, containing the same organic remains with that of Dudley, in Staffordshire, skirt the primitive district on the southern shore of the gulf of Finland, in the Baltic islands of Gothland, Oeland, and Bornholm, and in the Scandinavian provinces of Gothland, Dalecarlia, Nerike, Jemtland, and the vicinity of Christiania; in the latter locality, a phenomenon of the greatest geological interest occurs; for here the transition limestone is covered by old? red sandstone conglomerate, on which reposes a porphyritic formation of various character, passing at one extremity into decided granite, possessing all the characters of that formation; and on the other, into basalt and wacke equally well marked. This repetition of granite as a comparatively recent formation covering those of transition, and its association with rocks, to which a great and increasing majority of geologists (increasing, it may be added, with the extension of the science itself, and almost with its existence as a science of accurate observation), concur in assigning an igneous origin, is one of the most important facts yet contributed towards the foundation of a secure theory.

\* It has been objected to my remarks on the term floetz, as synonymous with horizontal, that it is really synonymous with stratified. I cannot, however, apprehend that it is used in the latter sense by the Wernerians; since in this case I must suppose them to apply as a distinguishing epithet to the secondary series, a character equally belonging to the majority of transition and primitive rocks; an absurdity which I do not wish to impute to them.

(B.) *Scotland—Ireland—England.*

The chains traversing the Shetland isles and the Scotch highlands and Hebrides, may be, perhaps, regarded as continuations of the Scandinavian range.

Mica slate is here the predominant rock: granite, however, forms the nucleus of several mountains. The phenomena exhibited by the junctions of the granite, and the veins sent off by it into the incumbent rocks, have been here minutely studied, and accurately described. The only writers who have yet given precise accounts of this district (Drs. Mac Culloch and Boué), concur in favouring the hypothesis which ascribes to the granite an igneous origin.\*

A very narrow zone of transition slate separates this chain from the great central valley of Scotland occupied by the carboniferous series.

The highland chains of mica slate after expiring against the Irish channel in the peninsula of Kintyre, appear to be resumed on the opposite side in the north-east angle of Antrim; and though concealed for an interval by the overlying rocks of the basaltic area, re-emerge beyond them in the west of Derry, and occupy nearly the whole of Donegal, the structure of these countries exactly corresponding with that of the Scotch highlands. Donegal Bay for a time interrupts the continuity of the chain; but beyond that Bay, the primitive mountains of Sligo, Mayo, and Connemara, or Western Galway, must be regarded as its prolongation.

A second chain of mountains parallel to the former traverses the south of Scotland skirting on that side the great carboniferous valley, and forming the natural boundary between this country and England. It is known by the name of the lead hills in its central and highest part; the Pentlands, &c. forming branches of it. It consists principally of transition slate, through which granitic nuclei emerge in several groupes; and may be characterised from the northern chain by the absence of mica slate.

This chain also may be traced in its prolongation on the opposite coast of the Irish channel; its abrupted ends appearing on each side of the usual passage across that channel from Portpatrick to Donaghadee. In Ireland it extends from Down into Longford, retaining the characters already mentioned; the Mourne Mountains, a granitic nucleus, here constitute its principal summits.

The southern chains of Ireland may be more conveniently enumerated after those of Wales.

The small group of mountains containing the lakes of West-

\* I desire to be here considered as only faithfully reporting the opinions of others, although I readily acknowledge myself strongly influenced by the concurrent testimony of almost every late and minute observer of such districts, and can hardly record their evidence without feeling myself convinced by it.

moreland and Cumberland is the next which presents itself in advancing to the south. Granite is here of confined and limited occurrence; various rocks of the greenstone family and transition slate predominate; transition limestone occurs associated with the slate.

The central chain of the Isle of Man lying midway between this group and the Irish coast is also composed principally of transition slate.

The mountains of North Wales, including the northern half of South Wales, are like those of Cumberland, composed of transition slate, and many varieties of greenstone rocks.\*

The Isle of Anglesea presents, in addition to these, chlorite slate, serpentine, quartz rock, and granite. The granite is described by Mr. Henslow, who has given a most elaborate and interesting account of this island as presenting the strongest evidence of igneous origin; and having been formed, in one instance at least, by the fusion of old red sandstone *in situ*.

A zone of transition limestone skirts the east of the Welch chains in their course through Shropshire.

On the south-east of the Welch chains lies the detached group of the Malvern hills, consisting of a protruding range of sienitic rocks flanked on the west by transition limestone, dipping rapidly in that direction, and appearing, according to Mr. Horner, as if elevated by the protrusion of the sienitic mass on which they repose.

In the centre of England another group of sienitic and greenstone rocks starts up in the midst of the secondary deposits constituting the ranges and tors of Charnwood Forest, in Leicestershire. The coal measures, &c. in approaching this group, are much disturbed and elevated. Some slate rocks here accompany the sienite.

The coasts of Ireland opposite to the Welch, to which we may next proceed, present in the Wicklow mountains a fuller development of the usual primitive series, mica slate, gneiss, &c. than the chains last described. The granite here forms lower mountains principally on the western side of the chain, and extends into the plains at their base, mica slate constituting the loftiest summits. Transition slate, greenstone, and quartz rock, occur on the eastern border. This chain prolongs itself to the south-west through Wexford, Cork, and Kerry, where it skirts the lakes of Killarney, but in the two latter counties, transition rocks almost exclusively occupy it. Through its whole extent it forms a zone running parallel to the south-eastern and southern coast of Ireland; and it is a remarkable circumstance, that many rivers rising beyond

\* I now use the term only as a general description of an extended class of rocks in Cader Idris, the Arrans, &c. and do not enter into the more minute details lately published in the *Annals* concerning the Snowdonian range.

this zone in the interior cross its chains by transverse valleys in order to reach the sea. Such is the condition of the Barrow, the Suire, and the Blackwater.

The veins branching off from the granite of the Wicklow mountains are well displayed on the coast.

The eastern portion of these chains has been excellently described by Mr. Weaver.

The Ocrinian chain of Devonshire and Cornwall, is the last which remains to be noticed in the British isles. It extends from Exmoor to the Land's End. It exhibits, 1. A series of granitic nuclei, of which the loftiest and most extensive is the mountain plain of Dartmoor. The granite ramifies into the incumbent slates, and the slate for a short distance from its junction assumes an appearance approaching to gneiss, which the Huttonians have considered as an alteration induced by the contiguity of a heated mass. 2. Slates of a doubtful character and æra (that is, whether primitive or transition) succeed, abounding in metalliferous veins, and traversed by porphyritic dykes. 3. Slates decidedly of transition associated with granular greywacke, transition limestone and greenstone occupy the exterior of the groupe. Serpentine rocks also occur (in one tract very extensively) probably as members of the decided transition series. The Ocrinian chain, like those of England generally, is destitute of gneiss and mica slate.

Lundy Island, situated opposite the northern coast of this chain in the middle of the Bristol channel, is a mass of granite.

(C.) *Norman Isles, and North Western France.*

The Norman isles, which may almost be considered as a connecting link between the similarly constituted countries of Cornwall and the Cotentin, are principally granitic. 1. Guernsey presents granite and granitelle in the northern, and gneiss in the southern division. 2. Jersey in its higher and northern tract consists of granite, in its southern and lower of slate reposing on it. 3. Alderney has on the south-west cliffs of porphyry, on the north-east low shores of a grit formed from granitic detritus. 4. Sercq exhibits trap rocks on the west, and sienite on the south.—(See account of these isles by Dr. Mac Culloch, *Geol. Trans.* vol. i.)

*Chains of Bretagne and La Vendee.*—These appear in the nature of their constituent rocks and metalliferous deposits to bear a near resemblance to the Cornish chains. In the Cotentin the granite appears in some places decidedly to rest upon the clay slate; both these rocks alternating with quartz rock, porphyry, and sienite. Much, if not all, of this clay slate is evidently that of transition; in the shafts of the argentiferous lead mine of Huel Goat terebratulæ (spiriferæ?) have been found in it, and at Angers, trilobites occur. In La Vendee, the granitic rocks appear to predominate. A line drawn from Cherbourg by Alençon,

Angers, and Fontenay, to Sables d'Olonne, will indicate the eastern boundary of those chains which extend westward to the Atlantic. The Loire, however, breaks through their line in its course to the sea, and where they open to give it passage, a small coal basin reposes against them.

(D.) *Central Group of France.*

The primitive rocks, after plunging for a short interval beneath the secondary calcareous beds, reappear in great force in the central regions of France, constituting an extensive body of mountains, sending out various ramifications between the sources of the Loire, the Garonne, and their tributary streams, being bounded on the east by the Rhone; and occupying the greater portion of the departments, Haute Vienne, Creuse, Correze, Puy de Dome, Cantal, Loire, Haute Loire, Rhone, Lozerre. Viewed generally, this group presents a vast inclined plain rising gradually from NE to SW, in which last direction is its principal and highest chain, that of the Cevennes. Granite is the predominant rock in this vast mountain plain, slates appearing to be of rare occurrence. In the departments Puy de Dome, Cantal, Haute Loire, and Ardeche (including the ancient districts of Auvergne Forez Velay, and Vivarais), ridges and conical peaks of basalt, of Trachyte, and of volcanic scoriæ, are superimposed on the elevated granitic plain, and constitute the loftiest summits of this group. Similar peaks are also scattered over the more recent deposits contiguous to the primitive chains. Many of these peaks contain regular craters, and streams of undoubted lava may be traced from them. Many of the valleys descending from this mountain group contain deposits belonging to the coal formation reposing against the primitive chain, sometimes also the tertiary deposits from upfillings in the valley. The north-eastern branch of these ridges (called the Beaujolais mountains) extends between the Loire and the Rhone as far as the district of Morvan on the north of Autun, where the oolitic chains of the Cotè d'Or overlie and abut against the granite. The chains above described approach on the SE towards the Pyrenees, leaving but a narrow interval along which the great Canal de Languedoc is conducted; but on the SW, the broad basin of the Garonne is interposed between them, exhibiting the regular series of secondary formations, from the new red sandstone to the tertiary beds.

(E.) *Pyrennees.*

The Pyrennees consist, according to Raymond, of five parallel zones; namely, a central zone of granite bounded by two schistose zones, which are in turn succeeded by the exterior calcareous chains; the granitic axis, however, does not, as is usual in mountain chains, form the loftiest crest; but, though towering above the collateral chains on the side of France, is itself over-

topped by the schistose, and still more by the calcareous band on the side of Spain; the latter forming the Tours de Marbore, Mont Perdu, and all the loftiest summits of these mountains. What renders this circumstance more remarkable is, that this limestone appears from its organic remains, (*Echini* and *Alcyonia*), to be of comparatively recent formation. The specimens I have seen approach nearly to those varieties of the younger alpine limestone now generally considered as coeval with the green sand formation of England. I have been favoured with the sight of a very interesting letter from Dr. Boué, who has recently examined this chain; he observes that it is entirely transition (transition slate and limestones, and marle), through which, in various places, granitic masses protrude, occasionally surrounded by gneiss and mica slate; these latter rocks appearing as they recede from the granite to pass into clay slate. He adds that the various phenomena which have been alleged in proof of the igneous origin of granite (granitic veins, fragments of slate imbedded in the granitic masses, the elevation, contortion, and dislocation, of the slates, together with their altered character near the point of contact, &c.), are here displayed on the most remarkable scale, and with a clearness of evidence nearly amounting to demonstration. He is even of opinion that the slates called primitive may, in this instance at least, have been only transition slates altered by the igneous influence; the transition limestone, in like manner, becomes converted into granular limestone in approaching the granite. Great dykes of greenstone and syenite also traverse the slate; when these are large, the limestones are in the same manner altered by their contact, and may be seen at Pousac elevated and supported by them. On the north of the Pyrenean chain old red sandstone and traces of mountain limestone occur, succeeded by new red sandstone with salt springs.

(F.) *Chains of the Middle Rhine, or the Vosges, the Black Forest, the Bergstrasse, or Odenwald, and the Spessart.*

Were we to pursue the course of the great chains forming the general boundary of the grand European basin, we ought, in the next place, to proceed to the Alps; but within the limits of this basin there are several protruding primitive groupes, skirting in places the course of the Rhine, and effecting a kind of subdivision between that part of the basin which lies in Northern France, and that of Central Germany. The first of these protruding groupes to be noticed are those which skirt the opposite sides of the great Valley of the Rhine in its central region below Basle; namely, the Vosges, which rise on the left bank of the river above Colmar and the Black Forest, or Schwarzwald, on the right bank.

The Vosges exhibit granite, transition slate, limestone, and porphyry. The greywacke near its contact with the porphyry

is, according to Dr. Boué, much altered, and the limestone, under the same circumstances, assumes a granular character. Traces of coal occur on the borders of this chain in some places on the E and S, and a conglomerate, probably belonging to the newer red sandstone, often overlies and abuts upon the primitive rocks in surrounding the chain.

The intermediate Valley of the Rhine between these mountains and the opposite group of the Black Forest, is principally occupied by formations of the tertiary order.

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*The Black Forest.*—This range extends near the right bank of the Rhine from Basle to Rastadt; it rises gradually from the E, and breaks down with a steep escarpment to the W. Granite is the prevailing rock, occasionally associated with gneiss. A porphyritic formation sometimes rests on the granite, and forms summits. On the eastern slope, the range is covered by the overlying red sandstone. The Odenwald rises in the prolongation of the same line between Heidelberg and Darmstadt; its constitution is exactly similar. The Spessart is a small insulated granitic group, almost close to the NE end of the Odenwald.

(G.) *Chain of the Lower Rhine, or Slate Mountains of the Ardennes, Eiffe, and Rhingau.*

This is a district of low mountains exclusively occupied by transition rocks (i. e. greywacke, greywacke slate, and transition limestone), extending from the Meuse above its junction with the Sambre, across the Rhine below Mayence to Marburg. This chain is familiar to every continental traveller on account of the magnificent defile which opens directly across it to give passage to the rapid waters of the Rhine between Bingen and Bonn; an extensive and decided volcanic district partially overlies the transition rocks above Bonn. On the left bank of the river is a region presenting plains covered with ashes, from which rise some low but well defined craters; on the right are the trachytic and basaltic summits of the Siebengeberge. This schistose chain is skirted on the N by the formations of old red sandstone, mountain limestone, and coal; on the S also, the mountain limestone occasionally appears; but the new red sandstone generally overlies the transition rocks immediately.

(H.) *The Hartz.*

This is the last of the insulated and protruding groups we shall have occasion to notice.

It exhibits granitic summits: but no gneiss or mica slate has yet been found *in situ*. The granite is succeeded by clay slate, and the usual transition series. The coal formation rests against the transition series, and is followed by the new red sandstone

conglomerate or rothe todtelegende,\* on which the cupriferous slaty limestones, coeval with our magnesian series, repose.

(I.) *The Alps.*

Having surveyed the detached groups described in the preceding paragraphs, we may now resume our account of the principal chain, where it displays itself with the greatest magnificence in the colossal summits of the Alps: the older rocks constituting this chain first emerge from beneath the more recent calcareous chains of the Apennine in the north-west extremity of Italy. The first traces of the primitive rocks appear on the borders of Tuscany and Liguria. Serpentine is here the prevailing rock. In Tuscany this forms only low hills, but in Eastern Liguria, it constitutes the nucleus of the mountain chains, which are still called the Apennine. Gneiss and micaslate also occur; granite is rather rare, but beautiful varieties of primitive limestone are quarried in several places. It is not, however, till after passing Genoa that the name or character of the true Alps begins: here that portion of them called the Maritime Alps overhangs the coast of the Gulf. The primitive rocks appear to skirt the shore for about half the distance between Genoa and Nice, when

\* All the maps and descriptions of the Hartz concur in proving this order of superposition (which has been controverted by Mr. Weaver), with regard to the *great and principal mass of the rothe todtelegende*. My high respect for my opponent in this controversy, and a general hatred of dispute, arising from reverence for the Baconian precept, "*cordi et curæ sit non disputando adversarium sed opere naturam vincere*," make me reluctant to misemploy my time in such discussions; but since a correct view of the relations of this rock forms an essential point in the identification of the British and continental series, I pledge myself in a short time to prove distinctively that Freisleben does expressly recognise the rothe todtelegende as a distinct formation from that of the carboniferous rocks; and to establish the superposition above given by copies of the maps and sections of the districts in which the rothelegende occurs, pointing out at the same time the source of the confusion which has occurred.

From the perusal of Keferstein's memoirs on Germany, while this note was passing through the press, I find the most complete confirmation of this opinion, the rotheliegende is uniformly described as *reposing* on the porphyry and coal formation. It is necessary, however, to advert to a source of confusion which might otherwise arise, from a mutual misconception of each others nomenclature, between the German and English geologists; the series of rocks being, as is agreed between both parties, 1. Coal. 2. Porphyry. 3. Red sandstone conglomerate (rothe todtelegende or rother sandstein); 4. Limestone representing geologically our magnesian lime (alpen kalkstein of Keferstein); 5. Variegated marl with salt and gyps (bunter sandstein). The English geologists have been in the habit of treating No. 3, 4, and 5, as included under one great formation, to which they have applied the name, new red sandstone, in order to distinguish it from the older quartzose conglomerate which underlies the great limestone formation supporting the coal, a rock, generally reckoned by the continental geologists (particularly by Raumer, and by Beudant in his excellent work on Hungary) among the transition series.

Hence the English writers use indifferently the same terms for the rother sandstein and the bunter sandstein—a circumstance which has led Keferstein in one instance to believe, that a difference as to fact existed between himself and Buckland, where in truth a difference of nomenclature was alone concerned.

In England, the magnesian limestone being of limited occurrence, this mode of viewing the subject is sufficiently precise; but it will be necessary in order to prevent the recurrence of these misunderstandings to harmonize for the future with the German writers, and to speak of the three formations as distinct, though yet as appertaining to one great order, that, namely, of the newer or saliferous sandstone.

they trend inland, and are succeeded by secondary hills. The Alps, which may be said to expire on the side of Italy, appear rather to be continued as to their geological constitution in the primitive chains of Corsica and Sardinia, situated directly opposite the termination of the Maritime Alps against the coast, and in the line of their prolongation.

From the Gulf of Genoa, the Alps pursue their course first to the NNW, through Piedmont and Savoy; then turning suddenly to the E through Switzerland, the Tyrol, and Stiria. The primitive and transition chains have together an average breadth of between 50 and 60 miles. These are succeeded by exterior zones of limestone, principally coeval with the magnesian limestone and oolitic series, the carboniferous series being apparently absent,\* or at most of very limited occurrence. The beds of the Alpine rocks where these are stratified are generally vertical; the bordering chains exhibit marks of the greatest disturbance, their strata being contorted and dislocated. Thus on the north all the recent calcareous chains appear to dip towards, instead of rising against, the central and primitive ridges. The three following sections taken from Ebel will give a general idea of the distribution of the older rocks in the Swiss portion of the Alps:

1. *Over the Bonhomme and Mount Cenis.*

Clayslate (Sallenche).  
 Micaslate and gneiss.  
 Primitive limestone and clayslate.  
 Gneiss, granite, micaslate, hornblende slate  
 (the Bonhomme).  
 Primitive limestone.  
 Gneiss and micaslate.  
 Primitive limestone and gypsum.  
 Unexplored interval along the upper valley of  
 the Isere.  
 Primitive limestone and gypsum.  
 Primitive limestone and micaslate (Mount  
 Cenis).  
 Gneiss.  
 Clayslate.  
 Serpentine.  
 Granite.  
 Serpentine.

2. *Over Great St. Bernhard.*

Compact felspar and slaty sienite.  
 Greywacke slate.  
 Compact felspar and slaty sienite.

\* Mr. de la Beche has, however, noticed traces of the coal formation in the Col. de

Primitive limestone.  
 Micaslate and granite  
 Primitive limestone and gypsum.  
 Gneiss.  
 Primitive limestone.  
 Gneiss and micaslate (Great St. Bernhard).  
 Primitive limestone and micaslate.  
 Primitive limestone and talcose rocks.  
 Gneiss.  
 Primitive limestone.  
 Hornblende, slate, and serpentine.

### 3. *Over St. Gothard.*

Gneiss.  
 Gneiss and granite.  
 Primitive limestone and gypsum.  
 Gneiss, granite, and micaslate (St. Gothard).  
 Hornblende, slate, and greenstone.  
 Primitive limestone and gypsum.  
 Gneiss.  
 Granite and gneiss.  
 Micaslate.

According to Ebel's map, the transition formations, greywacke, greywacke slate, and transition limestone, form a band of small comparative extent on the north of the Alps, the rest of the chain being primitive; but, according to D'Aubuisson, Mr. Brochant considers much of the gneiss, micaslate, and serpentine, of the Alpine chains, as truly belonging to the transition series: the details are given in the subjoined note.\*

\* M. Brochant, Professeur de Minéralogie et de Géologie à l'Ecole des Mines, alors établie à Moutiers, dans la Tarentaise, en observant divers points de cette contrée, fut frappé de la multitude de brèches et de poudingues qui s'y trouvaient; il vit les roches de ces montagnes alterner avec ces poudingues, et avec un terrain anthraciteux contenant des empreintes végétales. Il exposa ces faits dans un mémoire qu'il publia en 1808, et dans lequel on nous montra, pour la première fois, des schistes-micacés, des serpentes, des quartz en roche et des calcaires grenus, hors de la classe des terrains primitifs, et postérieurs à l'existence des êtres organisés. Ce mémoire classique et fondamental, pour employer les expressions de M. de Buch, fait époque dans cette partie de la science."

"M. Brochant a poursuivi, dans les Alpes qui avoisinent la Tarentaise, les conquêtes qu'il venait de faire aux formations intermédiaires, et il ne s'est arrêté que devant le Mont-Blanc et les Grandes-Alpes, retenu par un reste de considération pour leur ancienne prérogative de primordialité, et par cette élévation qui les place au premier rang parmi les montagnes de l'Europe: mais sans désespérer qu'un jour de nouvelles découvertes ou de nouvelles analogies ne les fissent passer dans les terrains intermédiaires; et en remarquant formellement que lors même que ces hautes Alpes appartiendraient aux terrains primitifs, elles n'étaient séparées, par aucune interruption, du terrain intermédiaire de la Savoie, et qu'il y avait continuité entre la formation de ces deux terrains: conclusion très-importante, et sur laquelle nous avons déjà insisté.

"Je remarquerai ici que lorsqu'en 1807, M. Brochant fit le mémoire dont les conséquences ont été adoptées par tous les minéralogistes, ce savant manquait encore d'une partie des preuves qui les rendent aujourd'hui incontestables; alors on n'avait pas encore trouvé des toquilles dans les calcaires de la Tarentaise.

"Depuis les observations de M. de Brochant, dit M. de Buch, je commence à croire

In this map also a double zone of primitive limestone is represented pursuing its course with undeviating regularity on either side the axis of the chain. In the Tyrol, the secondary limestones encroach more on the primitive chains, which, though still of considerable height, are tame in their features, and are more than rivalled in elevation, and far surpassed in grandeur, by the oolitic ranges.

The steepest escarpment of the Alps is uniformly towards the Italian side.

(K.) *Chains surrounding the Basin of Bohemia, including the Bohemer Wald, the Thuringer Wald, the Erzgebirge, the Reiseugeberge, and the Sudetengeberge.*

The central line of the Alps may, perhaps, be considered as prolonged by the primitive chain, which passes from Presburg to join the Carpathians; but on the NW of this line is a vast group of ancient chains, extending between Vienna and Dresden, completely enclosing the sources of the Elbe, or the great basin of Bohemia, and thus forming, as it were, a detached ring in front of the general system. The Bohemer Wald, or branches connected with it, closes this basin on the SW and SE, ranging round the sources of the Elbe. On the NW border, clayslate alternating with greenstone rocks prevails; but in the rest of the chain, granitic rocks predominate.

The Fichtelgeberge connects this side of the Bohemer Wald with the Thuringer Wald: it exhibits granitic summits skirted by clayslate and greenstone. The slate district is very extensive on the N.

The Thuringerwald (a branch extending from the NW of this primitive circle) exhibits granite, gneiss, and micaslate, skirted on the S by the overlying deposits of rothe todte liegende and cupriferous slaty limestone (our new red conglomerate, and magnesian limestone series). On the N, porphyritic rocks overlying those of the coal formation occur.

que le gneis même, entre Martigny et Saint-Maurice, que tous les singuliers poudingues de la vallée de Trient jusqu'à Valorsine, que les rochers de gneis entre Martigny et Saint-Branchiez, appartiennent au terrain de *granwacke*, et ne sont pas primitifs. Ces roches se retrouvent dans tout le Valais, quoique sans poudingues."

The slate of Glaris, containing fish and turtles, which has sometimes been considered as transition, appears rather to belong to the secondary rocks.

I am desirous to avail myself of this opportunity to correct an error I have inadvertently committed in quoting a statement of Dr. Mac Culloch's on a subject nearly connected with this. In noticing his discovery of a calcareous formation containing organic remains underlying the gneiss, I have hastily used the term gryphite limestone, but the fossils observed were really orthoceratites: the whole sentence is also expressed too generally, and without sufficient precision. Instead of the brief clause "a gryphite limestone underlying gneiss in one of the Hebrides," I ought to have written "a limestone formation underlying gneiss in Garvagh, the most northerly of the Hebrides, and in the shores of Loch Eribol on the adjoining main land, which at Eribol contains a subordinate bed of calcareous sandstone, exhibiting traces of orthoceratites." The Doctor has little doubt that the associated beds of the limestone will also be found to contain similar remains both at Eribol and in Garragh Island.

The Erzegeberge (forming the NW border of the Bohemian basin) presents the same primitive rocks skirted by the transition series. Its ridge supports in many places insulated basaltic summits, which are also abundant on the Bohemian side, where in the Mittelwald between the Erzegeberge and the Eger, they repose on tertiary deposits containing lignite. Porphyritic rocks overlying coal abound on the N of this chain as in the Thuringerwald.

The Riesengeberge (a continuation of the Erzegeberge on the opposite side of the Elbe, forming the north-eastern border of the Bohemian basin) has a central granitic axis, skirted by gneiss sometimes including micaslate, by clayslate, and lastly, by transition rocks; The gneiss zone is most extensive on the north, and that of clayslate on the south, of the central range. Beyond the valley of the Neisse, the continuation of this chain (here principally composed of clayslate), assumes the name of the Sudetengeberge, and ranging round the district of Glatz, unites itself with the slaty ridges proceeding from the Bohemerwald, thus completing the enclosure of the Bohemian basin (which, as we shall hereafter see, is occupied by the carboniferous series, new red sandstone, tertiary, and basaltic formations). The Valley of the Oder on the north, and of Moravia on the south, separate these chains from those of the prolongation of Alps towards the Carpathians ranging by Presburg.

(L.) *Carpathian Mountains.*

These mountains range in a semicircle round Hungary from Presburg, on the W, to the neighbourhood of Belgrade on the E; the ancient rocks, however, are not exhibited on the surface throughout the whole of this semicircle, there being an interval near the middle of its course (above the sources of the river Theiss) in which the older rocks only appear in patches bursting through an overlying sandstone deposit. With this exception, however, which does not amount to more than a sixth part of the whole semicircular range, the older formations are uninterrupted. They present first a central granite, then granite, gneiss, and micaslate, alternating together. 3. Micacate and clayslate containing granular limestone. 4. Serpentine diallage rock (Euphotide) and greenstone porphyry. 5. Transition rocks.

Having enclosed Transylvania by a rapid bend, the primitive chain crosses the Danube below Belgrade.

It then appears to extend itself to the S, turning round to the E, and forming the chain of the Balkan or Mount Hæmus.

(M.) *Mount Balkan or Hæmus.*

I am not acquainted with any geological description of this chain. Macmichael, however, observes, in crossing it from

Gablova to Shipka, that the strata on the N are generally calcareous, and the summit a blue or variegated marble; but on the descent, the rocks change to a hard argillaceous schist, abounding in large veins of quartz.

On the S of the Crimea is a tract of slate which may be a prolongation of this chain.

(N.) *The Range of Caucasus.*

This, though beyond the limits of Europe, is necessarily included in our present survey, being placed exactly on the prolongation of the line of Mount Balkan, and of the slaty tract of the Crimea, of which it, therefore, appears to be the continuation, forming with it the southern border of the great European basin. It is said to exhibit the usual primitive and transition rocks skirted by compact limestone, and to exhibit near its centre some overlying summits of floetz trap.

(O.) *Granite Plains of the Dnieper.*

These appear to constitute a group of primitive rocks protruding through the secondary and tertiary deposits of the great basin: granite principally prevails, and the country is characterised by the unusual circumstance, that although of primitive structure, it is yet low, and, except where furrowed by the valleys of the rivers traversing it, level.

(P.) *The Ural Chain.*

The great European basin is open through a wide interval, destitute of any primitive barrier towards the Caspian and Aral. Whether any primitive zone exists behind these inland seas, or how far the secondary deposits extend in this direction into Asia, is unknown; but on the NE, the Ural Mountains on the confines of Europe and Asia again present a primitive border, exhibiting, according to Pallas, the usual central and collateral zones of ancient mountain chains. It seems probable, but has not, I believe, been ascertained, that the primitive rocks of Finland on the north of the great basin extend along the shores of the White Sea till they join the northern extremity of the Ural chain, thus completing the primitive margin of the basin on that side.

(To be continued.)

## ARTICLE II.

*On Fossil Human Bones, and other Animal Remains recently found in Germany.* By Thomas Weaver, Esq. MRJA. MRDS. MWS. MGS.

(To the Editor of the *Annals of Philosophy*.)

SIR,

Tortworth, Wotton-under-edge, Dec. 7, 1822.

THE admirable paper of Prof. Buckland on fossil teeth and bones, discovered in caves and diluvian gravel, in England and on the Continent,\* has excited, as might have been expected, a high degree of interest in the public mind. Persuaded of the value attached to every addition to our stock of knowledge, that may tend in any measure to elucidate the effects of diluvian or post-diluvian action, it appeared to me that an acceptable service might be performed, by conveying to the English reader an account of the fossil human remains lately discovered in Germany, in company with those of other animals, near the valley of the Elster. The facts detailed, and the speculations thence arising, which proceed from the pen of the Baron von Schlotheim, are contained in the following paper, to which I have added a few notes. You will favour me by inserting it in the *Annals of Philosophy*; and I am, Sir,

Your most obedient servant,

T. WEAVER.

*Geological Description of the Environs of Köstritz, with an Account of the Fossil Bones discovered in that Vicinity.†*

The recent discovery of human bones, as well as those of other animals, in a fossil state, in the neighbourhood of Köstritz, cannot fail to render a description of that district interesting to naturalists in general.

I had an opportunity of examining that part of the country this spring (1820), in company with Mr. Braun, Counsellor to the Land-Chamber, a gentleman distinguished by his exact mineralogical knowledge. Its geological relations are so well exposed in the ranges of hills, and in the quarries opened on their declivities, that no room is left to doubt the disposition and order of succession of the different floetz formations, which appear in that vicinity.

The valley of the Elster extends from Köstritz to the N, in an average breadth of about  $2\frac{1}{2}$  English miles, flanked by heights which are covered with fertile fields, and slightly wooded. These eminences form connected ranges on both banks of the

\* Published in the Philosophical Transactions for 1822, Part I; and also in the *Annals of Philosophy* for August and September, 1822.

† From the Introduction to the Petrefactenkunde of the Baron von Schlotheim. Gotha. 1820.

Elster, passing Politz and Kaschwitz; that on the E, near Politz, obtaining the greatest elevation. The bottom of the valley itself is perfectly smooth, refreshing the eye with its verdant meadows, diversified with groups of trees, from among which peep villages and mills. From the heights above, an extensive, varied, and very agreeable prospect may be enjoyed, particularly toward the N in the direction of Gera.

The foundation upon which the floetz formations repose, consists of transition reddish-grey clayslate, and firm fine-grained greywacke. The former may be seen in the valley of Eleonore, ranging  $30^{\circ}$  E of S, and W of N, and dipping  $70^{\circ}$  —  $75^{\circ}$  to the NE. It rests on the greywacke, which first appears behind Köstritz, rising further S into a considerable cliff, and exhibiting in this quarter traces of old mining works. Lamellar sulphate of barytes, with disseminated malachite, occurs in one of the old hillocks. The clayslate is immediately covered by the older floetz limestone,\* which rests upon it in an unconformable, and nearly horizontal position. The lowest strata are sandy, and occasionally somewhat bituminous; but in some places, where they nearly adjoin the clayslate, they are also micaceous, and of a smoke-grey colour (belonging to the so-called zechstein, or argillaceous limestone), and are traversed by small veins and fissures, which contain galena. In the upper strata, the sand, mica, and bitumen, disappear entirely, and, in their stead, traces of yellow ochraceous ironstone become visible.

All these appearances are very common in this formation of limestone. We nowhere observed, in the places we examined, the bituminous or cupriferous marl shale, nor the todtliegende. Both appear to be wanting in the vicinity of Köstritz, and the latter first occurs between Gera and Pforten, where it is exhibited in all its varieties.†

The limestone, just described, ranges principally on the left bank of the Elster toward Gera, as far as the vicinity of Hartmannsdorf, and again on the right bank near Politz, where it is well displayed in several quarries. On the other hand, the gypsum, which is imbedded in, and subordinate to, this limestone,

\* The older floetz limestone of the Baron von Schlotheim, noticed above is the same, as the lower floetz limestone formation of Germany described by Freiesleben, the first floetz limestone of Werner, the equivalent of what has been called the magnesian limestone formation of England; in illustration of which, see my papers in the *Annals of Philosophy*, for October, November, and December, 1821; and August, 1822.—T.W.

† The meaning attached by the Baron von Schlotheim to the term todtliegende, is consistent with that of German writers in general. In the restrictive sense, it signifies the old red sandstone; in the enlarged, it comprehends the coal formation also; for proof of which it may be sufficient to refer to the *Petrefactenkunde*, Introduction, p. 5 and 6, where the author observes, "Certain petrifications are found in the coal sandstone and slate clay that are subordinate to the todtliegende, or older floetz sandstone," and again in p. 385, of the work, "In a general point of view, petrified wood may be said to be of rare occurrence in the coal measures of the older sandstone formation, as well as in coal formations of more recent origin." I should hardly have thought it necessary to advert to this construction of the term todtliegende, had it not been lately controverted.—(See also my *Geological Remarks* in the *Annals of Philosophy*, for August, 1822.) I may here observe that todtliegende, rothliegende, and rothe todtliegende, are synonymous terms with German authors.—T. W.

occurs at the foot of the opposite range, on the W, near Köstritz and Kaschwitz, in the same manner as it appears near Tieschütz, not far from Hartmannsdorf, in the whole of which extent the numerous gypsum quarries afford an insight into its character. The depressions and sinkings of the earth which occur in the vicinity, proclaim that we have entered upon the domain of the cavernous gypsum and limestone, the latter of which is, as usual, covered by the variegated or new red sandstone formation, which appears near Hartmannsdorf, and on the ridge of the chain of heights near Politz. Over the whole of these floetz formations is spread an alluvial loamy tract, which is sometimes sandy, extends for many miles, and yields to the landholder a rich return when duly cultivated.\*

After this concise general view of the country, and of the order and succession of the floetz formations, appearing near Köstritz, I now proceed to a nearer description of the limestone and gypsum, with a detail of the circumstances under which the bones of land animals are not unfrequently found in those rocks.

The varieties of this limestone formation have obtained, as is well known, different appellations from miners, among which *zechstein* is one of the principal. Near Politz, that kind of *zechstein* appears more particularly to prevail, which passes into the cavernous limestone, being traversed, as may be seen in the upper quarries, immediately under the new red sandstone, by very considerable fissures and cavities, which often exceed 12 feet in breadth, the walls of which are coated with stalactite; while the smaller fissures are frequently wholly filled with that substance. The rock ranges 30° to 45° to the E of S, and W of N, with an inclination toward the NE, in strata which are in some places a few feet thick, and in others very thin, alternating with slight layers of clay marl, that are partially sandy; being also traversed by numerous fissures in various directions. At the foot of the heights, this limestone approaches more nearly to the character of *zechstein*; and in the next quarry, near Politz, it contains *aphrite*, distributed in nests, yet only in small portions, and not in such considerable masses as occur on the other side of the Elster toward Gera. In the vicinity of the latter town, the rock appears as *gryphite limestone*, yielding, as is well known, fine specimens of *gryphites aculeatus* and *g. cymbium*, beside indistinct remains of other shells, which seem to belong to *mytulites ceratophagus* and *terebratulites lacunosus*. On the

\* I have throughout this paper employed the expression *alluvial tracts or formations* (*auflageachwemmte gebirge*) in the sense established by common usage; namely, as a general term, comprehending both *diluvian* and *post-diluvian* deposits, yet involving no absolute decision with respect to either era. Wherever such epochs are distinctly marked, the latter terms become strictly appropriate; but in questions of a doubtful nature (such as that which constitutes the subject of the present article) the former retains its use. To confine the signification of the word *alluvial* to that of *post-diluvian* merely, as lately proposed, would be to deprive ourselves of a useful general expression. —T. W.

other hand, in the Politz limestone, I have not observed any petrifications of shells. At the latter place, the upper quarry, situated near the middle of the declivity, is particularly instructive, exhibiting wide fissures and cavities, entirely filled with the alluvial loamy deposit, that covers the whole country to a great extent. Considerable masses of stalactite appear in several places, and here principally were found those bones of large land animals noticed in the following description, and now contained in my collection. They were met with at the depth of about 20 feet, imbedded in the loam of one of the widest cavities. They consist of

1. Lower bones of the foot, hollow bones, dorsal vertebræ, and single fragments of bones, of the *rhinoceros antiquus* of Blumenbach.

2. Lower jaws and single teeth of a species of *horse* of the former world, particularly distinguished by the extraordinary length of the teeth.

3. Dorsal vertebræ and hollow bones of ruminating animals, belonging to species of the *ox* and *deer* tribes of the ancient world, of an unusually large size. Of the latter occur also very large pieces of the horns, with the coronets adhering, and brow antlers and branches of great length: these closely resemble the drawings of Cuvier, tom. iv. pl. 1, fig. 3; but it is doubtful whether they are derived from the *Cervus elaphus primordialis*, or from the *Alce gigantea*, the upper or palm part of the horns being wanting.

4. Lower jaws, with the teeth mostly in a perfect state of preservation, of a large species of *hyæna* of the former world, *canis crocutaformis major*. Cuvier, tom. iv. p. 28, figs. 10, 12, and 14.

5. Fragments of the upper and lower jaws, and single tusks of the *leo diluvianus*, which approaches most nearly to the *jaguar*. Cuvier, tom. iv. pl. 1, figs. 3, 7. Compare with Mem. sur les grands Chats, pl. 1, figs. 3, 5. It is doubtful whether the fragments of one of these jaws do not belong rather to a kind of *tiger* of the former world.

All these bones are more or less changed and penetrated with calcareous matter, the alteration being particularly observable in the bones of the rhinoceros, and in those of the ruminating animals. The condition of the greater part of the bones of the other animals is nearly the same as that of the bones found in the caverns at Gaylenreuth, Scharzfeld, and elsewhere; and hence it seems probable that they are of an equal age, and referable to the same epoch of the ancient world. As, however, the fossil bones of the rhinoceros, and of the ox and deer tribes, are principally met with in loam, calcareous tufa, and other alluvial beds; and the latter in particular have hardly occurred in the chambers of the cavernous limestone (*köhlenkalkstein*) in company with those of bears, hyænas, lions, &c. it is certainly possi-

ble that the remains of the land animals thus found commingled in the fissures of the limestone, compacted in the same alluvial soil that covers its surface, may be derived from different periods.\* But as, on the one hand, the remains of several of the animals mentioned above have been found accompanying each other in tuffaceous and loamy beds, e. g. near Kannstadt, Osterode, Thiede, and other places, it is also possible that they may be of a contemporaneous origin.†

Turning now our attention to the NW side of the Elster, to the heights above Kaschwitz, we find the gypsum there imbedded in the limestone, and appearing in the declivities. The former seems to have been laid bare by the destruction of the superincumbent limestone; but the latter becomes again visible in the slopes and eminences, proceeding toward Rubitz and Gera, occurring there in the form of zechstein and gryphite limestone.

The gypsum seems to constitute in this quarter a large isolated mass, included in the limestone. As far as exposed in the quarries, which have no where penetrated deeper than 30 or 40 feet from the surface, it is compact, and of so firm a consistency as to require to be blasted with gunpowder. Colour, greyish-white, rarely inclining to yellowish and flesh-red. It is sometimes striped in the ribbon and undulated manner, and alternates with slight layers of clay, in the vicinity of which it passes into the foliated variety, and acquires partially a greyish-black colour. Farinaceous gypsum occurs adjacent to the fissures, and also in their interior, in the form of nests. Specular gypsum has been met with only in crystals in the smaller crevices. Other varieties would probably be found, in the same manner as in Mansfeld, were the quarries conducted to a greater depth. No traces of salt springs have been discovered in the vicinity of the gypsum, though from the geological position of the latter such might have been expected.

The entire gypseous mass is intersected and perforated by fissures and cavities, which follow every direction, and are connected with each other by serpentine channels, of larger or smaller dimensions. But these fissures no where attain so great a breadth as those of the superincumbent limestone near Politz. They are, however, filled throughout in a similar manner with the same alluvial loamy deposit, even to the greatest depth; and this loamy sediment appears horizontally disposed for short dist-

\* This argument seems to be invalidated by the consideration, that such remains have occurred together in other caverns, e. g. in England. (See Prof. Buckland's excellent paper referred to above, and also the Quarterly Review, Oct. 1822.)—T. W.

† According to the view of several naturalists, the bones of the animals met with in the caverns of Gaylenreuth, Liebenstein, Scharzfeld, &c. and the remains of bones found in the alluvial formations, belong to very different periods of the ancient world. This opinion, however, requires the stricter examination, since it is stated that the bones of elephants have occurred in some of those caverns; and in particular a large part of the skull of an elephant was thus met with in one of the caverns of the Hartz, which is now preserved in the collection of Blumenbach.

as it were, yielding in clusters, as it were, and under precisely the same circumstances, a number of bones of land animals, among which are distinctly to be observed also *human bones*.

Even from the first opening of these quarries, which took place about 30 years ago, the bones of man and other animals have thus been met with. According to the unanimous relation of the workmen, the former have usually been found at a depth of 10 to 30 feet from the surface, and this has happened in almost every quarry that has hitherto been opened in the gypsum, and always under the same relations. The cases are rare in which human and other animal bones have appeared singly near the surface of the gypsum, adjacent to the vegetable soil: these have undergone a much greater change, are more penetrated with calcareous matter, and are heavier than the bones met with in depth. Our own experience confirmed the affirmation of the workmen, that various bones are always found together, assembled in a heap, as it were, in the loamy deposit. On visiting Kornman's gypsum quarry, we discovered there in a nearly vertical fissure, and at the depth of 16 to 18 feet from the surface, a number of bones belonging to quadrupeds and birds, firmly imbedded in the loam. Though in a disjointed state, they appear referable to skeletons, that were formerly more or less complete. The idea has been advanced, that the bones of the smaller animals might have been brought there by owls, foxes, and other animals of prey; being, however, not found in caverns, but invariably enveloped in the loam, under the same circumstances, this supposition seems invalid; and it is besides contradicted by the appearance of the bones.

It is also evident that the human bones could neither have been buried here, nor have fallen into the fissures of the gypsum during battles in ancient times, nor have been thus mutilated and lodged by any other accidental cause in more modern days; inasmuch as they are always found with the other animal remains under the same relations, not constituting connected skeletons, but collected in various small groups in the deposit of loam, that occupies the fissures and cavities of the gypsum. They appear, therefore, to be strictly *fossil*, and to have been swept hither by floods, with the other animal bones, at the period of the formation of the alluvial tract itself.

If, as may be expected, this phenomenon should be further confirmed by the more extended examination of the Köstritz district, now in progress, it will render probable the supposition, that the human bones found in calcareous tufa also, are likewise referable to the same period; and consequently that *man existed here previous to the formation of the alluvial tracts, the last great revolution to which the earth has been subjected*. It has been already remarked by Cuvier (*Recherches*, &c. tom. i. p. 66), that this epoch of a great deluge, by which many animals were destroyed, whose remains are now found in alluvial tracts alone, in any strata of an earlier era, nearly coincides with our

chronology. And the tradition of such a deluge, preserved among all nations, now appears further confirmed by the instructive documents lying before us.

The following are the remains of bones hitherto found in the gypsum quarries near Köstritz, almost all of which are in my possession.

1. *Of Man*.—A perfectly preserved forehead, extending to one-half of the orbits of the eyes. The upper maxillæ, with the teeth mostly well preserved. The left side of the pelvis of a man, the left humerus, the right and left ossa femoris. The right thigh bone is in a more altered state than any of the other bones; and hence it is much heavier, having been found, according to the statement of the workmen, near the outcrop of a fissure. Beside these, some other fragments of human bones are contained in collections at Gera, and in that of the Natural History Society at Altenburg. All these bones are of a rather large, yet not unusual size, and certainly not of a gigantic stature, as stated by loose report.

2. Bones of *ruminating animals*, of the same description as those found in the fissures and cavities of the limestone near Politz, among which the deer's horns appear in a similar manner deprived of their animal gluten, or calcined, as it were.

3. Bones derived from animals closely resembling the *sheep* and the *roe*, although not wholly coinciding with the bones of the recent species.

4. The lower right jaw, with several dorsal vertebræ and hollow bones, of an animal very nearly allied to the *squirrel* (*sciurus vulgaris*). On comparing this jaw with a skeleton of the common squirrel, it is found to differ in some respects: the molar teeth possess a different direction, being considerably more elevated in the anterior part, while the foremost of them are much smaller, differently formed, and turned more outwards; the incisors also are of a much larger size. The proportions of the hollow bones and dorsal vertebræ are likewise considerably greater; and hence it is highly probable that we here possess a different species of the ancient world.

5. The greater part of the skull, with fragments of the scapulæ and cervical vertebræ, of a kind of *mouse*, which seems referable to the *mus terrestris*. These bones perfectly correspond with the delineations given by Cuvier of those which occur so abundantly in the osseous breccias on the coast of Corsica, tom. iv. Breches Oss. pl. 2, fig. 7. The skull of the specimen before us is deeply penetrated with gypsum, and, in some places, studded with crystals of that substance.

6. A number of bones of small quadrupeds, among which are some very remarkable jaw bones and teeth, which, though presenting some resemblance to those of the *bat* (*sorex vespertilio*) and *mole* (*talpa*), are yet for the greater part essentially different. Some of these bones agree perfectly with those met with in the

beds of calcareous tufa near Meissen; and there, as well as here, are also found single bones of large kinds of *frogs*. Other bones seem to belong to the *hare* and the *rabbit*, and deserve to be accurately examined and compared.

7. Bones of the *gallinaceous* tribe of birds, and probably also of *marsh fowl*. The occurrence of the former is very remarkable, and I am not aware that they have been met with before. Among these is a perfectly preserved tibia, with the spur adhering, which is very long in proportion, and agrees very closely with the corresponding bone of the *common* or *domestic cock* (*gallus communis* or *gallinaceus*). It is well known that the common cock is principally distinguished by such a spur, while the peacock, the partridge, as well as the *tetrao canadensis* and *francolinus orientalis*, possess, for the greater part, much shorter spurs, and frequently only obtuse, knobby excrescences in their stead. There can be no doubt, therefore, that this bone is referable to a bird closely resembling the domestic cock. The length and form of the spur also prove, that it belonged to a full grown bird, and yet the bony tube is one-third smaller, and much thinner, than that of the common cock; the head of the joint of the knee and its posterior continuation also exhibit a slight variation in form; and hence it may be presumed that the bird differed in some respects from the recent species. Almost all the bones of the birds have undergone great change, and bespeak a high antiquity, although they do not appear in so perfectly calcined a state as the bones of the rhinoceros, and the horns of the deer.

Similar fossil bones are occasionally turned up by the plough in some parts of the fields in the vicinity of Köstritz, lying probably concealed almost every where in the alluvial loamy soil. Great caution, however, should be exercised not to confound common bones, brought casually upon the land in carrying manure, with real fossil bones exposed at the surface by the operations of agriculture.

In the preceding pages, I have endeavoured to describe as faithfully as possible all the natural circumstances by which these remarkable remains of bones are distinguished; and though I have expressed my opinion, that they were swept hither and deposited during the period of the formation of the alluvial tracts, still it may deserve further inquiry, whether this view be the most probable, or whether bones belonging to different periods have been here commingled by other natural causes.

*Continuation of the Description of the Fossil Bones and their Repositories in the Vicinity of Köstritz.\**

The very remarkable circumstance of fossil bones of animals

\* From the *Nachträge zur Petrefactenkunde* of the Baron von Schlotheim. Gotha. 1829.

of the ancient world being found intermingled without order with those of recent species, and with human bones, in the loam which occupies the cavities of the lower floetz gypsum in the vicinity of Köstritz, demanded the most careful investigation. The offer, therefore, of the Privy Counsellor Rudolphi in Berlin, a naturalist of experience and high repute, to compare and determine with exactness the nature of these bones, was highly acceptable; and I feel deeply indebted to that gentleman for his valuable services on the occasion.\* The true character of several of the bones was so distinct as to require no further comparison; but it became necessary to transmit the far greater number to Berlin. Of these the largest portion has been most accurately determined, a few only remaining of a doubtful nature. In the mean time, I have received several packages from Köstritz, among which are some specimens that may require a careful examination; and of these I shall not fail to give a fuller account on a future opportunity.

Of *man*, the following fragments of bones have hitherto been dug up:

1. The os frontis, extending to the orbits of the eyes.
2. The upper maxillæ, in two corresponding halves, in which, with the exception of the incisors, all the teeth are perfectly preserved, differing in no wise from the recent.
3. A fragment of the lower jaw, with the molar teeth.
4. The left humerus.
5. The right femur.
6. The upper half of the left femur.
7. The left side of the pelvis of a man.
8. A fragment of one of the true ribs.
9. Single fragments of thigh and arm bones.

All these pieces are contained in my collection, and betray a great antiquity, although they have not all undergone a change in an equal manner. Some of them have lost their animal gluten, and are even penetrated with gypsum (as is the case also with a considerable portion of the other animal bones), while others are only slightly calcined and decomposed. This varying condition of the bones is likewise observable in all the fossil bones of Köstritz.

Of the animal remains generally admitted to have belonged to inhabitants of the ancient world, the most numerous met with near Köstritz are those of the rhinoceros, although, upon the whole, they are of rare occurrence. Of the elephants primigenus, the mastodon, and other gigantic land animals, no remains have hitherto been found in that vicinity.

\* The Court Counsellor Oken also had the goodness to favour me with several observations on these bones, which have been confirmed in part by those of Rudolphi. Whether the supposition of the former gentleman, that the human bones found near Köstritz are the remains of the Gothic race, the ancient inhabitants of Germany, be well founded, must be left for the decision of future experience.

The bones of the *rhinoceros* in my collection, derived from the limestone and gypsum quarries near Köstritz, are the following :

1. The second or third molar tooth of the right upper jaw, in a perfect state of preservation.

2. A molar tooth, in an injured state, and wholly calcined, probably belonging to a young *rhinoceros*.

3. Several cervical vertebræ of the same, nearly complete, and but slightly altered.

4. A large dorsal vertebra, damaged, and much calcined.

5. Two fragments of the scapula of the *rhinoceros*, also much calcined.

6. A large and nearly perfect fragment of the right femur of the same, perfectly calcined. It was found on Dec. 12, 1820, in Friedemann's gypsum quarry, lately opened near Köstritz, and at the same time fragments of human bones were also met with.

7. The lower end of the left femur, in the same state, and found at the same time.

8. Two perfect phalanges of the *rhinoceros*, calcined in a similar manner, discovered in Winter's gypsum quarry at the depth of 18 feet from the surface. These pieces are particularly deserving of notice, as beneath them, at the depth of eight feet further, were found the human bones mentioned under No. 9 on that list.

9. Single fragments of thigh and hollow bones of the same, from Friedemann's and Winter's gypsum quarries.

10. Very large fragments of the tibia of the *rhinoceros*, with the apophysis perfectly preserved, completely deprived of animal gluten, from the limestone quarry near Politz, not far from Köstritz.

11. A fragment, probably belonging to the fibula of the *rhinoceros*.

It is very remarkable, as has been already observed, that these bones should have suffered such different degrees of change; and in this respect, the tooth No. 1 is particularly distinguished, retaining still in part the perfect enamel. It deserves the greater attention, as it is deeply worn by mastication, being obviously the tooth of an old animal. It is probably owing to this circumstance that it so nearly approaches in form the tooth of the recent *rhinoceros*. All the other bones coincide fully with the delineations of the fossil *rhinoceros* given by Cuvier.

The fossil bones of the *deer tribe*, found in the Köstritz quarries, are also in the same manner highly calcined. With respect to some of them, it remains doubtful whether they belong to recent species, or to animals of the ancient world. My collection contains :

1. A large fragment of the left scapula of the elk, which is obviously much greater than that of the recent elk, and hence it may, perhaps, really belong to the *alce gigantea*. From Winter's quarry.

2. Four large fragments of the lower halves of very thick deer's horns, partly with the coronets adhering, and several very long branches. They are not sufficiently perfect to ascertain with certainty whether they appertain to the elk, or to other species of deer.

3. Similar fragments, of a smaller size, which may be referable to recent species.

4. Fragments of hollow bones, apparently belonging to the deer tribe.

5. A metatarsal bone of the right foot of an animal of the deer kind, which seems most nearly to resemble that of the roe; but it may have belonged to one of the antelopes, which, however, is mere conjecture. From Kornmann's quarry.

The remains of the deer tribe enumerated above were found partly in the gypsum quarries in the vicinity of Köstritz, and partly in those of the limestone near Politz.

Of the *horse*, several large pieces of bones have occurred, which perfectly agree with those of the existing horse; and the fragment of a jaw, which contains six teeth, appears also referable to the recent species. On the other hand, single teeth are sometimes met with, which are of a much greater length, and more curved in their form, and there can scarcely be a doubt that they are derived from the horse of the ancient world.

In the same manner, the bones of the *ox tribe*, of which the number found is rather considerable, coincide fully with those of existing species; and although some of them are remarkably thick, this distinction is not sufficient to claim for them an origin in the ancient world. It seemed, therefore, superfluous, as in the case of the bones of the horse, to describe them separately.

That the bones of the rhinoceros, of the deer, horse, and ox tribes, should be found in common in so many places, is a phenomenon that deserves particular attention. A similar occurrence has been again lately observed by Cuvier in the neighbourhood of Quercy.

Rudolphi has not ventured to decide upon the fibula of an unknown animal, found in Winter's gypsum quarry. By some, it was supposed to belong to the *palæotherium* of Cuvier, though, it would appear, on insufficient grounds. I have, however, in my possession, a tooth, which was met with in the beds of calcareous tufa near Meissen, that was considered by Rudolphi himself from the first as answering to that of the *palæotherium*; and this animal, thus occurring, as it seems, in alluvial tracts, it is at least possible that its remains may also be found in the cavities filled with loam, in the neighbourhood of Köstritz.

Of *beasts of prey* found near Köstritz, my collection contains:

1. Two considerable fragments of the right and left lower jaws of the hyæna, containing several molar and canine teeth, and the tusks. They perfectly correspond with the delineations of

Cuvier. The magnitude and strength of these remains prove that they belonged to a larger species of the ancient world.

2. Two fragments of the lower jaws of a large animal belonging to the cat family, with remains of the tusks and canine teeth, probably referable to the jaguar of the former world.

3. Single tusks of the same animal.

4. A tusk which seems to belong to the bear found in caverns.

All these bones were discovered in the quarries of the superincumbent limestone near Politz, excepting some of the tusks which were met with in those of the gypsum.

I now proceed to notice the bony remains of the *smaller land animals*, which evidently belong to well-known species of the existing creation. There are a few, however, respecting which some doubts remain, and which I shall particularly notice, as requiring a strict examination.

1. Cervical vertebræ of the fox (*can. vulp.*)

2. The right lower jaw of a young dog, much calcined. The form and position of the teeth, however, which approach to those of the marten (*mustela martes*), excite some doubts.

3. The os occipitis, and dorsal and cervical vertebræ, of the weasel (*mustela vulgaris*). They deserve the greater attention, as similar remains are found also in the beds of calcareous tufa near Meissen.

4. A fragment of the right lower jaw of the shrew-mouse (*sorex araneus*).

5. Five pieces of the lower jaws of very young moles, in a very calcined state. The form of the jaw, however, differs considerably from that of the recent species, possessing at the anterior extremity a prominent hook-like process. Hence Rudolphi is of opinion that they require further investigation.

6. The os sacrum of the hare (*lepus timid*).

7. A skull nearly complete, and single upper and lower jaws, of the hamster (*mus ericetus*).

8. A lower jaw of the squirrel (*sciurus vulg.*)

9. A lower jaw of a similar animal, but somewhat different, which demands further examination.

10. A lower jaw of the field mouse (*mus terrestris*).

11. Several lower jaws of the rat (*mus rattus*).

12. Femora, tibiæ, and phalanges, of the common domestic fowl.

13. Bones of the first toe and middle toe of the owl (*strix bubo*).

14. A fragment of the os femoris of another species of owl.

15. Several bones of frogs, some of which are of a large size.

The bones of these smaller animals also betray for the greater part a high antiquity, although, like the other bones, the degrees of change which they have undergone vary very much.

From the whole of the facts now detailed in the present and my former communication, it is quite evident that, in the country near Köstritz, human bones are found intermingled without order with the bones of animals of the ancient world, and with those of existing species, and under precisely the same circumstances, being firmly enveloped and compacted in the loamy deposit which occupies the fissures and cavities of the bed of gypsum that occurs in that vicinity.

It is undeniable that, in Winter's gypsum quarry, human bones were discovered at the depth of 26 feet from the surface, lying eight feet below the bones of the rhinoceros there also deposited.

The human bones, like those of the other animals, are more or less altered, and deprived of their animal gluten. Human bones and skeletons have also been found in other places, within the tract of the alluvial formations, in the vicinity of the repositories of large land animals of the ancient world; but which have not hitherto received that attention which they deserve.

All these considerations give, on the first view, probability to the conclusion, that the other animals were destroyed at the same time with man, and consequently that the latter was already in existence in this country at the period of the destruction of the large land animals; an opinion which I have already advanced.

Several important doubts, however, arise, when we closely examine into the local peculiarities and geological relations of other tracts, in which animals of the ancient world are usually met with; and when we also consider that such animals are found in common with recent species in the neighbourhood of Köstritz.

As far as hitherto known, such remains of recent species have not been found in any other place intermingled with those of the more ancient,\* and still less with the bones of man. No remains of the large land animals of the ancient world have been met with in the osseous breccia of the coasts of the Mediterranean, which contains, according to the exact determination of Cuvier, only bones of recent species.

All the circumstances under which fossil human remains had hitherto been discovered in the latest deposits, obviously bespoke their modern destruction, and in the greater number of the recorded instances, implements and utensils were found in their vicinity, e. g. in Guadaloupe, near Pabstdorf, Burgtonna, &c.; and in the case of the first place, it is nearly proved, that a burying ground of the Caribs exists there, which is now washed by the sea, and covered with its deposits. All other reported cases of the occurrence of human remains in more ancient

\* To this position, the Kirkdale cave in Yorkshire, in which extinct and existing species occur together, appears to afford a direct answer. (See Prof. Buckland's luminous view of this question, in the paper already adverted to.)—T. W.

strata, or in caverns, accompanying the bones of beasts of prey, have not been confirmed on a closer investigation; and, according to later inquiries, it appears even probable that the bones and skulls of men found in beds of calcareous tufa, have been lodged there in an accidental manner.

It is also to be remarked, that the remains of the large land animals are always found in very low positions, in plains, on the banks of rivers, or in deep valleys, dells, and the concavities of hilly ranges, deposited in the alluvial strata, which is also the case in the Köstritz district; and it is obvious that they were here destroyed, and partly swept into such positions, by the concurrence of great floods. It is, moreover, highly probable that in these operations land floods were the agent, and not the sea. But then the attendant phenomena ought to be uniformly the same. If the remains of man, now found commingled with those of animals of the ancient world and of the existing creation, were destroyed with them at the same time, we ought to find human bones distributed in all parts of the alluvial tracts.\* But this phenomenon has as yet appeared only in the loamy deposit in the Köstritz gypsum, confined to a narrow space, and under peculiar circumstances.

The principal of these circumstances are the following: The narrow valley which extends from Kaschwitz toward Köstritz is bounded on the eastern side, near Politz, by a much more considerable range of eminences than on the other side, which, though gradually becoming more elevated toward Jena, is partially interrupted by dells and circular concavities. The deep narrow valleys and defiles prevailing in the neighbourhood of Jena, in the valley of the Mühl, and further toward Drackendorf and Köstritz, clearly show the power with which the ancient waters raged, when those channels were excavated in which at present flow the Saale, the Elster, and the adjoining smaller streams. It is manifest that during the course of this operation, large tracts of the limestone superincumbent on the gypsum, as well as of the new red sandstone, were torn and swept away, and that the gypsum, thus laid bare, was repeatedly covered, and its cavities filled, with the sediment of the waters, the existing loamy soil.

That the bones of the same species of animals, as well as human bones, should be found without order at different depths, and even immediately under the vegetable soil, lying upon the superior strata of the gypsum, is a circumstance tending rather to confirm than refute the idea of repeated depositions. In the same manner, to find animals belonging to very different epochs, assembled only in the gypsum, where situated in the

\* This argument proceeds on the assumption that the human race had overspread the whole face of the earth, at the period of the formation of the alluvial tracts. The physical evidence, however, hitherto obtained from the investigation of those tracts, seems to indicate the contrary.—T. W.

lowest position, seems to indicate floods in more recent times ; more especially, as no animals of the existing creation have been found in the cavities of the superincumbent limestone,\* placed on a higher level. These cavities, which are filled with the same loamy soil, seem to have been the repositories of the bones of beasts of prey, in the same manner as the caverns of Scharzfeld, Liebenstein, &c. and these bones appear to have been swept away by later floods, and deposited singly in the cavities of the gypsum, situated upon a lower level, and which presents in this spot the form of a basin, being one of the lowest positions in the district. Hence it is highly probable that animals of the ancient world, belonging to very different repositories and very different eras, reaching in part even to the remotest antiquity, have been here repeatedly brought together, and commingled in later periods with the remains of recent animals, and the bones of man ; yet in a manner very different from those met with in strata of calcareous tufa.† This substance, considered as the gradual and tranquil production of great lakes, covering on the spot the skeletons of large land animals previously swept thither and deposited, appears, for the most part, to belong to the more ancient of the alluvial formations ; and this high antiquity is also evinced by the state of the bones found in the tufa, which are perfectly calcined, and also partly petrified. Upon the breaking down of the dams which confined the lakes, and the outflow of their waters, a part of the land animals buried within their bosom appears to have been carried to a greater distance ; and to this cause, and more stormy floods, we may in part attribute certain depositions of loamy soil, in which are sometimes found considerable beds of boulders and pebbles, composed of limestone and other substances. In the district of Köstritz, even boulders of granite, of a considerable size, and which are foreign to the country, are found in the loamy deposition, which occupies the fissures and cavities of the gypsum.

The great difference in the state of calcination exhibited by the Köstritz bones, will long remain enigmatical, as well as several other of the peculiar circumstances that have been adduced ; and I am far from thinking satisfactory the attempt which I have made to explain the phenomena. At present, I consider it as most probable, that the human bones thus found belong to a much later epoch than the large land animals of the

\* This assertion of the author is surprising, after having stated above that the bones of the ox tribe, found in the cavities both of the limestone and gypsum, are all referable to recent species ; while the remains of the horse met with only in the limestone, coincide, it is said, for the most part, with those of the existing species.—T. W.

† The occurrence of bones of the common domestic fowl seems, in particular, to bespeak a much later epoch ; unless we assume (notwithstanding the local peculiarities attending them are contrary to the idea), that they were carried thither by beasts of prey, and that the place of their deposit has been subsequently filled, perhaps even in the latest period, with loam, the bones thus becoming enveloped and cemented in its mass.

ancient world. So much, however, appears to be proved, that they occur here in a really fossil state, having been brought thither by great floods at very remote periods.

*Note by T. W.*—In considering all the natural circumstances detailed by Baron von Schlotheim, following the course of his argument, and comparing both with the instructive facts and views contained in Prof. Buckland's paper (which may be valued as a model of just induction), the question arises, whether the phenomena attending the animal remains found in the district of Köstritz may not be most consistently explained by ascribing them to the effects of diluvian action? The existing form of the surface, the general distribution of the same sandy loamy soil over that surface, extending many miles in every direction, and the deposition of precisely the same soil in the fissures and cavities both of the limestone and gypsum, containing, it would appear, boulders and pebbles of limestone and other substances, and even of granite, a rock not to be found *in situ*, but at the distance of many miles: all these relations seem to bespeak the operation of one great cause at one fixed period. Now of the animal remains met with near Köstritz, it is to be observed,

1. That those which are merely confined to the fissures and cavities of the *limestone* are referable to the horse, belonging partly to an extinct species, but mostly agreeing with the existing horse.

2. The remains found in the cavities and fissures both of the *limestone* and *gypsum*, relate to

The rhinoceros, an extinct species.

The deer tribe, extinct, and apparently also existing, species.

The ox tribe, recent species.

The hyæna, and an animal approaching to the jaguar, both extinct species.

3. While the remains confined to the cavities and fissures of the *gypsum* consist of

The bones of man, of the fox, dog or marten, weasel, shrew-mouse, field-mouse, rat, hamster, squirrel, hare, mole, domestic fowl, owls, and frogs; which agree with existing species, with some exceptions, however, which appear to require further investigation.

It may be asked, if the whole of these remains were deposited at the same era, whence does it proceed that they are not all distributed in the fissures and cavities of the superincumbent limestone, as well as in those of the subjacent gypsum? To this the natural answer seems to be, that, the limestone occupying a more elevated position, the greater mass of animal remains would follow the deeper current of the diluvian waters, and become principal

current. Now this is precisely the position of the gypsum, in which those remains are found. If it be maintained that the animal remains deposited in the cavities and fissures of the limestone and gypsum, belong respectively to different epochs; and that of such as occur in both; namely, the rhinoceros, ox and deer tribes, hyæna and jaguar, these were at later periods washed out of the limestone, and then deposited with the other remains in the gypsum, it may be inquired, why were not the remains of the horse equally dislodged? And as the fissures and cavities of the limestone are described to be entirely filled at present with the same loamy deposit as those of the gypsum, it may also be asked, in what manner could the former be partly emptied, and yet be filled again with the same alluvium at those supposed different epochs? And how could the cavities of the gypsum have remained empty, while those of the limestone were filled during the first of those periods. There appears to be an inconsequence in such a supposition. If, again, it be contended that the whole of these deposits were post-diluvian, it may be remarked that this seems to be contradicted by the same loamy soil which occupies the fissures and cavities of the limestone and gypsum, being spread over the whole country to a great extent. It is true, Baron von Schlotheim appears to suppose the former existence of a lake, whence the waters flowing out, on the breaking down of its barriers, bones belonging to different repositories and different eras have been commingled and swept together. But lakes, in the natural course of things, have a tendency to filling up, by a gradual accumulation on their bottoms, and not to bursting their barriers. Of the former existence of many such inland seas and lakes, there is ample evidence in the present form of the surface of the earth; but the gorges and defiles, by which their waters were discharged, clearly show that those channels were excavated by a mighty power; and as no physical cause now in action could have produced such effects, it may fairly be inferred that it was not post-diluvian. Where then is such a power to be found but in the agency of the diluvian waters, or in the more ancient causes which operated during or subsequent to the deposition of the earlier strata?

It is also to be observed, that in the fissures and cavities of both formations, the remains met with belong partly to extinct animals, and partly to such as agree with existing species. In considering the animal remains discovered in caves and in diluvian tracts, it appears hitherto to have been the practice to confine the terms "animals of the former, ancient, or antediluvian world to such as are now extinct. If the deluge was the great agent by which land animals were destroyed; and if in the existing order of beings the races were renewed with certain exceptions, we might expect to find in the depositions consequent to that catastrophe, the remains both of extinct animals, and of such as correspond with recent species; and we do so find them, e. g. in the cave, or rather series of caves, lately dis-

covered at Oreston, near Plymouth; and that these are referable to antediluvian races, appears to be proved by the unequivocal circumstances attending analogous remains in the Kirkdale cave in Yorkshire. If this be admitted, it will require the stricter caution in distinguishing between diluvian and post-diluvian deposits. The satisfactory solution of the general problem, as far as it relates to man, is probably to be sought more particularly in Asiatic regions, the cradle of the human race. Whether fossil remains of the recent elephant, rhinoceros, hippopotamus, and hyæna, are to be found in the diluvium of tropical climates, becomes also an interesting branch of the inquiry, since it has been conceived that the fossil species of those races distributed throughout the greater part of the temperate and frigid zones of the northern hemisphere, being different, were by nature adapted to those regions, and perished where they lived. In the mean time, in a district so highly interesting as the neighbourhood of Köstritz, it cannot be too strongly recommended to naturalists to continue to explore, and scrutinize with all that precision which the subject obviously demands, all the natural circumstances under which the various deposits of animal remains are to be found in the fissures and cavities both of the gypsum and limestone, as well as in the general tract of sandy loamy soil diffused over the surface of that country. In investigating such a question, a comparative view of the levels of the country, in relation to those deposits, would form an instructive part of the inquiry.

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### ARTICLE III.

*On the Temperature of Mines.* By M. P. Moyle, Esq.

(To the Editor of the *Annals of Philosophy*.)

DEAR SIR,

Helston, Dec. 8, 1822.

THE difference of opinion evinced by Mr. Fox and myself, relative to the augmented temperature of the earth in the descent from its surface, seems to have drawn considerable attention; and as strong arguments are brought in support of decidedly opposite theories, I consider it but just that the public should be in possession of all the facts, sentiments, and experiments, to ground theirs. It, therefore, will become necessary to make a few remarks on what has been advanced by Mr. Fox and Dr. Forbes, in the Transactions of the Cornwall Geological Society, as well as that published by the former in the *Annals of Philosophy*.

Some few of my earliest experiments you have done me the favour of publishing in the *Annals* for April and June last. These experiments, together with others, formed the substance

of my communication to the same Society, and which has been published in the second volume of their Transactions. The substance of this communication I conceive it necessary in this place to detail, as I shall have frequent occasion to refer to it. I have since collected some additional facts which, I conceive, will tend to strengthen very materially the opinion I originally advanced. These shall follow in the second place, reserving any comments for the conclusion.

As it is only by comparing the different results of the experiments of individuals that the truth, or an approximation to it, can be elicited, I conceive too much attention cannot be paid to the manner in which these experiments are conducted. With respect to the temperatures now given, where there has been any degree of uncertainty in the result, they have been taken twice or thrice in the same spot, by different methods, such as burying the thermometer in the earth, or rock of the gallery, in mud or water, lodging in the gallery, in the full stream of water flowing from the veins, by allowing it to remain 15 or 20 minutes during each observation, and by the correspondence of two or more thermometers at the same time.

I have lately ascertained the temperature of three levels which have been driven from Crenver directly under the deepest level in Trenoweth (the mine alluded to in the Annals for April, p. 416). At the depth of 124 fathoms below the adit level, or 936 feet from the surface, it was  $57^{\circ}$ ; at 984 feet,  $58^{\circ}$ ; at 1044 feet,  $58^{\circ}$ . Five months before, when the miners were at work in the last mentioned level, the temperature was  $68^{\circ}$ .

In Oatfield copper mine in May, 1822, the temperature of the air in the engine shaft at the adit level was  $61^{\circ}$ . At the depth of 1092 feet from the surface, it was  $77^{\circ}$ , and at 1272 feet, and 480 feet east of the shaft,  $78^{\circ}$ ; at 1332 feet, and 600 feet east of the shaft, it was  $81^{\circ}$ ; but at the same level, 360 feet west of the shafts, it was only  $78^{\circ}$ : neither of these spots were working places; but the latter was more contiguous to them than the other. At the depth of 1392 feet, in a working part 72 feet east of the shaft, it was  $80^{\circ}$ ; and only 24 feet deeper, 180 feet west of the shaft, in a confined *end*, it was  $85^{\circ}$ . Here the water issuing in considerable streams from two small veins, at the bottom of the gallery, a few feet apart, indicated the different temperatures of  $82^{\circ}$  and  $86\frac{1}{2}^{\circ}$ .

Since the temperature of the different parts of this mine has been taken, the pumps have been drawn up from the deepest part, and the shaft, below the depth of 182 fathoms from the surface, has been for some months full of water. At this level, the temperature was previously  $77^{\circ}$ , but a few months afterwards (in Sept. last), when the water had risen to the level, its temperature, a few feet below its surface, was  $69^{\circ}$ ; and at the depth of 72 feet in the water, it was  $71^{\circ}$ . A *fortnight* after this, I repeated the experiment, and found the temperature, a few feet

below the level,  $66^{\circ}$ ; and at 12 fathoms, as before, deep in the water,  $67^{\circ}$ ; it having cooled  $3^{\circ}$  in a fortnight, and  $11^{\circ}$  since its admission into the shaft.

In Crenver copper mine, very little work has been done for a great while. At 300 feet, and 300 feet east of the engine shaft, the temperature was  $55^{\circ}$ ; at 432 feet deep,  $56^{\circ}$ ; at 492 feet,  $61^{\circ}$ ; at 617 feet,  $62^{\circ}$ ; at 672 feet,  $64^{\circ}$ ; at 732 feet,  $64^{\circ}$  in the shaft; but in a gallery 360 feet east, only  $60^{\circ}$ ; at 792 feet,  $63^{\circ}$  in the shaft; but 1200 feet east of it, only  $61^{\circ}$ ; at 852 feet,  $62^{\circ}$ ; but at 180 feet west, it was  $64^{\circ}$ ; and at 1200 feet west, it was  $68^{\circ}$ .

Huel Abraham is on the same lode as Trenoweth, Crenver, and Oatfield; and is, in almost every part, in full work. At 1332 feet below the surface, the temperature was  $84^{\circ}$ . At 1392 feet deep, at the extremity of the level, on a Monday morning, before the workmen had returned to labour, and where a machine was erected for blowing fresh air to the miners, the thermometer stood at  $90^{\circ}$ ; but a few days afterwards, when a communication had been formed, it fell to  $86^{\circ}$ . At the depth of 1452 feet, it was, in one gallery,  $84^{\circ}$ ; and in another (the only spot where there were no workmen), it was  $86^{\circ}$ .

I shall now mention the result of some experiments, to ascertain the temperature of the water, at different depths, in mines which have been long abandoned.

Herland copper mine, in Gwinear, has ceased working (except above the adit), 15 years. On the 28th May, 1822, while the temperature in the shade at the surface was  $64^{\circ}$ , and in the sun  $74^{\circ}$ , I found that of the water running through the adit, 32 fathoms deep,  $52^{\circ}$ ; as we approached the engine shaft, it was increased to  $53^{\circ}$ ; and on sinking two self-registering thermometers, properly secured in a box with iron weights, the temperature, at the depth of 10, 20, 40, 60, and 100 fathoms in the water, and 792 feet from the surface, was uniformly  $54^{\circ}$ . In another shaft in this mine, 360 feet north-west of the great engine shaft, the water running through the adit was  $54^{\circ}$ ; and at 10, 20, and 40 fathoms deep,  $56^{\circ}$ .

On the 8th of June, 1822, I visited Huel Poel and Huel Rose lead mines near Helston. In the former, the temperature of the water, at 10 and at 20 fathoms deep, was  $53^{\circ}$ . In another shaft it was precisely the same.

In Huel Rose, the water in the engine shaft was, at 60 feet deep,  $53\frac{1}{2}^{\circ}$ ; at 120 feet,  $53\frac{1}{2}^{\circ}$ ; at 240 feet,  $53\frac{1}{2}^{\circ}$ ; and at 300 feet, only  $53^{\circ}$ . The time allowed for the thermometer to remain at the different depths (except the last) was 10 minutes, which, perhaps, was scarcely long enough.

In Huel Alfred (visited in July, 1822), the temperature of the water in the adit, 18 fathoms below the surface, was  $56^{\circ}$ ; and at the several depths of 60, 120, 240, 360, 600, and 672 feet of water, or 780 feet from the surface, it was uniformly the same.

Relistian mine in Gwinear (visited on the same day) is reported to have been, when at work, much hotter than mines in general; but I found the water in the adit, 25 fathoms deep, only  $55^{\circ}$ ; and at 10, 20, and 25 fathoms below the adit, it was still the same.

Huel Ann an old tin mine, 353 feet above the level of the sea, is situated in granite, and is on the same lode as Huel Trumpet, in the parish of Wendron. This mine has not been wrought for the last 20 years, but is to be immediately reworked; and men have consequently been employed to open that part of the mine above the adit. On uncovering a shaft, I found that I could drop 120 fathoms perpendicular. This I did with half a hundred weight, to which was affixed a common bottle properly corked, sealed and secured, so that with a small cord affixed, the cork was withdrawn after a lapse of a few minutes. The water thus drawn from the bottom was of the temperature of  $52^{\circ}$ , and a pint of it left only a residuum of  $1\frac{1}{2}$  gr. I then sunk a day and night thermometer properly secured, as in my former experiments, and found the temperature at the depth of 30, 60, 120, 240, and at 720 feet, or the bottom of the mine, uniformly of the temperature of  $52^{\circ}$ . The adit is 14 fathoms, thus making the depth from the surface 804 feet; and the thermometer was allowed to remain at the bottom of the water for four hours.

On repeating my experiments on the temperature of the water in the Herland mine, I found the heat at all depths, as before stated, viz.  $54^{\circ}$  in the old engine shaft, and  $56^{\circ}$  in another about 60 fathoms distant; and in a third, not before tried, the water was only  $52^{\circ}$ . I was given to understand by Capt. S. Grose, who accompanied me, that all these shafts extended to nearly the same depth. This circumstance I conceive rather remarkable, and clearly proves the operation of different causes of temperature in a very circumscribed portion of ground.

At Huel Franchise, a tin and copper lode in the parish of Wendron, 313 feet above the level of the sea, and parallel with Huel Trumpet. The water in this mine, which has ceased working for about two years, was in November last at the bottom, or 180 feet in depth,  $51^{\circ}$ .

Huel Nancy is on the same lode, but half a mile east of the latter mine. This mine is about 200 feet deep, and indicated at all depths  $51^{\circ}$ ; while the temperature in the shade at the surface was  $55^{\circ}$ . This mine has stopped more than 20 years.

In making my experiments with the registering thermometer, in order to obtain as correct results as possible, I always reduce the degree of the mercurial one to about the freezing point, by sprinkling its bulk with ether, and by raising the spirit one with my tongue, bringing the indices to correspond before each immersion.

There appears to be little or no difference in the mean temper-

ature of the same spot in a *deep* and confined part of a mine at work, in summer or winter; or at least the miners are not sensible of any. Capt. W. Teague assures me, that he has often met with ice in great abundance in Tin-Croft mine, at the depth of 318 feet below the surface; and in such quantities that the ladders have been impassable; deep crevices in the walls have been completely filled, and icicles hanging abundantly around him.

As liquid and aeriform bodies convey heat on a different principle from that observed in solids, viz. by an actual change in the situation of their particles, instead of an augmentation of their bulk, as in solids, in proportion to the absorption of caloric, it may not be deemed irrelevant to explain this principle, and apply it to the circumstances of the water in a relinquished mine, and show how the whole water becomes of an uniform temperature.

Supposing the greatest temperature is at E, the bottom of the mine, and the whole becomes filled with water by infiltration from the surface. This water must bring with it a medium temperature of the surface, and the different strata of earth through which it has passed, which, if of less heat than that possessed by the earth at the bottom, will of course become heated to the same degree; it, therefore, is expanded, and, becoming specifically lighter, ascends to the surface, and is replaced by a colder portion from above. This, in its turn, becomes heated and dilated, and gives way to a second colder portion; and thus the process goes on as long as the fluid is capable of imbibing heat. Therefore, whether the thermometer is sunk to A, B, C, D, or E, in an old mine, it must be found of an equable degree.

A
—
B
—
C
—
D
—
E

M. de Mairan asserts, that the rigour of the cold of winter is tempered by the heat imparted to the atmosphere by the earth itself, which heat, probably possessed from its origin, is preserved and renewed by the incessant influence of the sun, to which one half of its surface is constantly exposed.

Admitting this fact, the temperature of the atmosphere must depend on the capacity of the earth for receiving and retaining heat, and for communicating it to the surrounding medium. But as the earth is composed of land and water, it should be considered that the capacities of these constituent parts, for receiving heat are very different. Land, particularly when dry, receives heat from the sun's rays very readily, but transmits it through its own substance to great depths very slowly; and, on the other hand, water, by reason of its transparency, receives heat very slowly, but diffuses what it receives more readily.

Dr. Hales found, that in the month of August, 1724, when the air and the surface of the earth were both at  $88^{\circ}$ , a thermometer placed only two inches below the surface of the earth stood at  $85^{\circ}$ ; another, 16 inches under the surface, indicated  $70^{\circ}$ ; and a

third, 24 inches deep, stood at  $68^{\circ}$ . The last two thermometers preserved the same temperature, both day and night, till the end of the month, and then fell to  $61^{\circ}$ , the earth obstinately retaining its heat at that depth, though the temperature of the air frequently varied. On the 26th of October, a thermometer exposed to the air stood at  $35.5^{\circ}$ ; but one sunk two inches below the surface was heated to  $43.85^{\circ}$ ; another, at the depth of 16 inches, stood at  $48.8^{\circ}$ ; and another, 24 inches deep, showed  $50^{\circ}$ ; and from the 1st to the 12th of November, when the temperature of the external air was  $27^{\circ}$ , a thermometer placed at the depth of 24 inches, stood at  $43.8^{\circ}$ ; but from the month of March to that of September in the following year, the external air was constantly warmer than the earth at the depth of 16 or 24 inches.

From these experiments, it may be inferred that the surface of the earth is much heated during summer, but that the heat descends very slowly, a great part of it being communicated to the air; that during the winter the earth gives out to the air the heat which it had received during the summer; and that wet summers are generally succeeded by cold winters.

Marriotte's experiments furnish nearly similar results to those of Dr. Hales. Hence it appears that at the distance of about 80 or 90 feet below the surface, provided that there be a communication with the external air, or at a less depth if there be no communication, the temperature of the earth admits of very little variation, and generally approaches to the mean annual heat.

M. Van Swinden has observed, that the greatest cold, and even that which is below  $0^{\circ}$  of Fahr. if it lasts no more than a few days, penetrates no deeper than 20 inches, when the earth is covered with snow, and not above 10 inches, if no snow lies on the surface.

Such facts tend to prove that the heat of the earth does not increase as we descend into it; but at the greatest depths, it is nearly the same as the mean annual temperature of the latitude.

The following table of temperatures, taken by myself, will be convenient for a general reference, distinguishing the mines at work from those long since abandoned; and the medium in which the temperatures were taken:

MINES AT WORK.										ABANDONED MINES.																						
Depth from sur- face in feet.	Huel Unity.		Creegbrann.		Huel Trumpet.		Oatfield.		Cronver.		Huel Abraham.		Huel Tre- noveth.		Huel Pool.		Huel Rose.		Relistian.		Herland.		Huel Alfred.		Huel Ann.		Trevenen.		Huel Fanchise.		Huel Nancy.	
	a.	w.	a.	w.	a.	w.	a.	w.	a.	w.	a.	w.	a.	w.	a.	w.	a.	w.	a.	w.	1.	2.	3.	a.	w.	a.	w.	a.	w.	a.	w.	
60 to 120	54	54	55	51	56	56	61	61	60	60	60	60	54	53	53	53	53	53	55	55	54	56	56	56	56	56	53	53	53	53	51	51
120 150	54	54	55	51	56	56	61	61	60	60	60	60	54	53	53	53	53	53	55	55	54	56	56	56	56	56	53	53	53	53	51	51
150 200	54	54	55	51	56	56	61	61	60	60	60	60	54	53	53	53	53	53	55	55	54	56	56	56	56	56	53	53	53	53	51	51
200 250	54	54	55	51	56	56	61	61	60	60	60	60	54	53	53	53	53	53	55	55	54	56	56	56	56	56	53	53	53	53	51	51
250 300	54	54	55	51	56	56	61	61	60	60	60	60	54	53	53	53	53	53	55	55	54	56	56	56	56	56	53	53	53	53	51	51
300 350	54	54	55	51	56	56	61	61	60	60	60	60	54	53	53	53	53	53	55	55	54	56	56	56	56	56	53	53	53	53	51	51
350 400	54	54	55	51	56	56	61	61	60	60	60	60	54	53	53	53	53	53	55	55	54	56	56	56	56	56	53	53	53	53	51	51
400 450	54	54	55	51	56	56	61	61	60	60	60	60	54	53	53	53	53	53	55	55	54	56	56	56	56	56	53	53	53	53	51	51
450 500	54	54	55	51	56	56	61	61	60	60	60	60	54	53	53	53	53	53	55	55	54	56	56	56	56	56	53	53	53	53	51	51
500 550	54	54	55	51	56	56	61	61	60	60	60	60	54	53	53	53	53	53	55	55	54	56	56	56	56	56	53	53	53	53	51	51
550 600	54	54	55	51	56	56	61	61	60	60	60	60	54	53	53	53	53	53	55	55	54	56	56	56	56	56	53	53	53	53	51	51
600 650	54	54	55	51	56	56	61	61	60	60	60	60	54	53	53	53	53	53	55	55	54	56	56	56	56	56	53	53	53	53	51	51
650 700	54	54	55	51	56	56	61	61	60	60	60	60	54	53	53	53	53	53	55	55	54	56	56	56	56	56	53	53	53	53	51	51
700 750	54	54	55	51	56	56	61	61	60	60	60	60	54	53	53	53	53	53	55	55	54	56	56	56	56	56	53	53	53	53	51	51
750 800	54	54	55	51	56	56	61	61	60	60	60	60	54	53	53	53	53	53	55	55	54	56	56	56	56	56	53	53	53	53	51	51
800 850	54	54	55	51	56	56	61	61	60	60	60	60	54	53	53	53	53	53	55	55	54	56	56	56	56	56	53	53	53	53	51	51
850 900	54	54	55	51	56	56	61	61	60	60	60	60	54	53	53	53	53	53	55	55	54	56	56	56	56	56	53	53	53	53	51	51
900 950	54	54	55	51	56	56	61	61	60	60	60	60	54	53	53	53	53	53	55	55	54	56	56	56	56	56	53	53	53	53	51	51
950 1000	54	54	55	51	56	56	61	61	60	60	60	60	54	53	53	53	53	53	55	55	54	56	56	56	56	56	53	53	53	53	51	51
1000 1150	54	54	55	51	56	56	61	61	60	60	60	60	54	53	53	53	53	53	55	55	54	56	56	56	56	56	53	53	53	53	51	51
1150 1250	54	54	55	51	56	56	61	61	60	60	60	60	54	53	53	53	53	53	55	55	54	56	56	56	56	56	53	53	53	53	51	51
1250 1300	54	54	55	51	56	56	61	61	60	60	60	60	54	53	53	53	53	53	55	55	54	56	56	56	56	56	53	53	53	53	51	51
1300 1350	54	54	55	51	56	56	61	61	60	60	60	60	54	53	53	53	53	53	55	55	54	56	56	56	56	56	53	53	53	53	51	51
1350 1400	54	54	55	51	56	56	61	61	60	60	60	60	54	53	53	53	53	53	55	55	54	56	56	56	56	56	53	53	53	53	51	51
1400 1450	54	54	55	51	56	56	61	61	60	60	60	60	54	53	53	53	53	53	55	55	54	56	56	56	56	56	53	53	53	53	51	51

Secondly, it is necessary to make a few remarks on what Mr. Fox and Dr. Forbes have stated, trusting that those gentlemen will excuse the liberty which I have taken.

In Mr. Fox's tables, the irregular ratio of augmented temperature is very conspicuous; as it appears to be as hot at the depth of 600 feet in Chacewater mine, as it was in Dolcoath at the depth of 1440 feet, each being  $82^{\circ}$ . In the next place, it is as hot at 420 feet in the United Mines, as in Dolcoath at 1200 feet; as hot in Chacewater at 480 as at 840 feet in Huel Damsel; as hot at 780 feet in Treskerby as at 1380 in Dolcoath, &c. &c.; and hotter in the United Mines at the depth of 1080 feet than in any other mine in the county. From this statement, it appears that the temperature of the earth in Chacewater increases  $27^{\circ}$  in 540 feet in depth; while Dolcoath is augmented only the same in 1380 feet; and the United Mines the same number of degrees in 1080 feet, or exactly double the depth. These facts would induce me to look upon the progressive ratio of heat in a different light from those gentlemen.

Mr. Fox and Dr. Forbes are at variance in opinion about fixing a limit as to the precise point below the surface, for the commencement of augmented temperature: an examination of an experiment or two will prove the confidence we may place in the conclusions of either.

Mr. Fox commences at 50 feet, and Dr. Forbes at 200 feet, below the surface; and from the extreme temperature observed in our deepest mines, would deduct  $6^{\circ}$  for artificial and extraneous causes of heat, thus reducing the actual degree at about 1300 or 1400 feet from  $72^{\circ}$  to  $74^{\circ}$ ; and after the ratio of  $1^{\circ}$  for every 50 feet, it would be at the depth of 1044 feet,  $68^{\circ}$ . Now reverse the order of calculation, and we shall find Mr. Fox to make it  $69\frac{1}{2}^{\circ}$ , and Dr. Forbes,  $66\frac{1}{2}^{\circ}$  for the same depth. This is the precise depth of the lowest of the three levels driven under Trenoweth from Crenver, the temperature of which is actually only  $58^{\circ}$ , although a spot not in the course of working, yet has a distant communication with the mine in general, and at a working spot on the same level, the temperature is but  $68^{\circ}$ , after being exposed to all the extraneous sources in common.

Dr. Forbes remarks (and Mr. Fox in his last communication says the same), "hence it follows, that the *natural temperature* of the earth in the mines in Cornwall, at the depths mentioned, must be considerably above that of the mean of the climate. This conclusion is equally deducible from many facts which have been observed in mines. The most conclusive of these is the high temperature of extensive collections of water in abandoned mines, or in parts of mines that have been long since abandoned. In cases of this kind, it is impossible to believe that the temperature can have been derived from any other source than the rocky walls of the cavity in which it is contained; and as these walls could not derive their temperature

from any foreign source, the conclusion is equally in favour of the natural high temperature of the body of this earth at certain depth."

Will these gentlemen, I would ask, still maintain the same sentiments? If so, their theory must fall to the ground, as we can now clearly prove that these very collections of water possess even a less temperature than the supposed mean of the climate; e. g. Huel Ann, and the third shaft in Herland; one 130, and the other 160 fathoms in depth, Ding Dong, Huel Rose, Huel Franchise, &c.

The only instances of importance, Dr. F. brings in support of this hypothesis, are the following: a large isolated collection of water in Botallack mine is stated to be  $62^{\circ}$  at the depth of about 400 feet from the surface. Here it may be necessary to refer to his general description, and we find it is an old working full of water, 80 fathoms deep below the gallery at 400 feet, and is under the sea. The surface of this water is  $62^{\circ}$ , while the air was  $66^{\circ}$ ; the heat of this water at the bottom of the working is not given. He continues, "a still stronger instance, perhaps, in support of the same conclusion, is afforded by the details of the other submarine mine Little Bonnds. This mine was formerly worked to the depth of 500 feet. Of late years, very little has been done, and the water has risen to within 40 fathoms of the adit, where it is kept stationary by the partial operation of the pumps. There is thus a permanent body of water extending from the depth of 300 to that of 500 feet below the surface, and, perhaps, to half that distance horizontally. This water, as discharged by the pumps in 1822, is of the temperature of  $56\frac{1}{4}^{\circ}$ ." Pray what has this to do with the temperature of the central part or bottom of the collection? And yet Dr. F. in nearly the following page, states, that a large body of water resembling the last has accumulated in the old wrought part of Ding Dong mine; at the depth of 444 feet below the surface, the workmen had just cut through the barrier which divided them from this old working, and the stream of water which issued forth (and which was the bottom of the large collection), was only  $52\frac{1}{4}^{\circ}$ , thus at once proving what is actually the case, that, as I before stated, it may be as cold at the very centre of the earth as at any distance beneath its surface.

In the next place, I do not conceive that their opinion can be supported, because Dr. Forbes's philosophical reasoning on all the extraneous sources of caloric falls short of what is actually observed, and that we must attribute this extra portion as derived from the earth itself; for I should imagine that there are few more difficult problems, than a true estimation of the power of the *infinite* sources of caloric in a mine in the full course of working.

In making a few observations on the foregoing experiments, I must remark in the first place, that in mines which are at work

scarcely two places of equal depth below the surface, and under similar circumstances, exhibit the same temperature. As these differences and irregularities of temperature always occur in mines which are at work, they must arise from adventitious causes. I am, therefore, of opinion, that the true temperature of any part of a mine in the full course of working is difficult of attainment, and that we must have recourse to those mines, and parts of mines, which have been long since quitted by the miner, in order to obtain any thing like a true datum.

The hot springs which frequently occur, while they prove the existence of causes sufficient to give them their high degree of temperature, prove, at the same time, by their rarity, the local and adventitious nature of those causes.

I cannot conclude without stating, that I hope those gentlemen who have taken such an interest in the present inquiry, will not lose sight of it, but avail themselves of every opportunity likely to develop this apparently mysterious subject; and as we are in possession of the temperatures of Huel Ann, and the Herland mine, and which are to be immediately reworked, and of Huel Abraham, our deepest mine, which report says is about to be relinquished, I think a favourable opportunity will occur to reverse the nature of our present experiments, and thus afford by synthesis, as well as analysis, an accurate statement of facts.

I am, Sir, your obedient servant,

M. P. MOYLE.

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#### ARTICLE IV.

*On the State of Water and Aëriform Matter in Cavities found in certain Crystals.* By Sir Humphry Davy, Bart. PRS.\*

THERE are few inquiries in natural science more calculated to awaken our curiosity, than those relating to the changes which the matter composing the surface of our globe has undergone. The imagination is excited by the magnitude of the operations by the obscurity of the phenomena, and the remoteness of the time at which they occurred; and all the intellectual powers are required to be brought into activity to find facts or analogies, or to institute experiments, by which they may be referred to known causes.

The crystallizations constituting the whole of the rocks which are usually called primary, and those found in such abundance, even in the rocks which are termed secondary, prove that a considerable part of the materials of the surface of the globe

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must have been either fluid or aeriform ; for these are the only states from which the regular arrangements of the molecules of bodies constituting crystals, can be produced.

Geologists are generally agreed that the greater number of the crystalline mineral substances must have been previously in a liquid state ; but different schools have supposed different causes for their solution ; some attributing this effect principally to the agency of water, others to that of heat.

When, however, it is considered, that the solvent power of water depends upon its temperature, and its deposition of solid matters upon its change of state or of temperature ; and that being a gravitating substance, the same quantity must always belong to the globe, it becomes difficult to allow much weight to the arguments of the Wernerians or Neptunists, who have generally neglected, in their speculations, the laws of chemical attraction.

There are many circumstances, on the contrary, favourable to that part of the views of the Huttonians or Platonists, relating to the cause of crystallization ; such as the form of the earth, that of an oblate spheroid flattened at the poles ; the facility with which heat, being a radiating substance, may be lost and dissipated in free space ; and the observations which seem to show the present existence of a high temperature in the interior of the globe.

I have often, in the course of my chemical researches, looked for facts or experiments, which might throw some light on this interesting subject, but without success, till about three years ago ; when, in considering the state of the fluid and aeriform matters included in certain crystals, it appeared to me, that these curious phenomena might be examined in a manner to afford some important arguments as to the causes of the formation of the crystal.

It is well known that water, and all fluids at usual temperatures, are more expansible by heat than siliceous or other earthy matters ; and supposing these crystals to have been formed, and the water or fluid enclosed in them, at a pressure and temperature not very unlike those of our existing atmosphere, this fluid ought to fill nearly the same space as when included, and the elastic fluid confined with it, supposing it non-absorbable, ought to be in the same state of density. On the contrary, if the earthy matter and the fluid separated from each other under a much higher temperature than that now belonging to the surface, a certain vacuum might be expected in the cavity from the contraction of the fluid, and if any gas were present, a considerable rarefaction of it ; and though, supposing a much higher temperature on the surface of the globe, the atmosphere formed by aqueous vapour must have had much greater absolute weight, which, as liquids are *compressible*, must have influenced the volume of the fluid at the time it was enclosed, a circumstance

which would render it impossible to draw any conclusion as to the exact temperature, yet still the experiments appeared to offer, on any view, interesting results; and I was the more desirous of performing them, as I believe the nature of the fluid and aeriform matters included in rock crystals and other siliceous stones, has never been accurately ascertained.

Having purchased some crystals, and having had others committed to my care by the liberality of my Brother Trustees of the British Museum, and of my friend Prof. Buckland, I proceeded to make the necessary experiments upon them. It will be improper for me to take up the time of the Society by a minute description of my manipulations. Holes were drilled in the crystals by the use of diamonds, generally by Mr. Newman, under distilled water, or mercury, the gas was expelled by the introduction of wires, and the fluids included in the cavities were drawn out by means of fine capillary tubes, and experiments were afterwards made to determine the space they occupied, which had been accurately measured and marked upon the crystal. The chemical nature of the fluid and gas was determined by processes which were necessarily difficult from the smallness of the quantities operated upon; but which are too well known to the chemical philosophers of this Society to need description.

The first three crystals that I examined were from Schemnitz, in Hungary; the cavities that they contained were proved not to be permeable to the atmosphere, by exposure to rarefied air, alone, and under water, in the receiver of an air-pump, a circumstance which it was necessary always to attend to, in order to render the experiment availing.

A cavity in one of the crystals was pierced under oil, three under distilled water, and one under mercury. In all of them the fluid rushed in when the cavity was opened, and the globule of elastic fluid contracted so as to appear from six to ten times less than before the experiment. The fluid in all the crystals (in two it was minutely examined) was found to be water nearly pure, containing only a minute portion of the alkaline sulphates. The elastic fluid, as well as I could ascertain from the very minute quantities I could procure, appeared to be azote, unmixed with any other substance.

The largest cavity, which was in the crystal put into my hands by Prof. Buckland, contained a space equal to 74.5 grains of mercury; the water in it equalled in volume 48.1 grain measures of mercury; and the globule of air, after the experiment, equalled in diameter a globule of mercury weighing 4.2 grains, so that the elastic fluid had contracted at least between six and seven times.

In the other experiments, the cavities being much smaller the quantities of air and fluid could not be accurately measured; but there seemed to be nearly the same relation between the space

filled by fluid, and that containing aeriform matter; and in all of them the contraction of the globule of aeriform matter was evidently greater, and in one instance to less than 1-10th of its original bulk.

The fourth crystal that I experimented upon was of unknown locality; but I have reason to believe that it was from Guanaxuato, in Mexico, as it strongly resembled some that Mr. Heuland showed me from that place. The cavity in it was extremely small, and when pierced into, under distilled water, the globule of gas, from being one-eighth of an inch in diameter,\* diminished so as to be less than 1-25th; so that its rarefaction was much greater in this than in the other instances; the water was too small in quantity to be minutely examined; it seemed to be nearly pure, producing a cloudiness barely perceptible in solutions of nitrate of silver and muriate of baryta.

It was an interesting point to ascertain whether the same circumstances occurred in productions found in rocks which have been generally considered as of igneous origin, such as the basaltic rocks in the neighbourhood of Vicenza, the chalcedonies of which so often afford included water. I found it much more easy to make experiments of this kind, and to procure specimens, which were abundantly supplied to me from the same sources as those I have just referred to; and though some of these specimens proved to be permeable to the atmosphere, and to have been filled with water artificially, yet many occurred, in which the sides of the cavity were absolutely impervious to air or water.

The results that I obtained were very analogous. Water containing very minute quantities of saline impregnations, occasioning barely a visible cloudiness in solutions of silver and of muriate of baryta, was found to be the fluid; the gas was azote, but it was in a much more rarefied state than in the rock crystals, being between 60 and 70 times as rare as atmospheric air.

The quantity of water was to the void space in greater proportion than in the rock crystals. In the instance in which the most accurate experiment was made; namely, on the great specimen preserved in the collection of the British Museum, and which weighed 380 grains, the quantity of water was 29.9 grains, the space occupied by aeriform matter was equal to 11.7 grains of water, the volume of the globule of gas at the common pressure was to that of its rarefied volume as 1 to 63.

It occurred to me that atmospheric air might have been originally the elastic fluid included in these siliceous stones and in the crystals, and that the oxygen might have been separated from the azote by the attraction of the water, and a direct experiment

\* I have not thought it necessary to refer to the heights of the barometer and thermometer in these experiments, as it is impossible to gain any other than general results, upon quantities in which differences arising from atmospheric temperature and pressure, would be quite unappreciable.

seemed to confirm this idea. A chalcedony which had been bored was placed in water free from air under a receiver, which was exhausted till a portion of gas from the interior of the crystal had escaped into a proper receptacle. This gas examined by nitrous gas was found to contain nearly as much oxygen as atmospheric air; so that there is every reason to believe that the water had emitted oxygen during the exhaustion.

I endeavoured to find some calcareous secondary rocks, or crystals belonging to them, containing cavities, on which experiments of the same kind might be made; but in a number of trials, I have as yet found none impermeable to the atmosphere; and the cavities of such, when bored, are always found to contain atmospheric air in a common state of density.

I was surprised to find that this was the case even with cavities in calcareous spar in the centre of a limestone rock; yet these cavities which contained atmospheric air did not fill with water when the stone was placed in water under an exhausted receiver. When, however, it was dry, and placed in a receiver alternately exhausted and filled with hydrogen, the air that was produced by piercing the cavities, was found mixed with hydrogen; proving that the substance of the stone was permeable to elastic fluid.

I hope soon to be able to make further researches on this subject; but in reasoning upon the vacuum, or rarefied state of the aeriform matter in the cavities of these rock crystals and chalcedonies, it appears difficult to account for the phenomenon, except on the supposition of their being formed at a higher temperature than that now belonging to the surface of the globe; and the most probable supposition seems to be, that the water and the silica were in chemical union, and separated from each other by cooling.

Water in the temperature of the arctic winter is constantly a crystallized body. As a fluid, its solvent powers are increased as its heat becomes higher, and, when elastic, the density of its vapour is exalted in proportion to its heat; so that an atmosphere of steam, supplied from an indefinite source above water, would render it capable of receiving a very high degree of heat. Lime retains water in combination at a heat above 250° Fahrenheit; baryta retains it (even under ordinary pressures) at a strong red heat, and fuses with it. It is extremely likely that a liquid hydrate of silica would exist, under pressure, at high temperatures; and like all liquid bodies in the atmosphere, would probably contain small quantities of atmospheric air; and such a supposition only is necessary to account for the phenomena presented by the water in rock crystal and chalcedony.

As, however, steam or aqueous vapour may be considered as having a share in these results, if it be supposed included in the cavity, no exact conclusions can be drawn from the apparent degree of contraction of the water; particularly as the late inge-

nious researches of Mr. Perkins show, that water is much more compressible than was formerly imagined; and the volume of water, however high its temperature, must be influenced by the pressure to which it is exposed; so that a certain compressing weight may not only impede, but altogether counteract, the expansive force of heat.

Many speculations might be indulged in on this subject, but I shall not at present enter upon them; and I shall conclude by observing, that a fact, which has been considered by the Neptunists, above all others as hostile to the idea of the igneous origin of crystalline rocks, namely, the existence of water in them, seems to afford a decisive argument in favour of the opinion it has been brought forward to oppose.

#### APPENDIX.

Since the foregoing pages were communicated to the Royal Society, I have made some new experiments on the same subject; all of them, except two, offered results of the same kind as those I have detailed, and upon such I shall not enter; but these two, from their peculiarity, will not, I trust, be thought unworthy of a particular notice.

In examining, with Mr. Heuland, the beautiful specimens of rock crystals in the collection of Charles Hampden Turner, Esq. I observed one crystal which, Mr. Heuland informed me, was from La Gardette, in Dauphiné, that contained a considerable cavity, in which there was a viscid brownish liquid, resembling in its appearance and consistence linseed oil. As the void space or cavity filled with aeriform matter appeared considerable in proportion to the fluid, I expressed a desire to pierce the crystal; and Mr. Turner, hearing of my wish, was so kind as to gratify it in the most polite and liberal manner, by presenting to me the specimen. With Mr. Newman's assistance I made the usual experiments upon it. The cavity was pyramidal, and nearly the third of an inch in diameter. I soon ascertained that the fluid was not water, as it congealed and became opaque at a temperature of  $56^{\circ}$ . When the crystal was pierced under distilled water, the water rushed in and entirely filled the cavity, so that no other aeriform matter but the vapour of the substance could have been present: the water was rendered white and cloudy, apparently by the substance. I endeavoured to collect some of it for chemical examination, but it was too small in quantity (not equalling in volume one-sixth of the volume of the cavity), to be submitted to analysis. It swam on the water, had no distinct taste, but a smell resembling naphtha; a portion of it taken out mixed with the water, when exposed to heat acted like fixed oil, and it seemed to have a high temperature of ebullition. It inflamed, producing a white smoke.

The fact, of almost a perfect vacuum existing in a cavity con-

aining an expansible but difficultly-volatile substance, may be considered as highly favourable to the theory of the igneous origin of crystals : the other experiment is of a nature entirely different, though its result *may* be explained in the same supposition.

In examining a crystal in the collection of the Royal Institution, and which from its characters I believe to be from Capaó d'Olanda, Province of Minas Geraes, Brazil, I observed that the quantity of aeriform matter was unusually small in proportion to the quantity of fluid, in two or three cavities not occupying one-tenth or one-twelfth of the space ; and from the peculiarity of its motion, it appeared to be more likely to be compressed than rarefied elastic fluid ; and in piercing the sides of the cavities, I found that this was the case ; it enlarged in volume from ten to twelve times ; the fluid was water, but the gas was too minute in quantity to be examined.

It will be interesting to ascertain under what circumstances, and in what situations, crystals of this kind are found. If they be supposed of igneous origin, they must have been formed under an immense weight of atmosphere or fluid, sufficient to produce a compression much more than adequate to compensate for the expansive effects of heat, a supposition which, in consequence of Mr. Perkins's experiments, already alluded to, may be easily formed.

## ARTICLE V.

*On Grey Whin.* By N. J. Winch, Esq.

(To the Editor of the *Annals of Philosophy*.)

DEAR SIR,

Newcastle-upon-Tyne, Dec. 18, 1822.

THOUGH I believe it to be generally known to such of your readers as have paid attention to the geology of Britain, that the workmen engaged in the collieries and quarries of the north of England and Scotland apply the term *whin* not only to basalt or greenstone, but to most other species of hard stone, yet I am not aware that a peculiar rock called by them *grey whin* has ever attracted particular notice. The rock constituting the stratum, or possibly strata in question, consists of minute grains of hair-brown compact felspar or clinkstone, with a few spangles of silvery mica scattered through it. From the close cohesion of its small particles, it might be mistaken at first sight for clinkstone, strongly resembling the well-known rock of Lamlash, but by a lens, the granular texture is detected, and the difference between them easily proved. Owing to this compact texture,

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and the nature of its component parts, it is both heavy and tough, requiring a smart blow of the hammer to break it. Before the blowpipe, it fuses without much difficulty, into a pale-brown glass; it is not magnetic like basalt. At St. Anthon's colliery (see Geological Transactions, vol. iv. p. 41), the bed lies nearly 104 fathoms from the surface of the earth, and is a foot in thickness; its situation is between a stratum of strong white post (sandstone) seven feet thick, and a stratum of blue metal (shale, slate clay) eight and a half feet thick. In Walbottle Dene, five miles west of Newcastle, a similar stratum creeps out by the edge of the brook; but its thickness or relative position cannot there be so accurately ascertained as at St. Anthony's, where a shaft has been sunk through it. Whether this be a continuation of the same bed, I cannot determine; but the high main coal cropping out at Benwell Hills, two miles to the east of Walbottle, and this bed lying 29 fathoms below that seam, I am inclined to consider them one and the same, notwithstanding the stone is finer grained at the latter place; for all our coal strata rise to the clay in succession towards the west and south-west.

It is by no means improbable that similar beds may be known to exist in other districts; but never having heard of a stratum of compact felspar and mica, *as a member of a coal formation*, you will oblige me by giving publicity to this short notice through the medium of your journal.

I have the honour to be, Sir,

Your most obedient servant,

N. J. WINCH.

## ARTICLE VI.

*Queries on the Plumbago formed in Coal Gas Retorts.*

By the Rev. J. J. Conybeare, MGS.

(To the Editor of the *Annals of Philosophy*.)

MY DEAR SIR,

Bath Easton, Dec. 15, 1822.

THE very general use of coal gas, and the degree of scientific information mostly to be found in those connected with its manufacture, render it probable that for many persons the remarks which I am about to offer will possess but little of novelty. As, however, I am not aware that this subject has yet been noticed in any periodical or other publication, I venture to intrude them on the notice of your readers, rather indeed in the hope of obtaining further information from those who are more competent than myself, than of adding much to the public stock.

The cast-iron retorts used in the Bath gas works, being, on an average, somewhat less than one inch and a quarter in thickness, are *burned through*, as it is technically termed, in the part exposed to the greatest heat, in about 18 months. After this they are of course no longer serviceable. This destruction is effected by the progressive oxidation and scaling off of the metal. The scales consist chiefly of peroxide of iron, and the powder which they afford by grinding and washing might be applied to any of the purposes for which the *crocus martis* is in request. It is observable that the portion of cast-iron which remains unoxidated in the parts exposed to a strong heat is found to present, on being broken, a texture much more highly crystalline than that of the portion further removed from the action of fire.\* Is this simple development, or is it to be ascribed to that mysterious re-arrangement of particles which in some other cases appears to take place at a heat short of actual fusion?

The unserviceable retorts on being withdrawn from their beds are found lined with a coating of plumbago averaging the thickness of four inches. This coating is thickest towards the bottom of the retort, nearer to its mouth it becomes scantier and more intermixed with coaly and fuliginous matter. The general aspect of the predominant variety may be thus described: *Colour*, iron-grey, somewhat lighter than that of native plumbago; *texture*, scaly; *structure*, mammillated, usually in very close aggregation; some specimens exhibit this structure on the larger scale; generally it is discoverable only by the use of the lens; *hardness*, somewhat variable, but always greater than that of the best native plumbago; scratches gypsum, but is scratched by calc-spar; *lustre* of the exterior surface (especially where the mammillated structure is distinct), sometimes very considerable: *lustre* of the fracture usually but small: the *powder* uniformly resembles that of common plumbago, excepting that it is somewhat less brilliant.

In another variety, the structure of the mass is stalactitic, and its texture, where broken, perfectly compact and uniform; even under the lens, this variety closely resembles some of the stalactitic grey ores of manganese, and occasionally exhibits on the surface a considerable degree of iridescence. The mass for the greater part gives evidence of its gradual formation, by its slaty aspect, and tendency to break with greater readiness in the direction of what may be termed its strata. In many parts, it is rify, and the rifts are usually coated with a brilliant deposition of a lighter colour than the general mass. Fragments projected

\* For this observation, as well as for most of the facts contained in this letter, I am indebted to the kindness of Mr. Eastwick, the intelligent and obliging superintendent of the Bath Gas Works.

on nitre deflagrate, but not rapidly, leaving after combustion traces of iron. Some rude experiments gave me reason to think, that the quantity of iron varied in different specimens, and that it scarcely amounted, at the most, to the nine per cent. stated by Berthollet to exist in native graphite. That the substance in question is a true artificial plumbago admits, I apprehend, of no doubt. It must be formed plentifully in many other gas works besides those of Bath, and must, therefore, in all probability have attracted the notice of persons far better able to inquire into its production and properties than myself. In our works it has not yet been converted to any useful purpose. In writing or drawing, its hardness and general want of compactness render it in its crude state altogether unserviceable. When finely powdered and washed, it might be used in composition pencils. For the domestic uses to which plumbago is applied in coating grates, &c. I have found it to answer sufficiently well; but it is objected by the *savantes* in housewifery, that it has not a sufficient *lustre*, and would, therefore, I suppose, be seldom used by them; but where preservation only, and not ornament, was the object,\* I have been informed that it has been applied with success to the purpose of covering razor strops. For that of diminishing friction, and for the manufactory of crucibles, furnaces, &c. it would, I should apprehend, answer sufficiently well, for the latter especially. It appears from a paragraph in the article Coinage (Supplement to Encyclopædia Britannica), that the blue pots used in the mint are all of foreign manufactory, those made in this country containing *too small* an admixture of black lead. Is this to be attributed to the scarceness of that material, or to the want of sufficient tenacity in the English clay with which it is worked up, rendering its addition beyond a certain point impracticable?

But whether this substance prove useful or worthless in an economical view, its occurrence cannot fail to strike the more speculative inquirer as adding one more to the many instances in which the unintentional products of art have been found to resemble those of nature, and as contributing, remotely at least, to throw fresh light upon one of the most controverted points of geological theory. Plumbago is well known to be among the most *infusible* of mineral substances. Now in the present cases, if not the whole of its mass, at least all those portions in which the mammillated structure is discernible, and yet more its stalactitic form, must have been brought to a state of fusion by a heat inferior to that at which cast-iron begins to run. Will this be

\* I know not at what price it might be rendered, but as its production is matter of necessity, and it is at present considered as useless, the charge could scarcely be high. The retail price of the black lead of the shops is 2s. per lb. and it is said to be much adulterated by the admixture of a micaceous hematite obtained near Bovey Tracey (C. Devon), which must be injurious to its preservative qualities.

the better accounted for by the long duration of the heat, or by assuming that this compound, like some others, is more fusible at the moment when its constituents first enter into chemical union. Should it be apprehended that no *actual fusion* whatsoever has taken place, the formation and consolidation of the substance by heat without fusion will still furnish the vulcanist with a new point of analogy.

Believe me, my dear Sir, very truly yours,

J. J. CONYBEARE.

P. S. The character of some portions of this plumbago has struck me as not unlike that ascribed to the points of charcoal altered and fused by voltaic electricity, in some late American experiments.

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## ARTICLE VII.

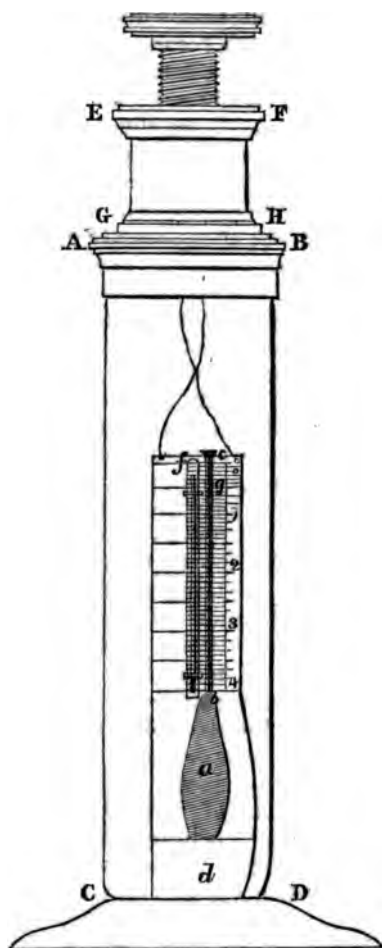
### *On the Compressibility of Water.* By Prof. Oersted.\*

PROF. OERSTED, several years since, laid before the Royal Society of Copenhagen some experiments on the compression of water, and showed at that time that this might be effected by a much smaller power than is generally supposed, provided the instrument was constructed according to the well-known principle, that a pressure acting upon a small surface of an enclosed liquid had the same effect as a power equally great, acting upon each similar part of the whole surface. For the compression of water he made use of a large cylinder of brass upon which one smaller was screwed, furnished with a well-fitted piston. He was, therefore, able to show the compression of water by a small power, quite as well as Abich and Zimmermann had done by many hundred pounds weight. To measure the power, a tube full of air, which was confined by mercury, was used, by which contrivance, of course, the air underwent the same pressure as the water from which it was separated by the mercury. According to the principle, that the compression of the air is in proportion to the pressing power, it was easy to calculate this power. But notwithstanding the great strength of the brass cylinder in which the water was compressed, it was possible that it might have given way, so that not only the compression of the water might have been measured, but a result obtained, in which the flexibility of the instrument

\* Extracted from a memoir read before the Royal Society of Copenhagen, 1822.

was included. In the first experiment of M. Oersted, as well as in all those of former philosophers, Canton only excepted, any change of temperature, which might have happened during the experiment, had not been taken into consideration, which, however, in several respects was necessary, as it might even be supposed that heat was produced by the very compression. The excellent experiments of Canton, which, in later times, have been almost forgotten, were made with the pressure of condensed and rarified air. But every condensation or expansion of air is accompanied with an adequate elevation or diminution of temperature; it was, therefore, to be feared, that this ingenious philosopher had been deceived by this influence. He found the compression of water, at a pressure, equal to that of our atmosphere, to be between  $\frac{1}{1000000}$  and  $\frac{1}{1000000}$  of the bulk of the water. The experiments of Canton possessed a great advantage over all which have recently been made, viz. that the vessel which contains the liquid to be compressed undergoes the same pressure both internally and externally; so that neither its form nor its size can be altered. Within a few years, Mr. Perkins, the ingenious inventor of the siderographia, has made some experiments, which have the same advantage as those of Canton; he included the tube of metal in which the water was to be compressed, in water which was exposed to the same pressure. His ingeniously contrived experiments will always be of considerable importance, because he has made them with a power which a philosopher seldom has at his disposal, viz. a pressure several hundred times exceeding that of our atmosphere. It was not, however, his intention to ascertain by these experiments, whether heat was produced by the compression of water, and what influence it had upon the result. Prof. Oersted endeavoured, therefore, to contrive an instrument which allowed an exact measurement of the compressing power, as well as of the compression of the water itself, and which at the same time made it easy, exactly to ascertain the influence which heat might have on the effect. The water which is to be compressed is included in a glass tube, *a*, which holds about two ounces of water: it is closed below, and its upper part terminates in a capillary tube, *bc*, 52 French lines long, and of even bore, so that the vessel is like a flask, the neck of which is a long capillary tube. On the upper end of this tube is a small funnel two lines wide. The flask holds 709.48 grammes of mercury, but the mercury which fills 24.6 lines of the capillary tube weighs only 96 milligrammes, which gives  $\frac{1}{1000000}$  for the length of a line, or, to be more exact, 0.000005501 of the contents of the flask. When the experiment is to be made, the flask is warmed a little by being kept for a moment in the hand; if possible the temperature must not rise above  $\frac{1}{4}^{\circ}$  centigrade. Then a drop of mercury is introduced into the funnel, which, while the water in the flask is

cooling, will partly sink into the capillary tube *f*, as deep as the mark *g*, and separate the water in the flask from that in the outer vessel. This flask is now placed into a strong cylinder of glass *A B C D*, upon which another smaller cylinder of brass *E F G H* is fixed. A piston connected with a screw *F* is moveable in this upper cylinder. If a pressure by means of this piston be exerted upon the water in the glass cylinder, this will press upon the mercury, and thus upon the water in the flask. As soon as the water in the flask is compressed, the mercury in the capillary tube will sink; and the least pressure produces that effect. To measure the compression, the author fastens the flask in a cylinder of lead *d*, which bears a scale on which one-fourth part of a French line is marked, and a small glass tube, *e f*, filled with air, evenly bored, serves to measure the compressing



power by the compression of the air. All changes of temperature are easily perceived on the narrow neck of the flask much more accurately than on any thermometer; for an increase of temperature amounting to one degree (centigrade) makes the water rise 27 lines, its temperature supposed to be about  $15^{\circ}$ . If the temperature is considerably higher or lower, the changes of course will be either greater or smaller. The scale being divided into one-fourth part of a line, and one-eighth being easily perceived by the eye, it is evident that  $\frac{1}{100}^{\circ}$  cannot escape observation, and that  $\frac{1}{1000}$  is by no means difficult to observe. It is scarcely necessary to add, that the temperature of the liquid is to be ascertained by a thermometer, at the beginning of the experiment. If the experiment be made quickly, and no per-

sons present beside the observer, the difference in the height of the mercury before and after the experiment will generally be one-eighth of a line; frequently, however, one-fourth. In the first case, it announces a change of temperature amounting to not quite  $\frac{1}{100}^{\circ}$ , in the second to hardly  $\frac{1}{100}^{\circ}$ . If the experiment be performed slowly, the difference may be one-half, and even one line. In every case the mean height between these two observations is to be taken. By a long series of experiments, of which the most accurate were made at a temperature of  $15-16^{\circ}$ , a pressure equal to one atmosphere has produced a compression = 0.000047 of the original bulk of the compressed water. Several alterations on the pressure, from one-third to five atmospheres, were tried, and agreed in proving, *that the compression is in direct proportion to the compressing power*. The same result had been deduced by the author from his former experiments, which, however, were influenced by the giving way of the metallic vessel; the expansion of which must likewise be in proportion to the pressing power.

It seems pretty evident that no heat is produced by this condensation of the water, the limit between mercury and water being, after the experiment, on the same place as before; the insignificant elevation of temperature must be considered as a necessary result of the contact of the observer during the experiment. Even after a pressure of five atmospheres, the difference of temperature was not quite  $\frac{1}{100}^{\circ}$ , and in general neither greater nor less than that, if only a pressure of one atmosphere had been used. It was, however, possible, that the expansion of the water when the pressure had ceased, would absorb the heat produced before by the compression; therefore a thermometer of Breguet, on which a difference of  $\frac{1}{10}$ th of a degree may easily be perceived, was placed in the water in the large cylinder, and exposed to the greatest compression which could be procured, but not the least trace of any change in the temperature was observable. The manner in which these experiments of Prof. Oersted agree with those of Canton is really interesting. The English philosopher had at  $64^{\circ}$  Fahr. =  $15\frac{1}{2}^{\circ}$  centigrade, a compression of  $\frac{4.1}{1000000}$  by a pressure equal to one atmosphere, and at  $34^{\circ}$  Fahr. =  $1\frac{1}{10}^{\circ}$  centigrade, it was  $\frac{4.2}{1000000}$ . This rather unexpected result may easily be explained by small differences in the temperature, but it does not, on either side, deviate much from the result Prof. Oersted obtained, which was  $\frac{4.7}{1000000}$ .

## ARTICLE VIII.

*Astronomical Observations, 1822.*

By Col. Beaufoy, FRS.

*Bushey Heath, near Stanmore.*

Latitude 51° 37' 44·3" North. Longitude West in time 1' 20·93".

Nov. 21. Immersion of Jupiter's first satellite.....	{	16 <sup>h</sup> 14' 49"	Mean Time at Bushey.
Nov. 23. Immersion of Jupiter's first satellite.....	{	16 16 10	Mean Time at Greenwich.
Nov. 25. Emersion of Jupiter's first satellite.....	{	10 43 19	Mean Time at Bushey.
Nov. 27. Emersion of Jupiter's second satellite.....	{	10 44 40	Mean Time at Greenwich.
Nov. 29. Emersion of Jupiter's third satellite.*	{	7 21 20	Mean Time at Bushey.
Dec. 7. Emersion of Jupiter's first satellite.....	{	7 22 41	Mean Time at Greenwich.
Dec. 11. Emersion of Jupiter's second satellite.....	{	9 50 15	Mean Time at Bushey.
	{	9 51 36	Mean Time at Greenwich.
Dec. 7. Emersion of Jupiter's first satellite.....	{	16 41 20	Mean Time at Bushey.
Dec. 11. Emersion of Jupiter's second satellite.....	{	16 42 41	Mean Time at Greenwich.
	{	15 04 00	Mean Time at Bushey.
	{	15 05 21	Mean Time at Greenwich.

## ARTICLE IX.

*Analysis of Uranite from Cornwall.*

By Richard Phillips, FRS. L. &amp; E. &amp;c.

THIS mineral has been found in several of the Cornish mines; the crystals are sometimes of a yellow colour, more especially those which occur in Tin Croft copper mine, near Redruth; while in Gunnis Lake copper mine, at the eastern extremity of the county, it is met with of a beautiful deep-green colour.

The primary crystal of this substance is a right square prism, and there is no difference in this respect between the yellow and green crystals; the modifications of this form, with their varieties, have been described by my brother (Mr. W. Phillips), in the third volume of the Geological Transactions.

Both varieties of uranite have been subjected to chemical examination by the late Rev. Mr. Gregor. With respect to the yellow crystals, he states (Phil. Trans. 1805), that beside oxide of uranium, they contain some lime, silica, and oxide of lead; and, he observes, that "the green crystals differ in no respect

\* According to the Nautical Almanac, the immersion of this satellite should have taken place at 17<sup>h</sup> 26' 56"; but although I placed the telescope at 17<sup>h</sup> 09' 29", the eclipse had previously occurred.

from the yellow, except in containing a little of the oxide of copper." This opinion is confirmed by the identity of the crystalline form already noticed; Mr. Gregor states, however, that he had not a sufficient quantity of the crystals to allow of subjecting them to a rigorous examination. Since this period, Mr. Gregor has given an analysis of the green crystals in the fifth volume of the *Annals*, according to which they consist of

Oxide of uranium, with a trace of oxide of lead . .	74.4
Oxide of copper . . . . .	8.2
Water . . . . .	15.4
Loss. . . . .	2.0
	<hr/> 100.0

M. Berzelius, in his *Nouveau Système Mineralogique*, has given an analysis of the uranite of Autun: he says, "I have found that this mineral is a compound of oxide of uranium, with lime and water; in fact, that it is a true salt with a base of lime in which the oxide acts as an acid." He further states, that the oxygen of the uranium is three times, and that of the water six times, the oxygen of the lime; there is, however, he observes, a slight excess of oxide of uranium and of water. The results of this analysis are thus stated:

Lime . . . . .	6.87
Oxide of uranium . . . . .	72.15
Water. . . . .	15.70
Oxide of tin . . . . .	0.75
Silica, magnesia, oxide of manganese. . . . .	0.80
Matrix . . . . .	2.50
	<hr/> 98.77

"The same mineral," continues Berzelius, "is met with in Cornwall, but it is of a very fine deep-green colour; this colour is owing to the presence of a certain quantity of arseniate of copper, which is an accidental admixture. When this uranite is treated with soda by the blowpipe, it gives white metallic globules, composed of arseniuret of copper."

With the intention of procuring some peroxide of uranium, I dissolved a quantity of the green uranite in nitric acid; the oxide of uranium was precipitated by ammonia, and the oxide of copper dissolved by excess of it. In order to free the oxide of uranium from any arsenic acid which it might contain, I boiled the precipitate in a solution of potash; on adding a portion of the filtered solution to nitrate of silver, an abundant yellow precipitate was formed, which had the appearance of arsenite of silver; but as arsenious acid does not, I believe, exist in nature combined with any metallic oxide, and as also the mineral in

question had been dissolved in nitric acid, it was evident that the yellow precipitate could not be arsenite of silver.

As phosphoric acid is well known to afford a yellow precipitate with oxide of silver, I suspected the presence of this acid, and I found that the alkaline solution gave a blue precipitate with sulphate of copper; and when it was saturated with an acid, mixed with ammonia and muriate of magnesia, the well-known minute crystals of phosphate of ammonia and magnesia were formed.

Under these circumstances, it appeared to me worth while to subject the uranite to another analysis, and I proceeded as follows:

One hundred grains of the green uranite of Cornwall were dissolved in dilute nitric acid; half a grain of silica remained undissolved. In attempting to saturate the solution previously to adding nitrate of lead for the separation of the phosphoric acid, I found that precipitation occurred long before sufficient potash had been added. As this free nitric acid would dissolve the phosphate of lead formed, I decomposed the nitric solution by boiling it with excess of potash, and then having added excess of acetic acid to the alkaline solution, nitrate of lead gave an abundant precipitate of phosphate. I prefer acetic acid for this purpose to nitric, because as acetic acid has scarcely any action upon phosphate of lead, it is not requisite to take the trouble of avoiding slight supersaturation.

The phosphate of lead thus obtained, after washing and drying, weighed 80 grains, equivalent, according to Dr. Thomson, to 16 grains of phosphoric acid; the oxides of uranium and copper were redissolved in nitric acid, and the solution being added to ammonia, the oxide of copper was dissolved, and that of uranium precipitated; the latter, after washing and drying, weighed 60 grains, and the oxide of copper, after ebullition with potash, weighed 9 grains. The ammoniacal solution contained no lime. It appears, therefore, that 100 grains of this substance contain

Silica .....	0.5
Phosphoric acid .....	16.0
Oxide of uranium .....	60.0
Oxide of copper .....	9.0
Water .....	14.5
	<hr/>
	100.0

I attempted to determine the quantity of water by direct experiment; for this purpose 50 grains of the mineral were heated on a platina crucible by a spirit lamp, 8.5 grains were lost = 17 per cent. This experiment was repeated with a precisely similar result, and no further loss was occasioned by exposing the mineral to a strong red heat. If, however, we add 17 to the acid and oxides, there will be an excess of 2.5 over

the 100 grains submitted to analysis. Suspecting, therefore, that some phosphoric acid had been volatilized with the water, I held litmus paper over a further portion of the mineral while subjected to the heat of the lamp; I found that it was reddened during the expulsion of the last portions of the water, and turmeric paper, which had been reddened by ammonia, had its yellow colour restored by the phosphoric acid thus volatilized.

M. Berzelius, it has been already stated, attributes the green colour of this mineral to the presence of arseniate of copper. In order to examine this point, Mr. Children was good enough to submit some crystals to the blowpipe while I was present. When crystals without admixture were heated upon charcoal, not the slightest arsenical smell could be perceived by either of us; but when the ore was pulverised and mixed with bicarbonate of soda, and strongly heated in the reducing flame on charcoal, a slightly arsenical smell was discoverable, but no fumes were visible. As, however, this ore contains so large a portion as nine per cent. of oxide of copper, the arsenic acid, combined with it, would be detected with the greatest facility.

Still further to examine whether the oxide of copper is in combination with arsenic acid, I supersaturated some of a nitric solution of the mineral with ammonia. By this the arseniate of copper would be dissolved, and the phosphate of uranium precipitated without decomposition. I boiled some of the ammoniacal solution with potash, and added nitrate of silver to the filtered solution; a yellow precipitate was obtained having a scarcely perceptible tinge of red, instead of being of a deep-red colour, as would have occurred if arsenic acid had been present in sufficient quantity to form arseniate with the oxide of copper. It is, therefore, evident, that the oxide of copper is in the state of phosphate, a circumstance which is likely to occur, when it is known that the mine which produces the uranite also yields phosphate of copper.

It is difficult to determine in what state of oxidation the uranium exists in the mineral; but as it is acted upon during analysis by nitric acid, and as I found by direct experiment that when dissolved in nitric acid, and heated to redness, it gained no weight, I think we may conclude, that it is procured in the state of peroxide, and I shall consider it as existing in this state in the ore.

According to Dr. Thomson, hydrogen = 1; the atom of peroxide of uranium is represented by 137, and that of phosphoric acid by 28; 60 will, therefore, combine with 12.2 of phosphoric acid, forming 72.2 of phosphate of uranium, and there remain 3.8 of phosphoric acid to combine with 9 of peroxide of copper; but as phosphate of copper is composed of 80 oxide and 28 acid, or 1 atom of each, 9 of oxide will combine with only 3.1 of acid, leaving an excess of 0.7.

Assuming the phosphate of uranium to be combined with ~~one~~ of water, and the phosphate of copper, as deter-

mined by Mr. Lunn,\* with two atoms, the mineral will appear to consist of

Silica .....	0.5
Phosphoric acid. ....	15.3
Oxide of uranium .....	60.0
Oxide of copper .....	9.0
Water .....	13.8
	<hr/>
	98.6
Loss .....	1.4
	<hr/>
	100.0

Neglecting the silica, we may consider the ore as essentially composed of nearly

Phosphate of uranium .....	73.2
Phosphate of copper .....	12.3
Water .....	14.5
	<hr/>
	100.0

On comparing the results of this analysis with those obtained by Mr. Gregor, it will be observed, that the quantity of phosphate of uranium in 100 parts of the mineral differs only 2.2 from the weight of what he considered to be pure oxide of uranium. It is indeed evident that in his mode of operating, he precipitated, dried, and weighed the acid and oxide in combination.

M. Berzelius seems to admit that the only material difference between the uranite which he analyzed and the green variety, arises from the accidental admixture of the latter with what he supposed to be arseniate of copper; when also it is observed that what he estimates as oxide of uranium, agrees in quantity with the phosphate in the green variety as nearly as 72.15 to 72.2, little doubt can be entertained that the mineral from Autun, as well as that from Cornwall, is essentially composed of phosphate of uranium.

\* *Annals*, vol. iii. New Series, p. 179.

## ARTICLE X.

*Proceedings of Philosophical Societies.*

## ROYAL SOCIETY.

On Saturday, Nov. 30; being St. Andrew's Day, the Royal Society held their anniversary meeting at Somerset House. A great number of members were in attendance at 12 o'clock, when the chair was taken by the President, Sir H. Davy, Bart.

In the course of business, Sir Humphry announced the names of the Fellows lost to the Society by death since the last anniversary; among whom were Sir H. C. Englefield, Bart.; Sir W. Herschel, Dr. Marcet, Rev. Mr. Vince, Plumian Professor of Astronomy at Cambridge, Dr. C. H. Parry, Sir C. Pegge: and among foreign members, M. Delambre, the Abbé Haüy, and Count Berthollet. He gave a new interest to this ceremony, by entering into a brief but elegant and discriminating tribute to the scientific merits of these distinguished individuals. He prefaced his eulogies by observing, that the occasion was a particular one—that the Society had never before lost in one year so many distinguished fellows—that the respect paid to the memory of the illustrious dead might, he hoped, awaken a feeling of emulation among the living; and that although he was unable to do justice to their respective merits, yet he trusted that in all he should have to say, the judgment and the feelings of the Society would be in unison with his own.

Sir H. C. Englefield, said he, was an accomplished gentleman, gifted with a great variety of information, and possessing considerable talents for physical research. His knowledge of astronomy was evinced by his early work on Comets: he was a clear writer—a learned antiquarian—eminently distinguished for conversational powers—a truly honest man—and an ornament to that class of society in which he moved.

Of Sir William Herschel it was observed by Sir Humphry, that the progress of modern astronomy was so connected with his labours, that his name would live as long as that science should exist: his happy and indefatigable spirit of observation was spoken of as proved by his discovery of a new planetary system; and of a number of satellites before unknown—his inductive powers of reasoning, and bold imagination, as shown in his views of the stellar systems in the heavens—and his talents for philosophical experiments, as proved by the discovery of the invisible rays in the solar spectrum. He was a man, continued the eulogist, who, though raised by his own efforts—by the power of his own intellect, to so high a degree of eminence, was spoiled neither by glory nor by fortune; and who

retained, under all circumstances, the native simplicity of his mind. His amiable character and the felicity of his life were dwelt on at some length. He died full of years and of honours; and, when unable to labour himself, saw a kindred disposition and kindred talents displayed by his son. In conclusion,—his mental powers seemed to have acquired such a degree of expansion here, as to have become incapable of further elevation or extension but in a higher stage of existence.

Sir Humphry then expressed his regret at the premature death of Dr. Marcet, whom he characterized as an ingenious and accurate chemist—a learned physician—a liberal, enlightened, and most amiable man.

Appropriate characters were likewise given of Mr. Vince, Dr. Parry, and Sir Christopher Pegge.

In eulogising the foreign members, the name of Haüy was spoken of as one that would always be remembered in the history of mineralogy, in consequence of his having established what may be considered as a mathematical character in discriminating mineral species. Delambre, the learned Secretary of the Royal Academy of Sciences of Paris, was mentioned with great praise as an excellent astronomer, and candid and liberal historian of his own science; and an able observer, whose name will be for ever associated with the first very accurate measurement of an arc of the meridian in France. M. Berthollet was called by Sir Humphry the patriarch of modern chemistry. He dwelt on his discoveries and labours at some length; and paid a just tribute to the candour and liberality of his mind—to his warm and zealous patronage of rising genius, and to his quiet and amiable social virtues.

The President next read the list of members admitted into the Society since the last anniversary, among whom were Mr. Dalton, of Manchester; Dr. Kidd, of Oxford; Mr. James Thomson; and Mr. Rennie.

He then proceeded to state the decision of the Council respecting the award of the medal on Sir Godfrey Copley's donation, which, he announced, had been adjudged this year to the Rev. W. Buckland, Professor of Mineralogy and Geology in the University of Oxford, for his paper, on the Fossil Bones and Teeth discovered in a Cave near Kirkdale, in Yorkshire, printed in the Society's Transactions.

Prior to delivering the medal to Prof. Buckland, as the present was the first time that a paper on a subject of pure geological research had been honoured by this mark of distinction, Sir H. Davy gave, in an eloquent discourse, a concise view of the history and importance of geological pursuits in general, as well as of the interest and value of Mr. Buckland's recent labours in particular. Of this discourse, we are happy to present a condensed epitome.

While the phenomena of the distant stars and other objects

of astronomical science had long been subjects of investigation, in consequence of their relations to the seasons and to time, the structure of the earth had been scarcely noticed until a late period. Cosmogonies, "or dreams of the formation of the world," had been brought forward at various periods, and some general views in geology had been advanced by Hooke, Lister, Strachey, and other early members of the Royal Society, or contributors to its Transactions; but the commencement of geology as an exact science did not take place until about 50 years since, when a regular classification of minerals having been effected, Pallas, De Saussure, and above all, Werner, further arranging this, the alphabet of geology, proceeded to read with it that part of the book of nature; and the logic of the science was subsequently furnished by chemistry and comparative anatomy. The space of a discourse would not admit even of naming the labourers, our contemporaries, by whose zeal and accurate spirit of observation, the field of geological research had been so successfully cultivated within the last 20 years; but among them that of Prof. Buckland was highly distinguished by his indefatigable ardour for inquiry, and by his caution and sagacity in drawing conclusions.

For the purpose of illustrating the subjects of Mr. Buckland's paper, Sir Humphry now gave a general view of the constitution of the superficial part of the globe, of the arrangement of its constituent rocks, and the distribution in them of organic remains: in this he mentioned that he had himself ascertained that those remains of the animal kingdom, the position of which was geologically the lowest, contained the least quantity of the original matter of the animals. Although it had been strongly suspected that the large animals of the elephant, rhinoceros, hippopotamus; tiger, hyæna, and other kinds, the remains of which are met with in the diluvian strata, were once inhabitants of the countries where their bones and teeth are thus found, yet this had never been distinctly proved until Prof. Buckland described the Yorkshire cave, in which several generations of hyænas must have lived and died. By the industry and happy talent for observation of that gentleman, there had been distinctly established an epoch from which to trace the revolutions of the globe. Sir H. had himself since visited the cave, in company with Mr. B. and could testify to the accuracy of his description;—announced that the Professor had recently examined a number of similar caverns in Germany, and that by the phenomena of these, he had found his former conclusions confirmed. Sir H. then expressed his hopes that Mr. Buckland would communicate the results of his late researches to the Royal Society. Two theoretical views might be taken respecting the animal remains in question; one, that the animals had been of peculiar species fitted to inhabit temperate or cold climates; thus, that the elephant and rhinoceros of Britain might have

been as different from those of tropical regions as our common cattle are from the musk ox of Siberia : in the other hypothesis, the globe was considered to have undergone a change of temperature. Sir Humphry was of opinion that the latter supposition is the most probable one : if the former were to be adopted, it would be difficult to conceive how the elephants could obtain sufficient food in polar climates, or how the hippopotami could have inhabited their frozen lakes. He then entered into some general views on this interesting subject, and its connexion with that of the chaotic state of the globe, and with those of the successive creations of living beings, and the early revolutions of our planet, until it became at last fitted for the habitation of man.

In presenting the medal to Prof. Buckland, the President desired him to receive it as a tribute of respect from a body, which he believed to be very impartial in its decisions, and which considered the actual advances that had been made in science, rather than the nation, school, or individual, by whom they had been effected. Sir Humphry expressed his hopes that Mr. B. would enjoy health to continue his researches, and that his example would stimulate other members of the Society to similar inquiries and labours ; for that geology was abundant in objects of research, and was most worthy of being pursued, on account of its connexion with the useful arts, of the happy views which it affords of the order of nature, and of the assistance which it lends to true religion.

Sir H. considered that the scriptural account of the deluge was now completely established from geological grounds ; but the science of geology, he maintained, should be studied in a manner altogether independent of the authority of the Sacred Scriptures ; for that these, as Bacon had said long before, merely gave some remarkable facts in the history of the globe, and not systems of philosophy ;—the latter were left to be framed by the industry of man, and by the exercise of his god-like faculty of reason, which, in its highest sphere, approximates to revelation itself. The discourse was concluded by some appropriate moral reflections arising out of the subjects discussed.

The Society then proceeded to the election of their officers and council for the year ensuing, when the following gentlemen were chosen.

*President.*—Sir Humphry Davy, Bart.

*Treasurer.*—Davies Gilbert, Esq. MP.

*Secretaries.*—William Thomas Brande, Esq. and Taylor Combe, Esq.

*Of the Old Council.*—Sir H. Davy ; W. T. Brande, Esq. ; Samuel Goodenough, Lord Bishop of Carlisle ; Taylor Combe, Esq. ; Davies Gilbert, Esq. ; Charles Hatchett, Esq. ; J. F. W. Herschel, Esq. ; Sir Everard Home, Bart. ; John Pond, Esq. Astronomer Royal ; William Hyde Wollaston, MD. ; Thomas Young, MD.

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*Of the New Council.*—Charles Babbage, Esq.; Sir Gilbert Blane, Bart.; Charles, Lord Colchester; J. W. Croker, Esq.; John, Earl of Darnley; Sir Henry Halford, Bart.; Charles Hutton, LL.D.; Capt. Henry Kater; W. H. Pepys, Esq.; Joseph Sabine, Esq.

The weekly sittings of the Society had been resumed on the 7th of November, and had taken place twice afterwards prior to the anniversary meeting. On the 14th, a paper by the Astronomer Royal was concluded, entitled, "An Appendix to a former Paper, on the Changes which appear to have taken Place in the Declination of some of the principal fixed Stars." Mr. Pond stated in this paper that he had obtained results confirming those given in his last communication.

A paper, by the same gentleman, was also read, "On the Parallax of  $\alpha$  Lyrae." In this, the absolute parallax of the star in question was stated to be a very small fraction of a second.

Nov. 21.—The Croonian lecture was commenced, entitled, "Microscopical Observations on the Suspensions of muscular Motion in the *Vibrio Tritici*," by Francis Bauer, Esq. FRS.

Dec. 5.—The Croonian lecture was concluded. The curious animal described in this paper is the cause of a particular disease in wheat, at first thought to be peculiar to Kent, and was discovered by the author in 1807 while engaged in the investigation of the various diseases to which that species of corn is subject. In the ears diseased by it, some grains were quite ripe and dry, while others, quite green, were unimpregnated germs. They contained cavities lined with a fibrous substance, every fibre of which was a very minute worm. These worms which, when moistened, were in active motion, after being dry, and apparently dead for five days, when again moistened, were in active motion as before. Mr. Bauer conceived that their spawn or eggs must have been introduced into the germs by the sap, as he had ascertained was the case with the minute fungi which produce in wheat the disease called *smut*. He sowed some infected grains, and some with the smutty infection in addition; and in the ears subsequently produced from them found both diseases co-existing; the eggs in these ears, he found, were of the third generation.

The worms are annulose; have a proboscis of four or five joints, which they extend like an opera-glass; and have a clawed tail. They are rather opaque on the back, but more transparent on the belly, through which are seen in them whole strings of eggs. Near the tail is a circular aperture with a fleshy rim, through which their eggs are exuded in strings of six or seven at a time; as each egg comes forth, the tail is elevated with a quick motion. The eggs (or rather envelopes of the foetal worm) are in some degree transparent, and appear to be truncated at first; but they soon emit water, and assume an oval figure, a little narrowed in the middle; they are about

1-300th of an inch in length, by between 1-800th and 1-900th in width; in one hour and a half after exudation, the young worm peeps out, and by twisting about, and other active exertion, wholly extricates itself in about an hour more. The worms just extricated from the eggs look like fine glass tubes full of water, with air-bubbles corresponding to the annuli of the older ones, but no indentations. The large egg-bearing worms, which were one-fourth of an inch long by 1-80th wide, have no other motions than those of moving their heads and tails; they present no external marks of sex, and are considered to be hermaphrodites. In the grains which also contained smut, there were two or three large worms surrounded with eggs.

Some of these worms were revived in wheat which had been dry for five years and eight months; the longest period of reviviscence observed, was six years and one month; the large worms do not revive. If kept in water longer than 35 days, they all die, and become as straight as needles, but remain a very long time without decaying. If merely kept moist in a watch-glass, they may be kept alive for three months; if too much water be applied to them, they cannot be so readily revived as when a little is used, which soon evaporates; and the longer their periods of desiccation have been, the longer they require to be kept moist for reviviscence. They appear to be preserved by a mucus in which they are imbedded, and which appears to be of an oily nature. This, however, remains for years when the worms can no longer be revived.

Mr. Bauer had completed his investigation of the *vibrio tritici* in 1810, and conceived that it had not been noticed before, when he met with some references to former descriptions of it; these, however, being very vague, and his time having been since occupied with researches of a different nature, he had no opportunity of consulting the authors referred to until lately: upon so doing, he found no occasion to alter any of his conclusions on the subject.

Dec. 12.—On Metallic Titanium, by W. H. Wollaston, MD. VPRS.

This paper commenced with a review of the results hitherto obtained in experiments on the reduction of this metal, of which even the most satisfactory are of a doubtful nature; hence Dr. Wollaston conceived that an account of some metallic crystals of titanium would be acceptable to the Royal Society. In the slag of the iron works of Merthyr Tydvil, there are minute cubes, which, from being imbedded in and possessing the colour and lustre of iron pyrites, have been considered to be that substance. They are not, however, like the striated cubes of pyrites, which often pass into the pentagonal dodecahedron; but have indented squares on their faces, like the cubes of com-

mon salt. An angle of one of them scratched not only a plate of steel, and ground-glass, but even plates of polished agate and rock-crystal. They were not affected by the nitric, muriatic, nitro-muriatic, or sulphuric acids; and were absolutely infusible before the blowpipe, by which their surfaces were slightly oxidated only, their brightness being restored by borax; that salt, however, did not act in any degree upon the metal, either alone, or with the addition of carbonate of soda. By nitre, they are oxidated, and they become purple or blue according to the degree of oxidation, or to the depth to which it penetrates. In this process, heat is evolved, but no proper detonation takes place. By nitre and borax in conjunction, they may be completely dissolved before the blowpipe, the former oxidating the metal, and the latter dissolving the oxide; but as these salts do not fuse together, the process is expedited by the addition of carbonate of soda. The oxide thus obtained is soluble in muriatic acid, from which it is precipitated white by alkalies and their carbonates; the solution yields by evaporation a soluble muriate. When this is dissolved, it yields with tincture of galls the well-known gallate of titanium, exactly like that precipitated from a solution of the oxide prepared from anatase. Triple prussiate of potash produces a precipitate so nearly resembling the gallate in colour, that in a number of trials, no constant difference between them could be detected. The crystals are titanium absolutely pure: though found in the vicinity of metallic iron, they contain none of that metal, nor do they contain silica, for which the oxide of titanium has a strong affinity. It was impossible to ascertain the specific gravity of crystals not more than 1-40th of an inch square; but Dr. Wollaston endeavoured to ascertain whether or not they would float in melted tin, and attempted to tin their surfaces for that purpose; but they refused to unite with that metal, nor would they combine with iron, silver, or copper. Their lustre nearly proves their metallic nature; but this is completely demonstrated by their perfectly conducting very feeble electricity. If a small slip of zinc, in contact with one of copper, be immersed in dilute sulphuric acid, bubbles of gas will rise from both metals; but if a nonconducting slip of paper be interposed, the bubbles cease to be given off by the copper. Now a small hole being made in the paper, and a cube of the titanium inserted, the communication was made perfect, and bubbles of gas were given off from both metals. This interesting paper was concluded by the remark, that the infusibility of these cubes of titanium evinced, that they were not formed from a state of metallic fusion, but by successive increments from the reduction of the oxide in the slag; in which manner we must likewise explain the formation in nature of many metallic crystals.

On the Difference of Structure between the Human Mem-

brana Tympani, and that of the Elephant; by Sir E. Home, Bart. VPRS.

In 1799, Sir Everard discovered the muscular structure of the membrana tympani in the elephant, and this discovery had led to that of the same membrane in the human ear being also muscular. He had likewise inferred that its oval shape in the elephant was the reason why the ears of that animal were not affected by musical sounds equally with those of man, in which the membrana tympani is circular; the muscular radii being of equal length in the latter case, and unequal in the former one. Since that period, he had laid all his friends in India under contribution for the head of a young elephant preserved in spirits; but remained unsuccessful until lately, when he received from Sir T. S. Raffles, to whom the natural history of the east is so much indebted, the head of an elephant only three weeks old. In this, the membrana tympani was in its proper place, and measured an inch and a half by an inch and one-eighth; the radii were attached to the point and to the two sides of the handle of the malleus, which was situated in a transverse direction. Having been informed by Mr. Corse that the elephant can hear very low sounds with great distinctness, the author attributes this faculty to the long radii of the membrane. Mr. Broadwood having sent one of his tuners with a piano to the elephant at Exeter Change, it was found that the animal scarcely attended to the high sounds, but listened to the low ones with apparent satisfaction. Some observations on the membrana tympani of various other quadrupeds were likewise given in this paper. It was found that the lion in the above menagerie was little affected with high musical sounds, but became quite infuriated by low ones; uttering loud yells, which ceased with the music.

#### LINNEAN SOCIETY.

The meetings of this Society were resumed on Nov. 5, when the reading of a paper was commenced, entitled, "Remarks on the Identity of certain general Laws which have been lately observed to regulate the natural Distribution of Insects and Fungi:" by William Sharpe Mac Leay, Esq.

Of this extended and profound paper, which was not concluded until the third meeting, on Dec. 3, we can give but a brief notice. It related to what may be termed the metaphysics of natural history. In the *Horæ Entomologicæ*, a work not long since published by Mr. Mac Leay, and with which our zoological readers are doubtless acquainted, he has advanced views respecting the natural series of animated beings, which, though founded on a close attention to their entire structure, may have appeared extraordinary as well as novel: they relate to a particular quinary distribution which he has observed of the subjects of the animal kingdom; and in this paper a very remarkable con-

firmation of them is announced and explained. M. Fries, in his *Systema Mycologicum*, published last year, observes laws of the same kind to obtain in the natural arrangement of fungi, which Mr. Mac Leay had pointed out as existing in the animal kingdom, and as probably extending to all organized beings. In the course of the paper, the difference between *affinity* and *analogy* was philosophically examined and defined. Some observations were also made on the law of continuity as far as it regards organized nature; in these was shown the distinction between *saltus* and *hiatus*, which have usually been confounded even by the first metaphysical writers. It was remarked that no *saltus* ever existed in nature, and that where there were *hiatus*, they had arisen from the extinction of species. Towards the conclusion, it was stated, that M. Decandolle had likewise observed the general laws of natural arrangement which formed the subjects of the paper; so that these laws had thus been presented to three individuals, in different countries, nearly at the same period of time.

Dec. 3.—A Description of some Insects which appear to exemplify Mr. W. S. Mac Leay's Doctrine of Affinity and Analogy; by the Rev. W. Kirby, FRS. and FLS. was commenced.

Dec. 17.—Mr. Kirby's paper was concluded, and another by the same entomologist, entitled, "Some Account of a new Species of *Eulophus* (Geoffroi)," having also been read,

The Society adjourned to Jan. 21, 1823.

#### GEOLOGICAL SOCIETY.

Nov. 1.—A letter was read from W. C. Trevelyan, Esq. MGS. addressed to Prof. Buckland, VPGS. on the Geology of the Ferroe Islands, dated Copenhagen, Dec. 3, 1821.

Mr. Trevelyan observes, that the Ferroe Islands appear to be of the same formation as that of Antrim and of the Western Islands; they are literally mountains surrounded by water, and are composed of numerous alternations of almost all the varieties of trap, which are so regular, that the hills appear as if they were divided into a number of terraces: most of the beds are divided from each other by others (generally thin), of a red colour (sometimes green), perhaps the clay ironstone of Werner. These beds Mr. Trevelyan thinks have not been remarked in other formations; neither a curious *lava-like* appearance, which is very frequent, and the alternations extend to the height of about 3000 feet, and in some parts, perpendicular sections may be seen of more than 2000 feet high. The rocks on decomposition are seen to be stratified; the strata display surfaces generally even, but sometimes waved. Excepting in the island of Myggeneas, the dip appears to be the same throughout. In that island are some thin veins of coal. In Suderoe, a coal similar to the Scotch, but superior, occurs in abundance; the best sections of this are near Frodboe, where it is seen between

two thick beds of a hard greyish clay, which crumbles on exposure to the air, and which contains sometimes numerous nodules of a rich ironstone (carbonate of iron). The coal in Suderoe is seen rising from the level of the sea through an extensive tract of country (which the author describes), until it reaches an elevation of above 1000 feet. Basaltic veins of various descriptions are very frequent; sometimes they *alter*, but seldom *disturb*, the strata they pass through; they are frequently accompanied by thick veins of zeolite, in a state exactly resembling the steatite of Cornwall. The coal, however, at Toidnences is disturbed by insinuated masses of basalt. Slips are not frequent in Ferroe, but a remarkable one is described in Soinoe, cavities of great length, perfectly circular, extending in a direct line, and incrustated with zeolite, are common; one was seen, of which the one side was filled with compact zeolite; the other with clay ironstone. Conglomerates are common; one only was observed of *rolled* pebbles, apparently of the rocks of the island. In two of the islands, a bed of greenstone, in some parts columnar, is seen of great extent: it is 100 feet in thickness.

Native copper is very frequent, but not abundant, in most of the rocks. At Famarasund, in Suderoe, it occurs in clay ironstone, in thin plates; in the other rocks, it is generally crystallized, frequently inclosed in zeolite. Specimens of most of the zeolite family were procured, and a variety of tabular calc spar, first found in Ferroe by Sir G. Mackenzie, and unknown elsewhere.

On his return, Mr. Trevelyan, in passing from Fludstrand, in Jutland, to Copenhagen, crossed a considerable extent of chalk; the whole of the country is covered with large rolled blocks of various primitive rocks.

Near Fludstrand are some beds of sand, clay, and marl, apparently above the chalk.

The letter concludes with observations on the aurora borealis, and with reasons tending to confirm the opinion that Ferroe is the Thule of the ancients.

#### ROYAL GEOLOGICAL SOCIETY OF CORNWALL.

*Ninth Annual Report of the Council.*—The progress of the institution since the last anniversary, if not equal to the wishes of its friends, is at least encouraging; though the Society had to regret the loss of their late able Secretary, Dr. Forbes, whose professional pursuits occasioned his removing from this county; and the Council considered it due to the zeal and ability displayed by that gentleman, to elect him an Honorary Member.

The cabinets have been enriched by many valuable donations; and in particular by a splendid series of minerals from Mount Vesuvius, for which the Society is indebted to the kindness of their illustrious townsman, Sir Humphry Davy. The value of this accession to the Museum is not a little enhanced by the

circumstance of the package having been missing for several years owing to the disturbances which occurred at Naples, so that it was given up as lost, when it happily reached its original destination.

The collection of specimens in most of the departments of mineralogy is now become interesting, and being open to public inspection, creates a growing attention to the subject, and has led to the discovery of minerals hitherto unknown in this county, and is thus accomplishing one important object of the institution. The series of our native metallic minerals has also been augmented both by purchase and donation: but while the Council gladly report its continued progress, they beg to remind the members that this department of the cabinet is at once the most interesting and hitherto most defective; and recommend it earnestly to the attention of its friends, particularly to those whose connection with the mines afford opportunities of procuring rare specimens.

The library and funds have also been increased, the latter by a liberal donation from John Hawkins, Esq. to whose interesting communications and encouragement, the institution has been from its commencement so greatly indebted.

Flattering invitations to correspondence have been received in the course of the past year from foreign institutions and men of science, which shows that this Society has already attracted notice, and is contemplated with interest both at home and abroad.

The Council have also the pleasure to congratulate the Society on the publication of the second volume of its Transactions, as ordered by the last general meeting, and they flatter themselves that it will not be found unworthy of its predecessor. It was judged proper to confine its contents entirely to papers relative to the geology and mineralogy of Cornwall; although it occasioned the regret of thereby omitting many valuable communications. A large space will be found devoted to the detail of numerous facts and experiments on the much controverted subject, the Temperature of Mines, and also on the Phenomena of Veins, which cannot fail to attract much attention and further investigation. To the authors and editors, the Society will feel much obligation, particularly to Mr. Carne, not only for so large a portion of the work itself, but also for his indefatigable attention to the arrangement of the papers and superintendence of the press. There are likewise two other circumstances deserving of notice;—that the volume was printed in this town (Penzance), and that the plates were engraved by a self-taught native artist; and the execution of both reflects no small credit on the parties.

Since the last anniversary, a course of lectures on the elements of chemistry has been delivered by the Secretary, Dr. Boase, to a numerous and attentive auditory, and received with much applause and merited approbation. And the Council have also

the pleasure to report, that there is evidently a growing attention to scientific pursuits, which encourages them to cherish and repeat the expression of their anxious hope that the period is not distant, when the great and leading object of this Society will be realised, and Cornwall at once distinguished and enriched by the establishment of a School of Mines.

By order,

HENRY S. BOASE, *Sec.*

The following papers have been read since the last report :

On the Tin Ore of Botallack and Levant. By Henry S. Boase, MD. Secretary of the Society.

A further Account of the Mineralogy and Geology of St. Just. By Joseph Carne, Esq. FRS. MRSA. Member of the Society.

On the Serpentine District of Cornwall. By the Rev. Canon Rogers, Member of the Society.

On the Neptunian Theory of the Formation of Veins. By Henry Boase, Esq. Treasurer of the Society.

On the Noxious Gases of Mines. By Dr. Boase.

On Submarine Mines. By Joseph Carne, Esq. FRS. &c.

On the Temperature of the Cornish Mines. By M. P. Moyle, Esq. Member of the Society.

A proposed new Method of Drawing Mining Maps and Sections. Communicated by Mr. Fox.

On the Temperature of Mines. By Robert W. Fox, Esq. Member of the Society.

On the Utility of a School of Mines. By Dr. Boase.

An Account of the Quantity of Tin produced in Cornwall in the Year ending with the Midsummer Quarter, 1822. By Joseph Carne, Esq. FRS. &c.

An Account of the Produce of the Copper Mines of Cornwall, in Ore, Copper, and Money, in the Year ending the 30th June, 1822. By Mr. Alfred Jenkyns.

#### *Officers and Council for the present Year.*

*President.*—Davies Gilbert, Esq. MP. VPRS. &c. &c.

*Vice-Presidents.*—William Rashleigh, Esq. Charles Lemon, Esq. John Scobell, Esq. John Paynter, Esq.

*Secretary.*—Henry S. Boase, MD.

*Treasurer.*—Henry Boase, Esq.

*Librarian.*—T. Barham, MD.

*Curator.*—Edward C. Giddy, Esq.

*Assistant-Secretary.*—R. Moyle, Jun. Esq.

*The Council.*—T. Bolitho, Esq.; Joseph Carne, Esq.; Stephen Davy, Esq.; Alfred Fox, Esq.; G. D. John, Esq.; Rev. C. V. Le Grice; M. P. Moyle, Esq.; Rev. Canon Rogers; H. P. Tremenhore, Esq.; John Williams, Jun. Esq.

## ARTICLE XI.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS  
CONNECTED WITH SCIENCE.I. *On the Temperature produced by the Condensation of Vapour.*

Mr. Faraday, in some observations published in the *Annales de Chimie*, xx. 329, has illustrated a curious property of vapour which, as he remarks, though it might have been deduced from known facts, had never been cited or confirmed by experiment. It seems, indeed, that the property had been known in Paris to some of the chemists, but that it had never been published.

Hold a thermometer horizontally so that its bulb may be introduced into a current of vapour as it issues from a boiler or tea-kettle: it will soon indicate  $212^{\circ}$ ; then drop on to it a little powdered nitre, and immediately the temperature will rise to  $230^{\circ}$  or higher. This effect is due to the condensing power excited by the salt on the vapour, which, reducing the latter to the fluid form, liberates the heat that raises the thermometer.

In making the experiment, care should be taken that the water condensed on the stem of the instrument does not run down and dilute the salt, for then the temperature falls. Another method of operating is to tie up the salt or substance, round the bulb of the thermometer in a bit of lint or flannel, and introduce it so covered into the steam. The following are temperatures obtained by the use of different substances; the first column of numbers as given by the last process, and the second by the other:

Sulphate of magnesia. ....	214	....	218
*Tartrate of potash. ....	230	....	236
*Tartaric acid. ....	221	....	226
Sugar. ....	223	....	216
*Muriate of ammonia. ....	227	....	230
*Citric acid. ....	228	....	230
*Nitre. ....	230	....	232
Nitrate of magnesia. ....	236	....	236
Nitrate of ammonia. ....	240	....	236
Acetate of potash. ....	258	....	244
Subcarbonate of potash. ....	262	....	258
Potash. ....	—	....	300 and upwards.

Those marked \* are convenient for the experiment with the naked bulb.

The effect, as might be expected, continues at different pressures, and the same difference of temperature which exists between a clean thermometer and one coated with a salt when placed in steam at  $212^{\circ}$  under atmospheric pressure, holds when the pressure and temperature are raised.

Mr. Faraday had stated that at the same pressure a boiling aqueous saline solution gave off steam of the same temperature as boiling water. M. Gay-Lussac makes some remarks on this statement, and shows, that

with the saline solution, the temperature of the vapour is the same as that of the solution itself. We understand that Mr. F. has verified M. Gay-Lussac's statement by experiment, and satisfied himself of its accuracy.

## II. *Index to Kirby and Spence's Entomology.*

(To the Editor of the *Annals of Philosophy*.)

DEAR SIR,

Dec. 22, 1822.

I beg to submit, through your journal, to the learned authors of the Introduction to Entomology how important an addition to each of their delightful volumes a pretty copious index would be; and how imperfect their work is without it. It is of course intended that the last volume should comprise all that is necessary in this way; but this is not enough, for during the many years that intervene between the appearance of the first and the last volume, the entomological student is deprived of all the advantages of reference, unless he make an index for himself. All the leading facts of the two first volumes may be very readily embraced in one index; and I would suggest to Messrs. Kirby and Spence, that it would be desirable to furnish this assistance to the public without delay, as well as to provide every future volume with an index for itself, with the exception of the last, which will necessarily contain general references for the whole work.

I remain, dear Sir, yours sincerely,

NORFOLCIENSIS.

## III. *Separation of Plumbago from Cast-Iron.*

A curious paper on this subject has appeared in No. 14 of the Edinburgh Philosophical Journal; the facts related in which we shall proceed to state, and afterwards to offer a few remarks on the theory which has been deduced from them by the writer of the paper, Dr. Mac Culloch.

In the frequent examination of the metal of the iron guns delivered by the contractors to the ordnance, by solution, according to Bergman's suggestion, Dr. M. found that the quantity of plumbago in the metal bore no relation to its strength or goodness, but that the results were sometimes curious, from the very variable quantity which different specimens of the iron contained. The trials were far from confirming the opinion that the worst or weakest metal contained most plumbago; and on one occasion, an exceedingly tough specimen was found to contain a most unusual proportion of this substance, and that in a disengaged state, being visible on breaking the metal; which appeared as if it had been rubbed with powdered black-lead, and left its trace on paper. Where the plumbago was most abundant, the colour of the fracture of the iron was most grey; and Dr. M.'s trials go to prove, in contradiction to the opinion of many iron-founders, that the greyest metal is the toughest. The plumbago was always obtained in the state of powder, and in very small proportion when compared to that obtained from pig-iron.

The same experiments were repeated on the pig-metal used for shells, which is generally distinguished into three kinds; white, grey, and black. All these afforded very large proportions of plumbago, but various in different specimens; the black metal seemed to afford the greatest quantity, but that which was most brilliant and silvery also

yielded it in abundance. In 1807 or 1808, Dr. Mac Culloch was requested to go to a London porter brewery to see an appearance which had very much surprised the people employed in it. They had taken some iron out of their porter backs in making repairs, and had found it, as they said, red-hot. This was found to be an exaggeration; but on removing the iron articles from the porter, they became so hot on scraping off the surface, that it was disagreeable to handle them; while they smoked from the evaporation of the moisture: they were cast cones, perforated with holes, and about an inch thick, used as strainers to prevent foreign substances from getting into the pipes, and had been immersed in the porter for many years. On examination, some of them were found to be entirely plumbago; while in others there was a thick coat of that substance on each side, a little iron only remaining in the middle.

Shortly afterwards, Dr. M. met with the following relation in one of his journeys in the Western Isles. In 1740 an attempt was made to weigh one of the Spanish Armada, which had foundered off the coast of Mull; this proved unsuccessful, but some brass and iron guns were brought up, the former bearing the date 1584. The iron guns were deeply corroded; and on scraping them, it was said that they were found so hot that they could not be touched, and that they did not become cool till they had been two or three hours exposed to the air.

The following are the general results of Dr. Mac Culloch's experiments on this subject: The blackest pig-metal appears to yield the greatest quantity of plumbago, and in the most solid state. When the experiment is complete, the produce equals the iron in bulk, and is a solid mass capable of being cut by a knife, even into pencils; but as far as has been observed, it is of a much more coarse grain, or scaly granular texture, than natural black-lead. To procure it in perfection, the acid should be very weak, and the operation is then necessarily very tedious. Acetic acid appears to be the best, and it is by this that it is produced in porter-backs in the waste-pipes of breweries, and in calico-printing-houses, where sour paste is employed. If the experiment be perfect, the plumbago becomes hot on exposure to the air, smoking while there is any moisture to be evaporated, particularly when the surfaces are scraped off in succession; there is no apparent difference in the plumbago before and after this operation. When the substance does not heat, on being taken out of the fluid, the whole process of oxygenation appears to have taken place in the solution, probably from an excess of strength in the acid.

From these facts, Dr. Mac Culloch draws the following inferences: that plumbago, by which name he designates the carbon, as it exists in cast-iron, is a metal, and black-lead its oxide: that in white-pig, probably, the combination is this metal and iron; and that in the black it exists in a state of approximation to black-lead: that the operation of the acid is to dissolve the iron, and to oxygenate the plumbago so as to convert it into black-lead: and that if the acid be strong, the whole operation is completed in the solvent; otherwise some additional oxygen is required to produce it in a state of permanence in the air, and that the absorption of this generates the heat in the experiment: lastly, that the metallic nature of the base of charcoal is also proved from these facts. And lest this reasoning should be deemed unsatisfactory, Dr. M. adds the following argument: "The specific gravity

of pig-iron is about 7'6, and that of black-lead is 2, or less. Now the bulk of black-lead procured in the experiment is equal to that of the original iron exposed to solution. Two such bodies could not co-exist in the same space, or, if that could be imagined, the specific gravity of such iron must be far more different from that of pure iron than it is. If pure iron indeed is freed from the effects of condensation by heating, it scarcely differs in specific gravity from pig-metal. Thus while we conclude that the plumbago combined with the iron is a metal, we may also infer that the specific gravity of that metal is not very different from that of iron."

Upon this theory we trust that Dr. Mac Culloch will excuse our offering the following remarks: In the first place, nothing but carbon has been detected in the diamond by the most refined and delicate investigations of chemistry, those of Sir H. Davy, Messrs. Allen and Pepys, and others: that gem, therefore, must be a form of Dr. M.'s new metal; now if black-lead be its oxide, supposing it to contain only one atom of oxygen, then, when equal weights of each substance are burnt in oxygen gas, the former should require, for its complete conversion into carbonic acid, *twice as much oxygen as the latter*; but the experiments of Messrs. Allen and Pepys have demonstrated, that both these substances require for their combustion *precisely the same volume of oxygen*.

Secondly, Dr. Mac Culloch does not seem to be aware of the existence of *silicium* in cast-iron. The experiments of Mr. Daniell, recorded in vol. ii. of the Journal of Science, show that the absorption of oxygen, and the consequent evolution of heat, by the black-lead separated from the metal, is owing to the conversion of *silicium* into *silica*, and not to that of any unknown principle into black-lead.

We are not sure that we understand the concluding argument, but does it not indicate, either that Dr. Mac Culloch considers cast-iron to be a mechanical mixture of carbon and the metal, or that he thinks, (which is likewise contrary to the known fact), that the arithmetical mean of the specific gravities of the ingredients of a compound is the true specific gravity of the compound itself?

## ARTICLE XII.

### NEW SCIENTIFIC BOOKS

#### PREPARING FOR PUBLICATION.

A Narrative of a Voyage round the World in the Uranie, Capt. Freycinet, dispatched on a Scientific Expedition by the French Government during the Years 1817, 1818, 1819, and 1820; in a Series of Letters to a Friend. By J. Arago, Draftsman to the Expedition. The Work will form a Quarto Volume, embellished with 26 Engravings.

A Description of an Antediluvian Den of Hyænas, discovered at Kirkdale, in Yorkshire, in 1821, and containing the Remains of the Hyæna, Tiger, Bear, Elephant, Rhinoceros, Hippopotamus, and 16 other Animals, all formerly Natives in this Country. With a Compa-

rative View of many similar Caverns and Dens in England and Germany; and a Summary Account of the Evidence of diluvian Action afforded by the Forms of Hills and Valleys, and the general Dispersion of Beds of Gravel and Loam, containing similar Bones, over great Part of the Northern Hemisphere. By the Rev. W. Buckland, FRS. FLS. and Professor of Mineralogy and Geology in the University of Oxford. Illustrated by a Map, Views, and Sections of the Caves and Drawings of the Animal Remains.

A Treatise on Navigation and Nautical Astronomy, adapted to Practice and the Purposes of Elementary Instruction. By Mr. Reddle, of the Royal Naval Asylum.

JUST PUBLISHED.

Illustrations of the Inquiry respecting Tuberculous Diseases. By John Baron, MD. Physician to the General Infirmary at Gloucester. With coloured Plates. 8vo. 15s.

Lectures on the Structure and Physiology of the Male Urinary and Genital Organs of the Human Body, and on the Nature and Treatment of their Diseases, delivered before the Royal College of Surgeons. By the late James Wilson, FRS &c. 14s.

Journal of an Horticultural Tour through Flanders, Holland, and the North of France. By a Deputation of the Caledonian Horticultural Society, consisting of Mr. Neill, Secretary; Mr. Hay, Planner; and Mr. Macdonald, Chief Gardener at Dalkeith Park. With Plates. 8vo. 16s.

A new View of the Infection of Scarlet Fever, illustrated by Remarks on other contagious Disorders. By W. Carmichael, MD. FRS. Fellow of the College of Physicians, &c. 8vo. 5s.

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## ARTICLE XIII.

### NEW PATENTS.

J. Witcher, Helmet-row, Old-street, mechanic, M. Pickford, Wood-street, carrier, and J. Whitbourn, Goswell-street, coachsmith, for an improvement in the construction of wheels.—Sept. 27.

S. Pratt, bond-street, trunk-manufacturer, for improved straps and bands for the better securing of luggage.—Sept. 27.

T. Binns and J. Binns, Tottenham-court-road, engineers, for improvements in propelling vessels, and in the construction of steam-engines.—Oct. 18.

W. Jones, Bodwellty, Monmouthshire, engineer, for improvement in the manufacturing of iron.—Oct. 18.

S. Wilson, Streatham, Esq. for a new manufacture of worsted.—Oct. 18.

U. Lane, jun. Lamb's Conduit-street, straw-hat manufacturer, for an improvement in the platting of straw, and in manufacturing bonnets, &c. therefrom.—Oct. 18.

J. Williams, Cornhill, stationer, for a method to prevent the frequent removal of the pavement and carriage-path, and for laying down and taking up pipes, &c.—Oct. 18.

ARTICLE XIV.

METEOROLOGICAL TABLE.

1822.	Wind.	BAROMETER.		THERMOMETER.		Evap.	Rain.	Daniell's hyg. at noon.
		Max.	Min.	Max.	Min.			
11th Mon.								
Nov. 1	S W	29.97	29.81	61	55	—	—	
2	S W	30.10	29.81	62	48	—	10	
3	S W	30.36	30.10	58	38	—	—	
4	W	30.36	30.34	53	40	—	—	
5	W	30.34	30.29	55	46	—	—	
6	S W	30.29	30.05	55	40	—	—	
7	S W	30.05	30.03	58	38	—	—	
8	N	30.07	30.03	52	28	—	—	
9	N E	30.07	29.85	53	39	—	29	
10	N E	30.35	29.87	52	40	—	—	
11	N E	30.35	30.19	50	40	—	—	
12	S	30.19	30.01	56	42	—	09	
13	N W	30.01	29.55	50	33	—	18	
14	N W	29.61	29.35	51	40	—	32	
15	S W	29.43	29.35	51	41	—	58	
16	N E	29.71	29.40	44	32	97	62	
17	S W	29.72	29.71	51	45	—	05	
18	S W	29.95	29.72	55	42	—	—	
19	S W	29.90	29.78	56	41	—	36	
20	Var.	29.78	29.75	56	49	—	13	
21	W	29.98	29.78	51	40	—	—	
22	S W	29.98	29.74	52	45	—	30	
23	S W	29.99	29.74	52	43	—	—	
24	S W	29.92	29.70	53	45	—	07	
25	S W	29.70	29.68	52	45	—	04	
26	S W	29.70	29.68	55	45	—	—	
27	S W	29.74	29.60	50	36	—	05	
28	Var.	29.58	29.46	45	36	—	15	
29	S W	29.58	29.50	46	31	—	08	
30	S W	29.50	29.50	47	34	91	05	
		30.36	29.35	62	28	1.88	3.46	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

## REMARKS.

*Eleventh Month.*—1. Fine. 2. Cloudy. 3. Fine. 4, 5. Cloudy and fine. 6—8. Fine. 9. Foggy. 10. Cloudy. 11. Morning, foggy. 12. Rain at night: day, fine. 13, 14. Cloudy. 15. Morning, fine: night, rainy. 16. Rainy. 17. Cloudy and fine. 18. Cloudy. 19. Cloudy: night rainy. 20. Cloudy. 21. Fine. 22. Cloudy. 23. Cloudy and fine. 24. Fine. 25. Fine. 26. Cloudy. 27. Cloudy and fine. 28. Rainy. 29. Showers. 30. Showers: overcast.

## RESULTS.

Winds: N, 1; NE, 4; S, 1; SW, 17; W, 3; NW, 2; Var. 2.

## Barometer: Mean height

For the month..... 29.860 inches.

For the lunar period, ending the 6th ..... 29.860

For 13 days, ending the 8th (moon north) ..... 30.048

For 14 days, ending the 22d (moon south) ..... 29.924

## Thermometer: Mean height

For the month..... 46.650°

For the lunar period..... 50.616

For 31 days, the sun in Scorpio ..... 50.000

Evaporation..... 1.88 in.

Rain. .... 3.46

# ANNALS

OF

## PHILOSOPHY.

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FEBRUARY, 1823.

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### ARTICLE I.

*Experiments and Observations on Indigo, and on certain Substances which are produced from it by Means of Sulphuric Acid.* By Mr. Walter Crum.

(To the Editor of the *Annals of Philosophy*.)

SIR,

Glasgow, Jan. 1, 1823.

INDIGO may be obtained in a state of tolerable purity by the ordinary process of agitating the yellow liquid, which forms the dyer's blue vat, with common air, till the deoxidized indigo which is there held in solution by lime-water is revived; and then digesting the precipitate in dilute muriatic acid, to remove a little iron, and carbonate of lime, which it contains. In this state, it is known by the name of *precipitated indigo*, and must be distinguished from the powder so called by Bergman, a substance to which I shall refer more particularly in the sequel. It generally contains a small portion of sulphate of lime, and, as Dr. Thomson observed, a little resin, which is removable by alcohol.

It has been long known, that when indigo is heated, it sublimes. The first mention of this fact that I have noticed is contained in a work on calico printing, published in 1789 by O'Brien, a pattern drawer in London, who also gives a method for collecting the sublimate. "The curious may sublime indigo, and thereby procure flowers, as with zinc, sulphur, &c. For experiments on a small scale, it may be done in a common flask

*New Series*, VOL. V.

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over a common fire, defending the flask from the contact of the fire."

To prepare these flowers, Chevreul directs\* that eight grains of common indigo in powder, in a covered crucible, should be placed upon burning coals; in which case the colouring matter will crystallize in the middle part of the crucible, from which it is to be detached with a feather when cold. I have never been able to obtain any quantity of it in this way; part of it is always discoloured, and the whole of it is condensed upon the ashes, which are in the state of a fine powder, and from which it cannot be detached without contamination.

I succeeded, however, by another method to procure as much as I needed for my experiments, perfectly pure. I used the covers of two platina crucibles, nearly three inches in diameter, of such a form, that, when placed with their concave sides inwards, they were about three-eighths of an inch distant in the middle. I placed thinly about the centre of the lower one ten grains of precipitated indigo, not in powder, but in small lumps of about a grain in weight; then, having put on the cover, I applied the flame of a spirit-lamp beneath the indigo. In a short time, this substance partially decomposed, begins to melt, and the purple vapour to be disengaged, which is known by the hissing noise that accompanies it. The heat is continued till this noise nearly ceases, when the lamp is withdrawn, and the apparatus allowed to cool. Then, on removing the cover, the sublimed indigo will be found planted upon its inner surface, with sometimes a few long needles upon the bottom of the apparatus, which are easily removed from the button of coaly matter that remains. In this way I have generally obtained 18 to 20 per cent. of the indigo employed; a small quantity unavoidably escapes, but I am persuaded that very little more can, by any means of this kind, be obtained. I have made several attempts, by using different forms of apparatus, to prepare this substance on a greater scale; but have found none that answers so well as that which I have described. When the cover is kept cool, as by means of a wet cloth, none of the indigo condenses upon it. The interior of the apparatus being then comparatively cold, the vapour is condensed before it reaches the cover, falling back, or rather crystallizing upon the cinder. The same effect is produced when ashes are formed instead of a fused coal; and pure indigo when pounded, and common indigo in any shape, always leave a quantity of loose matter.

Those who cannot readily procure precipitated indigo for the purpose of subliming, may find it convenient to combine with my method, one lately given in the *Journal de Pharmacie*, by MM. Le Royer and Dumas. It consists in spreading about 30 grains of common indigo in coarse powder upon an open silver capsule,

and applying the heat of a spirit-lamp till all the sublimed indigo is formed upon the surface of the ashes. On repeating this process, I find that ten parts of common indigo yield one of sublimed indigo, which is, however, far from being pure. By resublimation in my apparatus, it is again reduced one-half from the loss of its impurities, and some vapour.

I shall here describe what else takes place during the sublimation. Ten grains of precipitated indigo in the apparatus I have described yielded

1·88 grain of sublimed indigo.

6·44 grains of cinder remained, and consequently

1·68 grain of volatile matter escaped.

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10·00

Thirteen grains of the same indigo, kept a quarter of an hour at a strong red heat in a small platina crucible, firmly, though not exactly closed, left 7·9 grains of cinder, which is equal to 61 per cent.

In order to ascertain what gases were evolved during this destruction of the indigo, I introduced 5·28 grains into a small glass tube connected with a mercurial trough. On applying the heat of a spirit-lamp, the vapour of indigo was formed, and condensed in the colder part of the tube, but was at last destroyed by repeated applications of heat. A quantity of water appeared in the tube, and 0·96 cubic inch of gas was found in the receiver. On removing the tube, it was found to have lost in weight 0·71 grain, or 13·5 per cent. of the indigo employed. The water that was formed had a disagreeable burnt ammoniacal smell. I found, on analyzing the gas in the receiver (making allowance for the common air of the apparatus, and a small portion of gas remaining in the tube), that it consisted for 100 of indigo of

Carbonic acid ..... 2·8

Carburetted hydrogen and carbonic oxide ..... 0·8

Azote ..... 1·9

The difference between the sum of these and the

loss 13·5 was water, with a little ammonia. .... 8·0

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13·5

### *Sublimed Indigo.*

Indigo sublimes in long flat needles, which readily split when they are bruised into four-sided prisms.

Viewed at a particular angle, they have the most brilliant and intense copper colour; but when lying in heaps, they have a rich chesnut-brown colour; one that would be produced by mixing a very little yellow with a bright but deep reddish purple.

Besides these needles, this substance is found in the form of

plates, much broader than the needles, and extremely thin; twisted sometimes almost into tubes. These appear to the naked eye perfectly opaque. I was not a little pleased, however, with their unexpected appearance, when seen through the microscope. Viewed obliquely, they appear still opaque, and copper coloured like the needles; but when held perpendicularly to the rays of light, they are seen to be transparent, and of a beautiful blue colour, exactly similar to a dilute solution of indigo which has been acted upon by sulphuric acid. Their intensity varies, according to the thickness of the plate, from a blue just distinguishable from white, to one almost black. The bronze colour which these crystals assume when in heaps is obviously a mixture of the copper colour with this blue.

The vapour of indigo is transparent, and of a most beautiful reddish-violet colour, resembling a good deal the vapour of iodine, but sufficiently distinguished from it by the shade of red. The sublimation takes place at the heat of about  $550^{\circ}$  Fahrenheit; for the vapour rises at a heat lower than that of melting lead, and requires more than the melting heat of bismuth. Upon the rough bright surface of the lead, I observed some of the crystals melt while the vapour was rising; but I have in no other circumstances remarked any thing like fusion, till the substance was decomposed. Hence the melting point of indigo, its point of volatilization, and that at which it is decomposed, are remarkably near each other.

The specific gravity of sublimed indigo is 1.35.

These crystals sublime when heated in open vessels, leaving no residue. In close vessels, the vapour is at first reddish-violet, as in the open air; but as the heat advances, it acquires a tinge of scarlet; and before it is entirely decomposed, becomes deep scarlet, and then orange coloured: a quantity of charcoal is at the same time deposited.

*Action of Oils.*—Of the essential oils, oil of turpentine dissolves at its boiling heat as much indigo as gives it the fine violet colour of the vapour of indigo, but a slight reduction of temperature is sufficient to precipitate the whole of it. The fixed oils and fatty substances, as they may be heated to a higher degree, exert a more powerful action upon indigo. None of them that I have tried act upon it at the heat of boiling water; but when the heat is increased, they gradually dissolve it, acquiring the colour of its vapour much more deep than oil of turpentine does. If the solution be cooled at this period, the indigo precipitates blue. As the heat is continued, more of the indigo is dissolved, but the colour of the solution begins to change; it gradually inclines to crimson, and has then begun to be destroyed. It is now green when cooled. Heated still more, the solution is of a strong crimson; then becomes orange; and at last, when entirely decomposed, it is yellow, which colour it retains when cold.

*Determination of the Ultimate Constituents of Indigo.*

The apparatus which I employ for the analysis of indigo with peroxide of copper, is simply a tube of green glass, seven inches long, and three-eighths of an inch wide, shut at one end, and connected with a mercurial trough by means of a small bent tube, joined to the other by a tube of caoutchouc firmly tied.

The indigo is first ground with a quantity of peroxide of copper, and when that is introduced into the tube, another portion of peroxide is put over it, and then some metallic copper thrown down from its sulphate by a plate of zinc, and ignited in a close vessel. Lastly, I fill up the remaining space (about a couple of inches) with a piece of glass tube, shut at both ends, which nearly fits the other. This keeps the materials in their place, and reduces as much as possible the quantity of common air in the apparatus. The tube is then heated with three spirit-lamps, of which two keep the metallic copper and the pure peroxide at a red heat; while the third traverses gradually that part of the tube which contains the indigo.

When I began these experiments, I took the usual precaution of heating the peroxide of copper to redness immediately before I made use of it. I soon found, however, that before I could get the vegetable matter ground with it, and introduced into the tube, it had attracted a quantity of water from the air, which varied according to the hygrometric state of the atmosphere, and of course, the length of time employed. It then occurred to me that all error from this source would be obviated, if I allowed the peroxide to be saturated with moisture, by leaving it freely exposed to the air, and at every analysis (at least when the state of the atmosphere changed), press 150 grains of it into a small platina crucible which just holds that quantity, heat it dull-red within another crucible, and ascertain its loss of weight without removing the cover.\*

I have in this way made several analyses of indigo, which differed very little from each other. I choose the following one as being nearly a mean of the rest:

*Analysis.*—One grain of sublimed indigo was ground with 90 grains of peroxide of copper; 25 grains more were put over it in the tube; and above that, 30 grains of metallic copper. These substances contained 0.11 grain of water.

\* Berzelius has pointed out the instantaneous attraction of water by the oxide of copper (*Ann. de Chimie*, xvii. 27), and cautioned chemists against the fallacy arising from the neglect of it; and M. de Saussure, in a note to his analysis of hog's lard (*Ann. de Chimie*, xi. 395), makes a general remark upon those analyses where peroxide of copper is employed. "Ses resultats" (those of M. Berard), "ainsi qu'un grand nombre de ceux obtenus par ce procédé, m'ont paru pécher par excès d'hydrogène." This excess is owing in all probability to the neglect of a small portion of water attracted by the peroxide. I have found 100 grains when newly prepared take as much as 3-10ths of a grain of water for saturation in damp weather. When it has been several times used, it does not take so much.

The tube, after the operation, had lost in weight 3.17 grains, of which 1 grain being indigo, and 0.11 grain water from the copper, 2.06 grains were oxygen supplied by the peroxide. The gas in the receiver was = 6.46 cubic inches, mean temperature and pressure. Of this, potash absorbed 5.82 cubic inches, leaving a residue of 0.64 cubic inch.

This residue consists of the common air of the apparatus, together with the azote contained in the indigo. By filling the mouth of the tube, as I did, with a round piece of glass, and using a very small conducting tube, scarcely any empty space is left in the apparatus; but the peroxide of copper, being a very loose substance, and merely shaken down into the tube, without being pressed, encloses a quantity of air, which is very considerable. This, on the idea that the quantity was trifling, has sometimes been neglected altogether; but more frequently as much of it as possible has been driven out, by heating a part of the tube, previous to the commencement of the experiment, and the remainder neglected. A very simple and obvious method of ascertaining with accuracy its amount, consists in making an analysis of any vegetable substance, as *sugar*, which contains no azote, in the same apparatus, and with the same quantity of materials. Here nothing passes into the receiver but carbonic acid, and the whole of the common air which the apparatus contained. The former, being absorbed by means of potash, leaves a quantity of common air, exactly equal to the common air in the indigo experiment; of course the difference between that, and the total residue of gas obtained from indigo, is azote. I thus found that the apparatus which I employed contained 0.26 cubic inch of common air; which, being deducted from the total residue after absorption by potash 0.64, leaves 0.38 cubic inch, or 0.1126 grain of azote, as the quantity of that substance in a grain of indigo. This method, as well as that of estimating the water in peroxide of copper, I communicated to Dr. Ure, when I mentioned to him the existence of hydrogen in indigo.

The 5.82 cubic inches carbonic acid gas + 0.08 which I found to remain in the apparatus = 5.9; and this, allowing for aqueous vapour, is  $5.9 \frac{29.1 - 0.676}{29.1} = 5.762$  cubic inches dry carbonic acid gas, which consist of 0.7322 grain carbon, and 1.952 grain of oxygen. But 2.06 grains of oxygen were taken from the peroxide of copper, which is 0.108 grain more than was necessary to burn the carbon. This indicates 0.0135 grain of free hydrogen.\* The remainder is oxygen and hydrogen in the

\* The results of two careful experiments made at different times with loaf sugar, exactly in the same manner as with the indigo, perfectly satisfied me as to the accuracy of this part of the experiment. Though the quantities of carbon were not precisely alike in these two experiments, the loss of weight sustained by the peroxide, after deducting the water it contained, was exactly equal to the respective quantities of oxygen in the carbonic acid, calculated in the same manner as in the text. In one case, I obtained

proportions which form water. Hence the composition of indigo is

Carbon .....	73.22
Azote. ....	11.26
Oxygen .....	12.60
Hydrogen . ....	2.92
	<hr/>
	100.00

These numbers correspond very nearly to

1 atom of azote .....	1.75 or 10.77	
2 atoms of oxygen .....	2.00	12.31
4 atoms of hydrogen .....	0.50	3.08
16 atoms of carbon .....	12.00	73.84
	<hr/>	<hr/>
	16.25	100.00

The gas remaining in the apparatus at the conclusion of the experiment, was estimated by heating the same tube with an equal quantity of peroxide of copper without organic matter, and observing how much air was driven over into the receiver. The difference between this and the whole air, as found in the sugar experiment, is pretty nearly the bulk of gas remaining. The temperature of the gas when measured was 58°, and the barometric pressure 29.1 inches.

I have also made several analyses of well-dried precipitated indigo; and allowing for a minute portion of sulphate of lime which it contained, I got results which agreed perfectly with the analyses of sublimed indigo. I find too, that both these substances are acted upon in the same manner by other bodies. The different effect of heat depends obviously, upon some difference in the mechanical arrangement of their particles.

Brugnatelli has proposed to call this sublimate *indigogen*, because, when united to the fecula of the plant, it forms common indigo. But such a nomenclature is inadmissible in chemistry. Upon the same principle, potash ought to be called potassogen, because, when united to certain foreign substances, it forms crude potash. The same chemist considers this substance as a metal, because, as M. Von Mons informs us,\* he found it might be amalgamated with mercury. Dobereiner repeated this experiment of Brugnatelli, and formed not only the amalgam, but

41.55 per cent. of carbon, and in the other 42.14. The means of these give the following for the composition of sugar:

	Carbon .....	41.8
Water {	Oxygen .....	51.7
	Hydrogen .....	6.5
		<hr/>
		100.0

It is needless to add that the sugar was in both cases dried at 212° in a vacuum.

\* *Annals of Philosophy*, vi. 75.

what is still more wonderful, by placing this amalgam in a solution of nitrate of silver, he obtained crystals in the form of an artichoke, which were an alloy of the metal of indigo with silver. In consequence of these statements, I made several attempts to amalgamate sublimed indigo, as well by the process of Dobereiner, as by others which I thought more likely to succeed, but in vain. I could in no case perceive the least alteration in the fluidity of the quicksilver.

#### *Action of Sulphuric Acid upon Indigo.*

When indigo is digested in concentrated sulphuric acid, it is well known to suffer a remarkable change, being converted into a peculiar blue substance, entirely different from indigo, with which the Saxon blue is dyed.

This substance has been so little attended to by chemists that no one has yet thought of giving it a separate name. I shall venture to propose for it that of *cerulin*, from the colour of its solution.

The mixture of the blue substance with sulphuric acid is a semifluid, which requires a considerable quantity of water to dissolve it. When potash is added to this solution, previously filtered, a deep-blue precipitate is formed. I was surprised, however, when making the experiment, to find the precipitate as plentiful before one-fourth of the acid was saturated, as when the solution was made altogether neutral. To another portion, I added potash previously saturated with sulphuric acid, and found the same precipitate formed as with potash alone.\* I threw the precipitate upon a filter, and washed it with water, in order to examine whether it was the pure colouring matter that had separated. The first washing did not take away much of the precipitate; the next, however, sensibly diminished it; but with the third portion of water, it almost wholly disappeared.

In order to discover the cause of this increased solubility, I made a saturated solution of sulphate of potash in water, and, putting into it a little of the blue pulpy substance that remained upon the filter, I agitated it thoroughly. The solution remained altogether colourless. I found the same to be the case when the muriate, the acetate, or any other salt of potash, was employed. Alcohol also, and ether refused to dissolve this substance. Put into pure water, however, it immediately dissolved, forming the same deep blue solution that had passed through the filter.

\* That the solution of indigo in sulphuric acid is precipitated by neutral salts is not a new fact. Berthollet (Art of Dyeing, ii. 50) says, that the "fixed alkalies saturated with carbonic acid," as well as "alcohol, saturated solutions of alum, sulphate of soda, or other salts, containing sulphuric acid," form precipitates in this solution. My experiments show, I think, that the effect does not depend upon the presence of sulphuric acid in the precipitant; that some sulphates have no such effect; and that alcohol does not precipitate the original solution at all.

It seemed then only necessary to dissolve in water some salt of potash, to enable it to wash this substance without dissolving it. I found the acetate to answer extremely well, and this salt possesses the advantage over the muriate or sulphate of not being precipitated by alcohol from a weak solution in water, as they are. It may consequently be afterwards removed by washings with alcohol.

Such edulcorations with acetate of potash, I repeated so often upon a quantity of precipitate, taking it every time off the filter, and agitating it well in a phial with the liquid, that not  $\frac{1}{5000}$  th of a grain of the original soluble matter could be left in it. I then washed away the weak solution of acetate of potash as well as possible by means of alcohol, without, however, being able to remove all traces of that salt, although I used the alcohol somewhat dilute. The small quantity that remained could not materially affect the experiments I made upon it.

A portion of the substance thus prepared, when burnt in a large platina crucible, left a considerable quantity of ashes, slightly reddish coloured, which dissolved almost entirely in water. What remained was of a deep red, or rather brown colour, and was principally oxide of iron. The solution of the ashes was not at all alkaline; it gave a dense white precipitate with muriate of barytes; a very slight one with oxalate of ammonia; and with sulphate of alumina, large crystals of alum were formed in a few hours. It was sulphate of potash.

To remove all doubt of the precipitate having been sufficiently well washed to take away from it every thing that was not really insoluble in the menstrua I employed, and consequently the sulphate of potash, if that substance were not chemically combined with the cerulin, I mixed a large quantity of muriatic acid with the original solution in sulphuric acid, and formed the precipitate by means of muriate of potash. Then, having washed it as before, with the solution of acetate of potash, and afterwards with alcohol, I found that the solution of the ashes when the substance was burnt, gave indications with muriate of barytes, of abundance of sulphuric acid; while with nitrate of silver, no precipitate appeared; a quantity of small shining crystals alone being deposited after some time, which were sulphate of silver.

From these facts, I have no hesitation in concluding that this precipitate is a combination of cerulin with sulphate of potash. That salt forms more than a fourth of its weight. It may, therefore, be called *ceruleo-sulphate of potash*.

The salts of soda also form precipitates in the solution of cerulin with sulphuric acid, and these are likewise insoluble in solutions of potash or soda, though soluble to a certain extent in pure water. When heated, these ceruleo-sulphates dissolve even in solutions of their salts. On cooling, the greater part falls down again in blackish grains; a portion, however, remaining in

solution. The soda compound is evidently more soluble than that of potash. The washings of the former precipitate, with a cold solution of its acetate, are a little more coloured than those of the potash precipitate, with its acetate of the same strength. This last substance is totally insoluble in water containing one per cent. of acetate of potash, or even a half per cent. after two or threeedulcorations. I have generally employed a solution of two parts dry acetate of soda in a hundred of water; and any quantity of such a liquid may be used without the quantity of the substance being materially diminished.

The salts of ammonia likewise form precipitates in the sulphuric solution of cerulin, when not much diluted. The precipitate dissolves readily in hot solutions of ammoniacal salts, and again separates when cold, the whole mass becoming curdy. I believe this substance to be a combination of cerulin with sulphate of ammonia, from the quantity of that salt which I found in it, when as well washed as possible. It is much more soluble, however, than the ceruleo-sulphates of potash and soda, and, therefore, cannot be washed so freely as these fixed alkaline compounds. Potash and soda, and their salts, decompose it. It dissolves in great quantity in boiling pure water, and in 40 or 50 parts of cold water. It has the same general properties with the more insoluble compounds.

A corresponding combination with barytes may be formed by decomposing ceruleo-sulphate of potash by muriate of barytes. The compound formed is extremely insoluble. An abundant blue precipitate is thus formed in solutions of ceruleo-sulphate of potash, containing so little sulphuric acid, that they are not troubled in the slightest degree by a barytic salt, when the cerulin has been previously destroyed by nitric acid.

Similar compounds may be formed with other bases, whose sulphates are difficultly soluble in water; but these I have not particularly examined.

The salts of magnesia have no power of precipitating cerulin from its solution. The whole of the sulphuric acid in the original solution may be saturated with magnesia, without any precipitate being formed.

#### *Ceruleo-sulphate of Potash.*

This substance is of so deep a blue, when wet with water, as to appear absolutely black. When dry, it has a shining strong copper-red colour. By transmitted light it is blue. It attracts water from the air with great rapidity. In two hours, a portion which had been dried attracted a tenth of its weight.

It is soluble to a considerable extent in hot water. Cold water takes up  $\frac{1}{140}$ th of its weight of this substance, and forms a solution so deeply coloured, that when diluted with 20 parts of water in a phial of an inch in diameter, it may just be seen to be

transparent. Water in a wine glass containing  $\frac{1}{500,000}$ th of its weight of this substance is distinctly blue coloured.

The saturated solution is precipitated by spring water, and by every liquid that I have tried; except distilled water. From this it appears that the mere presence of any foreign substance in pure water greatly diminishes its solubility.

If the solution be diluted with 20 parts of pure water, it is still precipitated by solutions of the salts of potash and soda, lime, barytes, strontian, lead, and mercury. An addition of sulphuric or muriatic acid does not redissolve them. But neither ammonia nor any of its salts precipitate this weak solution. None of the salts of magnesia, zinc, or copper, nor the solutions of alum, sulphate of manganese, permuriate of tin, protosulphate or persulphate of iron, or nitrate of silver, decompose it. It is not precipitated by any of the acids, by infusion of galls, or by pure gelatine. Alcohol and ether do not precipitate the weak aqueous solution, though they do not dissolve any of the dry substance. Ceruleo-sulphate of potash dissolves readily in concentrated sulphuric acid, but not in concentrated muriatic acid.

When chloride of tin is mixed with the solution of this substance, its colour is immediately changed to yellow. This yellow product is not very soluble in water; it becomes blue again on the addition of any substance, as a salt of copper, capable of imparting oxygen to it.

When heat is applied to the blue substance, it does not melt; no purple vapour is given off, and in consequence of its being defended by the saline matter, a strong heat long applied is necessary for its being reduced to ashes.

When luminous objects, as the sun or moon, or the flame of a candle, are viewed through the blue solution of this substance, of the proper degree of intensity, they appear of a fine rich scarlet colour. It is worthy of remark, that a single drop of nitrate or sulphate of copper, mixed with a quantity of this solution, makes the same objects appear blue through it, although the general appearance of the liquid is not in the least degree altered. Zinc produces the same effect, though not so powerfully. Any acid restores to these mixtures the property of making luminous objects appear red, unless when a large quantity of copper has been added, which makes the liquid itself green.

Sir H. Davy\* was the first to discover that a vegetable substance had the power of precipitating in combination with certain neutral salts, which are themselves abundantly soluble in water. The carbonates of potash, soda, and ammonia, and the chlorides of tin and of iron, are among the substances which that philosopher found undecomposed in combination with tannin, in the precipitates formed by these salts in an infusion of

\* On Astringent Vegetables, *Phil. Trans.* 1803.

galls. It appears that cerulin acts a similar part, at least with the sulphuric salts. I am not aware, however, that any substance has been hitherto described by chemists which possesses the property I have found in the one here treated of, that of dissolving in pure water, and refusing to do so in neutral saline solutions which produce no change upon it. But in common life, some idea of this singular fact seems to have been long acted upon. Those who are in the habit of washing printed gowns, particularly dark ones, of colours not very permanent, always rinse them in a solution of common salt, or at least in very hard water, before they hang them up to dry. The salt, they say, fixes the colour, and prevents it from spreading out into the white, which it always does when they are suffered to dry in a cool place, without such immersion.

*On the Phenomena which are exhibited during the Formation of Cerulin.*

Some of the phenomena which attend the action of sulphuric acid upon indigo, have been noticed by different chemists. Bergman, in 1776, observed, that when indigo in powder was sprinkled upon sulphuric acid, greenish clouds were produced, which became blue by the addition of a drop of water.\* He added that the same effect was produced, but more slowly, without water. Haussman, of Colmar,† observed, that the acid in contact with indigo, became at first greenish-yellow, then deep-green, and at last blue. That gentleman remarked also, that the “effervescence and disengagement of sulphurous vapours, always observed in making the blue compound, leave no room to doubt that the acid exerts an action upon the particles of indigo, and that we should be wrong in considering this as a mere solution of indigo unaltered.” Berthollet, in his excellent work on Dyeing,‡ considers the change that takes place a species of combustion; the sulphuric acid furnishing the indigo with oxygen, and thereby being converted into sulphurous acid. Dr. Bancroft, whose work appeared soon after that of Berthollet, conceived the solution to be oxygenated indigo combined with sulphuric acid. Hence he gave it the name of sulphate of indigo.§

Such, as far as my information goes, is the extent of our knowledge, or rather our opinions, upon this subject. I shall state what appearances I have myself observed during this process.

When indigo is put into sulphuric acid, it is dissolved, and the acid assumes a yellow colour. When this solution is dropped into water, it becomes instantly blue; but the substance so produced is by no means the same as that which is formed after

\* Opusc. tom. v. p. 7. Edit. 1788.

† Journal de Physique for March, 1788.

‡ Vol. ii. pp. 50, 66, English translation.

§ On Permanent Colours, pp. 104, 132.

some time, without the assistance of water. It is indigo altogether unchanged, which precipitates, and leaves the sulphuric acid perfectly colourless. If the yellow solution be exposed to the open air in a watch glass for a short time, the blue colour is restored in the same manner, and the indigo falls down. This effect is produced, not by the action of the air, but merely by the absorption of moisture.

A considerable increase of heat takes place when the two substances are put together. There can be little doubt that this is caused by the sulphuric acid abstracting and combining with the 14.2 per cent. of water contained in the indigo.

If the yellow solution above mentioned be allowed to remain without dilution, it becomes blue in a few hours, and this is effected without the assistance of air, as I found by making the experiment in a small phial, the mouth of which was sealed up, as soon as the materials were put together. In consequence of the darkness of the liquid, these changes of colour can only be observed in the thin film which wets the empty part of the phial, when it is agitated.

All the chemists who have treated of this process, mention the formation of sulphurous acid during the solution of indigo, and, attributing this to the action of the indigo itself upon the acid, they naturally concluded that that substance became oxidated at the expense of the acid. But these chemists employed only the indigo of commerce in their experiments; a substance which contains more than half its weight of impurities, and great part of these vegetable matter. I have found that it is these impurities alone which decompose the acid; for during the solution of sublimed indigo, not a trace of sulphurous acid can be detected, though the heat of boiling water be applied for hours. Neither is there any production of hyposulphuric acid; for, if there were, it would be decomposed by the heat to which the liquid was exposed, or even by the presence alone of concentrated sulphuric acid, in which case sulphurous acid gas would be given off.

In less than 24 hours, if a slight degree of heat be applied, the indigo is entirely converted into cerulin; and, when mixed with water, it passes through the filter, leaving no residue whatever.

#### *On the Constitution of Cerulin.*

Since there is no production of sulphurous acid, nor absorption of air, during the formation of cerulin, it is clear that there can be no oxidation either of the carbon or hydrogen previously existing in the indigo. No carbon being deposited, and no gas evolved, during this process, prove also that the azote exists in the new substance, in the same proportion to the carbon that it does in indigo. That sulphuric acid does not enter into its composition, is evident from its precipitating with almost any sulphuric

salt, and carrying down no additional sulphuric acid of its own. It is only in the amount of combined water, then, that any alteration can possibly have taken place, and to ascertain whether in this case there has been an abstraction, or an addition of water to the indigo, it were only necessary to convert a given weight of that substance into cerulin, and to weigh the product, as M. de Saussure has done in the case of starch sugar. There are difficulties, however, which prevent such an experiment from being performed with any precision; principally the large quantity of sulphuric acid necessarily mixed with the product, and the solubility of the substance in water. I have contented myself with analyzing, by means of peroxide of copper, the ceruleo-sulphate of potash, after having ascertained as nearly as possible, by incineration, how much saline matter it contained. But, as this cannot be done with absolute precision, owing to the dissipation of a small portion of acid along with the vegetable matter, my results, particularly with regard to the hydrogen, were by no means so uniform as those which I obtained when operating upon indigo. All that is really necessary in this case, is to determine the quantity of carbon, which may be done with very little risk of error. The deficiency, after adding to the carbon the proportion of azote, and of free hydrogen found in indigo, is water.

Accordingly, I first of all dried a quantity of the blue substance at the heat of  $212^{\circ}$  in vacuo, and having weighed it as rapidly as possible, I exposed it to the air during a whole night, and noted the increase of weight from moisture. One portion of this was burnt to ashes. Another portion was ground with peroxide of copper, and the carbonic acid it produced was received over mercury. In this manner I found that 1 grain of pure cerulin yielded 4.5 cubic inches dry carbonic acid gas, which is equivalent to 0.5718 grain of carbon, reckoning 0.12708 grain as the quantity of carbon in a cubic inch of gas. The composition of the substance consequently is:

Carbon .....	57.18
Azote. ....	8.79
Oxygen .....	29.32
Hydrogen .....	4.71
	<hr/>
	100.00

This approaches so near to *indigo* + 4 *water*, that there can be little doubt such is its constitution.

1 atom azote .....	1.75 or	8.43
6 atoms oxygen .....	6.00	28.92
8 atoms hydrogen .....	1.00	4.82
16 atoms carbon .....	12.00	57.83
	<hr/>	<hr/>
	20.75	100.00

The quantity of azote in proportion to the carbon, I found by experiment to be the same as in indigo; but as all my attempts failed to determine the amount of free hydrogen, I preferred stating all the numbers after carbon, by calculation from the analysis of indigo, which is susceptible of much greater precision. The heat of a spirit-lamp is quite sufficient for the analysis of most vegetable substances which are not mixed with saline matter; but with cerulin, it is necessary to employ the full red heat of a charcoal fire for its entire combustion by the peroxide of copper.

There is not the slightest proof that any combination exists between cerulin and sulphuric acid in the original liquid. Alkalies, it is true, precipitate it from the solution; and this has been supposed to be the effect of a superior attraction on the part of the acid for the alkali, by which the vegetable substance was left at liberty; but such a theory falls to the ground as soon as it becomes known that neutral salts produce exactly the same effect; that magnesia does not precipitate it at all, though it neutralizes the acid; and that cerulin is itself soluble in water. Cerulin dissolves, indeed, in sulphuric acid, and that more abundantly than in water; but this does not argue the formation of a compound which we are entitled to call sulphate of indigo. Such a solution differs in no respect from that of resins and other organic bodies in the same acid, or even from the solution of these substances in alcohol or ether.

Those who are fond of speculating upon the manner in which the elements of water are arranged in organic bodies, may find it curious that sulphuric acid should abstract water from indigo, and not from cerulin, a substance which contains three times as much oxygen and hydrogen; or that the same acid which robs indigo of its water, should immediately restore three times as much.

*On a new Substance produced from Indigo by Means of Sulphuric Acid.*

While engaged with these experiments, I discovered that if the action of sulphuric acid upon indigo be stopped at a certain point, a new substance, altogether different from cerulin, is produced, possessing rather singular properties. It is formed at the instant that indigo changes from yellow to blue by the action of sulphuric acid.

By the following process, it is obtained of greater purity than by any other method I have been able to discover. Prepare a quantity of indigo by boiling it in sulphuric acid diluted with three parts of water, and drying, after it is well washed. By such treatment, it is deprived of more than a third of its weight of impurities. Mix one part of this purified indigo with seven or eight parts of concentrated sulphuric acid in a stoppered phial, and agitate the mixture occasionally, till it becomes of a bottle-green colour. Then mix it with a large quantity of distilled

water, and throw it upon a filter. By continuing to wash the filter with distilled water, the liquid which at first passes through colourless becomes more and more blue, and after some time, all the indigo which has been changed, passes through. The colourless washings must be thrown away. The blue liquid contains the new substance in solution, and does not differ in appearance from a solution of cerulin. On the addition of muriate of potash, the new substance precipitates of a most beautiful reddish-purple colour, exactly similar to the colour of the vapour of indigo. Let this precipitate be thrown upon a filter, and washed with distilled water, till the liquid which passes through forms no longer a whitish, but a red precipitate with nitrate of silver. It may then be dried.

If instead of precipitating by means of muriate of potash, we take the blue liquid which passes through the filter after all the sulphuric acid has been washed away, but while it is still deeply coloured, and evaporate this to dryness, we obtain the new substance, not purple coloured, but blue, like cerulin; and but for the difficulty of separating entirely the sulphuric acid, this would be the best method of preparing it. Indeed in no other way is it obtained free from saline matter.

From the property possessed by this substance of becoming purple-coloured on the addition of a salt, I have called it *Phenicin*, from the Greek word *ποινῆ*, purple; and, to prevent circumlocution, I shall hereafter make use of this term.

This substance, prepared with muriate of potash, is, when dry, of a brownish-black colour. Heated in a crucible, it gives off a little vapour of indigo. I was at first uncertain whether this might not proceed from indigo formed by the decomposition of part of the phenicin by the heat; but I shall state a fact which shows that the indigo may have another source, and that it may exist in a small quantity in the purple substance. After the filter is washed, till the washings are very slightly blue coloured, the liquid that passes through is precipitated blue, instead of red, by muriate of potash, and the precipitate consists of indigo with a little phenicin. Thus it appears that even indigo is, in certain circumstances, capable of dissolving in water. By drying the phenicin, prepared as I have stated, and redissolving it, a small quantity of indigo remains; but still the phenicin yields a little purple vapour when heated.

When the purple substance is burnt, it leaves about 15 per cent. of ashes, which dissolve in water, and consist of sulphate and muriate of potash.

Phenicin dissolves both in water and alcohol, and the solution in both cases is blue. It is precipitated again of its original purple colour by all saline substances whatever. Different salts; however, possess different powers of precipitation. Thus muriate of ammonia, chlorate and prussiate of potash, and muriate of soda, precipitate the phenicin entirely from about 60 times their

weight of any aqueous solution, and nitrate, muriate, and sulphate of potash, from about 100 times their weight. But the sulphates of magnesia, zinc, and copper, precipitate 2000 times their weight of a solution of phenicin; sulphate of iron about 3000 times; and alum and muriate of lime as much as 8000 times.

The phenicin being previously combined with a salt of potash, is not altered by being again precipitated by any alkaline salt. These salts do nothing more than saturate the water to such a degree, that the substance is no longer capable of dissolving in it. But the earthy and metallic salts combine with this substance, and displace the salt of potash previously united with it; and I have observed very little difference in the quantities necessary to precipitate solutions of different strengths. The precipitates formed by lime, barytes, alum, and copper, are totally insoluble in pure water, however well they may be washed. Those formed by iron and magnesia dissolve to a small extent, when freed from their former menstrua by filtration. Their solution is purple coloured.

Acids have no effect in preventing the precipitation of phenicin by saline bodies; and the precipitates once formed are not redissolved in the same liquid by the assistance of heat.

The method which I have given for preparing the new substance is tedious. As only a small part of the indigo is converted into phenicin, the quantity obtained each time is very small, and requires a great quantity of distilled water, and a long time to filter. But if we allow the indigo to be wholly converted into phenicin, its solution cannot be made to pass through any filter, however porous, or however well washed it may have been with water or ammonia. It was by accident that I discovered the possibility of filtering it when only a small portion of the indigo had had time to be changed; and it was only by preparing it in this manner that I found the purple colour to be owing to the presence of saline matter, and its own colour to belike that of cerulin.

The following is a method of preparing this substance in greater quantities, though not so pure. Mix together one part of indigo in powder, and 10 parts of concentrated sulphuric acid, in a phial, and agitate from time to time, till the blue colour, which the indigo loses at first, is completely restored. This, at the ordinary heat of summer, requires nearly three hours. At 100° Fahr. it is effected in about 20 minutes; and indigo mixed with sulphuric acid, at the heat of boiling water, becomes blue the instant the mixture is made. At 45° Fahr. 10 or 12 hours are necessary; and at lower degrees of heat, a still longer time, supposing always the quantity of materials small enough to cool very soon after mixture to the stated point. Pour this mixture into a large quantity of distilled water, and filter. Take the precipitate off the filter, wash it well with distilled water, contain-

ing the proportion of muriate of ammonia necessary to prevent the substance from dissolving in it, and filter again. Dissolve anew the precipitate in a large quantity of distilled water; heat the solution to drive off any particles of air which might prevent the impurities from subsiding, and let it stand two or three days in a tall vessel. Then draw off with a syphon as much as may be thought perfectly clear, leaving the remainder to be washed with more distilled water. Add to the solution any alkaline salt, till the substance be precipitated; then throw it upon a filter, and wash with distilled water till the liquid refuses to pass through.

The colour of the substance thus prepared is not much inferior in beauty to that prepared by the former process. Dried and burnt to ashes, a considerable quantity of earthy matter is always left, but very little of the alkaline salt that precipitated it. Its solution is always more or less purple, particularly if it be somewhat strong. In alcohol, it is completely blue, and the aqueous solution becomes blue when it is heated. After being dried, it does not dissolve in water.

When a solution of phenicin is precipitated, the liquid that remains is always more or less coloured with cerulin; and however often this be repeated upon the same material, a little cerulin is always left. If it has been heated, more cerulin is formed. It follows from this, that phenicin is changed into cerulin by the action of water alone.

Phenicin dissolves in the water of liquid ammonia without injury; but the fixed alkalies destroy it, though not very readily. Chloride of tin precipitates the solution, but gradually redissolves the precipitate, forming a yellow solution; and the phenicin is thrown down again of its own colour, by the salts of copper.

Phenicin dissolves readily in concentrated sulphuric acid, forming a blue solution; and if this be poured immediately into water, the greater part of it is precipitated again, the impurities of the acid being sufficient to prevent its solution in water. A portion is converted into cerulin, which remains in solution. When allowed to remain dissolved in sulphuric acid, it is soon entirely converted into cerulin; consequently in preparing phenicin by the second process, it is impossible to prevent the formation of a certain portion of cerulin.

#### *Constitution of Phenicin.*

The facts which I have stated to prove that cerulin differs from indigo only in containing more or less water, equally apply to this substance.

The perfect insolubility of phenicin in weak saline solutions enables us to determine pretty nearly how much of it a quantity of indigo is capable of producing. Ten grains of sublimed indigo were put into a small phial with 300 grains of sulphuric acid;

and after two hours and a half, when quite blue, the mixture was poured into a pint of water, and heated to boiling. It was then thrown upon a weighed filter, and washed; first with boiling water, in which had been dissolved a minute portion of sulphate of lime, and afterwards with boiling pure water. The purple substance remaining upon the filter, when thoroughly dried, weighed 9.61 grains; a portion of which being burnt, left a quantity of ashes, equal to 1.37 grain of the whole; consequently only 8.24 grains of pure phenicin had been produced. The washings, which were deeply blue coloured from the cerulin that had been formed, were put together; and when diluted with water to 95 cubic inches, exactly equalled in intensity a solution of 1 grain of indigo converted into cerulin, in 30 inches of water. Hence 8.16 grains of the indigo had been expended on the production of cerulin, and 6.84 grains had produced 8.24 grains of phenicin.

In another experiment, 4.2 grains of indigo produced 5.13 grains of phenicin; and in a third, 4.79 grains produced 5.65 grains. The mean of these makes 100 of indigo produce 120 of phenicin.

By analysis with peroxide of copper, I have obtained results which indicate a smaller increase of weight; and I am inclined to prefer these to the synthetic result, because the phenicin operated upon is much more pure, and the experiment altogether less liable to error. One grain of pure phenicin produced 5.085 cubic inches of dry carbonic acid gas, which contain 0.6462 grain of carbon. Hence, calculating as in the case of cerulin, the substance consists of

Carbon .....	64.62
Azote .....	9.91
Oxygen .....	21.49
Hydrogen .....	3.98
	<hr/>
	100.00

This is very nearly *indigo* + 2 *water*, and its atomic proportions may be thus stated:

1 atom azote. ....	1.75 or	9.46
4 atoms oxygen .....	4.00	21.62
6 atoms hydrogen. ....	0.75	4.05
16 atoms carbon. ....	12.00	64.87
	<hr/>	
	18.50	100.00

The experiments of Mr. Smithson, related in the Philosophical Transactions, have given us very correct ideas on the nature of a number of the vegetable colouring matters. It is sufficiently obvious that phenicin is not the principle which colours any of

the purple or blue vegetables examined by that chemist. I collected a number more of such purple flowers as are most commonly met with, and dipped them separately in concentrated sulphuric acid. But instead of becoming blue, they were uniformly changed to red, and formed red coloured solutions on the addition of water. Future inquiries, therefore, must determine whether phenicin exists ready formed in nature either in the blue or in the purple state.

Alcohol modifies remarkably the action of sulphuric acid upon indigo. A mixture of three parts of alcohol, of specific gravity 0.84, and two parts of acid, dissolves indigo without rendering it yellow, and the solution may even be filtered through strong paper. Probably a larger quantity of pure alcohol might be employed. On the addition of water, the indigo is precipitated without alteration; and if common indigo has been used, resin precipitates along with it. It may remain dissolved in this mixture any length of time without conversion into phenicin. A solution of phenicin in sulphuric acid may also be mixed with alcohol without precipitation, and the acid is rendered incapable of converting it into cerulin.

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## ARTICLE II.

*Some Experiments on the Changes which take place in the fixed Principles of the Egg during Incubation.* By William Prout, MD. FRS.\*

"In the year 1816," says Dr. Prout, "I was induced to commence a series of experiments on the egg during incubation, with the view of ascertaining the nature of the changes which take place during that process. My inquiry was chiefly limited to the fixed principles; namely, the earthy and saline matters; but my attention was more particularly directed to the source whence the earthy matter, constituting the skeleton of the chick, was derived."

"With these views, the egg was analyzed in its recent and unaltered state, and at the end of the first, second, and third weeks of incubation. My experiments were chiefly confined to the eggs of the domestic fowl, but have been likewise partially extended to those of the duck and turkey. The investigation has been renewed, and the experiments repeated at various intervals since the period above-mentioned; but the difficulty of the

\* Abstracted from the *Philosophical Transactions* for 1822, Part II.

subject and various accidents have prevented me from completing them till the present time; and the results, which, after all, are much less perfect than I could wish, I have now the honour of submitting to the Society."

*Preliminary Experiments on the Egg in its recent and unaltered State.*

It is here stated, that "the specific gravity of new laid eggs has been found to vary from 1·080 to 1·090;" and their apparent diminution in specific gravity is shown to depend "on the substitution of air for a portion of the water of the egg which escapes." A table is given, showing the gradual loss of weight of an egg during a period of two years. The original weight on the 19th May, 1820, the day it was laid, was 907·5 grains; and on the 19th of May, 1822, it had become reduced to 363·2 grains; having sustained a loss of 544·3.

"Hence we learn," says the author, "that a moderately sized egg loses on an average about ·75 grain in 24 hours, and that uniformly during a very long period. . . . On being broken, the whole of the contents of this egg were found collected at the smaller extremity in a solid state; but on being put into water, they absorbed a large portion of that fluid, and assumed an appearance not much unlike those of a recent egg; the smell also was perfectly fresh."

"The relative weights of the shell, albumen, and yelk of different eggs," continues Dr. Prout, "are very different. With the view of investigating this point, and of obtaining something like an average, the following experiments were made. The eggs were boiled hard in distilled water, and the different parts weighed immediately in their moist state."

Shell and membrane.	Albumen.	Yelk.	Total.
80·0	394·3	289·0	763·3
108·0	593·0	273·5	974·5
107·3	575·8	236·2	919·3
71·5	516·5	215·0	803·0
103·0	503·7	269·3	876·0
107·0	515·3	273·4	895·7
93·2	605·5	252·4	951·1
92·7	515·7	257·0	865·4
96·8	510·6	210·8	818·2
77·6	567·4	241·5	886·5
93·7	529·8	251·8	875·3

A similar table is then given, in which the weight of each of these eggs is supposed to be 1000 grains; and Dr. Prout thus

continues: "Hence, if we suppose a recent egg to weigh 1000 parts, the relative proportions of the shell, albumen, and yolk, will be as 106.9, 604.2, and 288.9; and for the sake of easier comparison in all the subsequent experiments, the numbers are reduced to the above standard, or to the supposition that the original weights of the eggs employed were, when just laid, exactly 1000 grains.

"When an egg is boiled in water, it loses weight, particularly if it be removed from the water when boiling, and be permitted to cool in the open air; the water also on examination will be found to contain a portion of the saline contents of the egg. The loss of weight from boiling is by no means constant, but has been found to vary from 20 to 30 grains, on the supposition, that the original weights of the eggs employed were 1000 grains. On the same supposition, also, it has been found, that the quantity of saline matter obtained by evaporating to dryness the distilled water in which an egg has been boiled, amounts, at an average, to about .32 grain. This saline residuum is strongly alkaline, and yields traces of animal matter, sulphuric acid, phosphoric acid, chlorine, an alkali, lime and magnesia, and carbonates of lime and magnesia; in short, of almost every principle existing in the egg. The carbonate of lime, however, is generally most abundant, and is obtained by evaporation in the form of a fine powder.

"The shells of eggs have been analyzed by Vauquelin and Merat Guillot; but these chemists seem to have over-rated the quantity of animal matter, and of phosphate of lime contained in them. When shells which had been dried in vacuo at 212°, were dissolved in dilute muriatic acid, the quantity of animal matter obtained was only about 2 per cent.; while the quantity of phosphates of lime and of magnesia never amounted to quite 1 per cent.; the rest was carbonate of lime mixed with a little carbonate of magnesia. When burnt, egg-shells, as Vauquelin has observed, yield traces of sulphur and iron.

"The *membrana putaminis*, on the supposition that the original weight of the egg be 1000 grains, weighs, when dried in vacuo at 212°, about 2.35 grains; and, on being burnt, yields traces of phosphate of lime.

"It may be observed here that the great differences in the quantities of the earthy matter existing in the shells of different eggs, have rendered the average totally inapplicable in these experiments, as will be shown hereafter; hence a more detailed analysis of this part of the egg was deemed unnecessary."

#### *Saline Contents of the recent Egg.*

Prior to giving the results of his investigation of this part of the subject, Dr. Prout relates the manner in which his analyses were conducted, premising generally, "that all the results were

obtained by combustion; and that the following observations are to be understood as applicable to the whole of the experiments subsequently related in this inquiry."

"The *albumen* burns with difficulty, unless care be taken to remove the saline matter by frequent washings; but if this point be attended to, the whole of the carbonaceous matter may be burnt off even in a covered crucible. In the subsequent experiments, the saline and earthy matters were removed from the crucible after combustion by distilled water; a little ammonia was then added, and the whole permitted to remain at rest for 24 hours; the clear solution containing the alkaline salts was now carefully poured off, and the insoluble residuum, consisting of the phosphate of lime and triple phosphate of magnesia and ammonia, after being washed with distilled water, was dried and weighed. The alkaline solution, together with the washings of the earthy phosphates, were then evaporated to dryness, and exposed to a low red heat; and the weight of the saline residuum being accurately noticed, the whole was again dissolved in distilled water. A few drops of nitric acid being now added to neutralize the excess of alkali present, nitrate of barytes was dropped into the solution as long as any precipitate fell. The precipitate was obtained by decanting off the solution as before, and, after being well washed, its weight ascertained: from this the quantity of sulphuric acid present was determined by calculation.\* To the solution, thus freed from sulphuric acid, nitrate of barytes, and afterwards ammonia, were added. The phosphate of barytes thus obtained was collected, washed and weighed as before, and the quantity of phosphoric acid present obtained by calculation.† Nitric acid was again added in slight excess to the original solution, and nitrate of silver dropped into it as long as any precipitate fell; from the chloride of silver obtained, the quantity of chlorine present was estimated.‡ Lastly, the weights of the sulphuric and phosphoric acids and chlorine were added together, and their amount subtracted from the weight of the alkaline residuum formerly obtained by evaporation, the remainder, of course, indicated the quantity of potash and soda,§ and carbonates of potash and soda present. Finally, the proportion of the earthy phosphates to one another was determined, and the quantities of the bases and acid obtained by calculation.

"The *yolk* of the egg is exceedingly difficult of combustion; and indeed without proper precautions cannot be burnt at all, on

\* On the supposition that the weight of the atom of sulphuric acid is 50, and that of barytes 97.5, oxygen being 10.

† On the supposition that the weight of the atom of phosphoric acid is 35, that of oxygen being 10.

‡ On the supposition that the weight of the atom of chlorine is 45, and of silver 137.5, that of oxygen being 10.

§ The quantity of soda equivalent to the sodium in union with the chlorine, was determined by calculation.

account of the large quantity of phosphorus it contains ; which, by undergoing a partial combustion, forms a glassy coating that effectually excludes the contact of the air from the coal, and prevents its further combustion. After a variety of attempts, the following were the two methods employed : The yolk of an egg which had been boiled hard, and dried by exposure to the air, was rubbed in a mortar with a quantity of bicarbonate of potash. The mixture was then introduced into a platina crucible, and exposed to a strong red heat, till the flame had ceased to escape from a small hole in the lid. The crucible being now removed from the fire, its contents when cold, were again pulverised in a mortar with nitre. The mixture was then introduced a little at a time into the covered crucible till the whole was burnt. To the residuum, distilled water was added, which, of course, took up every thing but the earthy phosphates, which were separated and weighed ; while the alkaline solution, like that before mentioned, obtained from the albumen, was submitted to the action of the appropriate reagents, and thus the quantities of the different acids present ascertained. In this manner, every thing was determined, except the proportion of alkaline matter present ; and to ascertain this, other experiments with different yolks were made, in which lime and nitrate of lime were substituted for the bicarbonate and nitrate of potash."

The foregoing detail is succeeded by the statement, that the probability that the sulphuric and phosphoric acids obtained from the egg, exist in it naturally as sulphur and phosphorus, and that the metallic bases of the earthy principles "are to be considered as constituent principles of the primary animal compounds," induced the author "to state the quantities of the acids obtained separately from the bases." Dr. Prout also remarks, that as his experiments "were made almost entirely with the view of comparison only," he did not "enter into any very minute discriminations, which did not appear to be immediately necessary to his purpose."

The contents of the following table of the relative proportions of the saline principles of different eggs, were selected as examples from a variety of other analyses ; the weight of each egg being reduced to 1000 grains :

	Sulphuric acid.	Phosphoric acid.	Chlorine.	Potash, soda, and carb. of ditto.	Lime, magnesia, and carb. of ditto.
No. 1. Albumen.	0.29	0.45	0.94	2.92	0.30
Yelk . . .	0.21	3.56	0.39	0.50	0.68
Total . .	0.50	4.01	1.33	3.42	0.98
No. 2. Albumen.	0.15	0.46	0.93	2.93	0.25
Yelk . . .	0.06	3.50	0.28	0.27	0.61
Total . .	0.21	3.96	1.21	3.20	0.86
No. 3. Albumen.	0.18	0.48	0.87	2.72	0.32
Yelk . . .	0.19	4.00	0.44	0.51	0.67
Total . .	0.37	4.48	1.31	3.23	0.99*

Dr. Prout next gives the following analysis of the shell of the recent egg: The egg from which the yelk had been taken, which is the subject of the following experiment, had been boiled hard in distilled water, and the yelk, in its moist state, was found to weigh 316.5 grains. It was then partially dried by exposure to the air for several weeks; and to remove the remainder of the water was reduced to powder, and exposed to a temperature of somewhat more than 212°. The total loss of weight was 170.2 grains, which was supposed to indicate the quantity of water present. The remainder was now digested repeatedly in alcohol of specific gravity .807, till that fluid came off colourless. The residuum was perfectly white and pulverulent, and possessed many of the properties of albumen; but it differed from that principle by the large proportion of phosphorus it contained in some unknown state of combination. The alcoholic solution was of a deep-yellow colour, and deposited crystals of a sebaceous matter, and a portion of a yellow semi-fluid oil. On distilling off the alcohol, the oil was obtained in a separate state. On cooling, it became nearly solid, and weighed 91 grains. The albuminous principle above-mentioned weighed 55.3 grains. Hence this yelk consisted of

\* " Besides the above principles, iron is met with in almost all products of combustion; and the quantity in the egg, as the process of incubation proceeds, apparently increases considerably; but it was found impossible to ascertain its quantity with any degree of precision."

Water. ....	170.2
Albumen* .....	55.8
Yellow oil. ....	91.0
	<hr/>
	316.5

"But I have reason to believe that the proportions of these ingredients differ a little in different eggs."

*Experiments on the Egg at the End of the first Week of Incubation, or about the 8th Day.*

At the end of the first week, it was found, on an average, that the egg had lost about 50 grains in the 1000, and the weights of its constituent principles in their moist state, were as follows:†

Unchanged albumen. ....	232.8
Modified albumen. ....	179.8
Liq. amnii, membranes, blood vessels, &c. .	97.0
Animal .....	22.0
Yolk .....	301.3
Shell and loss .....	167.1
	<hr/>
	1000.0

Dr. Prout here makes the following brief remarks on the general phenomena presented by the different constituent principles of the egg at those periods at which it has been submitted to examination.

"It has been remarked by many observers, that soon after the process of incubation has commenced, the yolk becomes more fluid than usual; and that as the liquor amnii increases, that portion of the albumen occupying the upper and larger end of the egg, begins to assume a peculiar appearance. In the present experiments (in which the egg was always previously boiled), the liquor amnii and portion of albumen in question, at the period now under consideration, exhibited somewhat the appearance of curds and whey. Nor did the analogy consist in mere appearance; for the curdy-looking matter, which was of a yellow colour, and which I have termed *modified* albumen, resembled the curdy part of milk in its properties, so far as to contain intermixed with it an oily or butyraceous principle. A portion of this oily principle, on being separated and examined, was found to be soluble in alcohol, of a bright yellow colour; and, in short,

\* "This proportion of the albuminous principle does not differ much from that stated to exist in the yolk of the common fowl, by Mr. Hatchett. Philos. Trans. vol. cvi. p. 308."

† In the original paper, the weights of the constituents of two eggs are here given; the same is the case with the following series of experiments, and with all the remaining analyses except two; but as no striking differences are thus presented, only one of each kind is given in this abstract.

to possess all the properties of the yellow oil existing in the yelk. The yelk at this period, as before observed, has become more fluid, and appears larger, and of a paler colour than natural. Haller, indeed, asserts, that it has not increased in weight; but the above table renders the reverse very probable. These appearances of the albumen and yelk have induced most observers to believe that an interchange of principles takes place between them; while others seem to have mistaken the yellow modified albumen for the yelk itself. That an interchange of principles has taken place, at least under the above circumstances, there can be no doubt; yet the two are not indiscriminately mixed; for when the egg has been previously boiled, the yelk, though softer than natural, is nevertheless rendered of a firmer consistence than the modified albumen, and can thus be readily separated from it; there is, moreover, a distinct line of demarcation between them, arising, apparently, from the proper membrane of the yelk. Another argument in favour of the opinion of the intermixture of the albumen and yelk at this period, is derived from the following analyses of these constituent principles of the egg; from which it will be found that the quantity of the saline matter is diminished in the albumen, and increased in the yelk. It is a singular and striking fact, however, that although the oily matter of the yelk has made its way to the albumen, very little of the phosphorus, which exists in such large quantities in the yelk, has been removed with it."

	Sulphuric acid.	Phosphoric acid.	Chlorine.	Potash, soda, and carb. of ditto.	Lime, magnesia, and carb. of ditto.
Unchanged albumen.	0.13	0.27	0.19	1.03	0.18
Modified albumen, liquor amnii, animal membranes, &c. . .	0.08	0.38	0.45	1.17	0.12
Yelk: . . . . .	0.09	4.03	0.60	0.80	0.68
	0.30	4.68	1.24	3.00	0.98

The results of an analysis made on the 10th day of incubation, show, that at this period "the proportion of phosphorus is somewhat diminished in the yelk, and increased in the animal and its appendages. The chlorine and alkaline principles seem also to have diminished in the yelk and to have increased a little in the albuminous portion."

*Experiments on the Egg at the End of the second Week, or about the 15th Day of Incubation.*

The egg has now lost, upon an average, about 130 grains in

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the 1000, and the weights of its constituent principles are as  
follows :

	Grains.
Unchanged albumen. ....	175.5
Liquor amnii, membranes, &c. ....	273.5
Animal .....	70.0
Yelk .....	250.7
Shell and loss. ....	230.3
	<hr/> 1000.0

"At this period, the animal has attained a considerable size, while the albumen has become diminished in a corresponding degree. The albumen has also acquired a very firm consistence, especially when coagulated by heat.

"The liquor amnii has become more fluid, and the *modified* albumen, formerly mentioned, has very much diminished in quantity, or disappeared.\* The yelk, which at the end of the first week seemed to have increased in bulk and fluidity, has now apparently acquired its original size and consistence."

	Sulphu- ric acid.	Phos- phoric acid.	Chlorine.	Potash, so- da, and carb. ditto.	Lime, mag- nesia, and carb. of ditto.
Unchanged albumen.	0.07	0.22	0.09	0.73	0.10
Liq. amnii, mem- branes, &c. ....	0.06	0.21	0.71	0.96	0.08
Animal .....	0.06	0.23	0.09	0.46	0.27
Yelk .....	0.30	3.34	0.16	0.68	0.69
	<hr/> 0.49	<hr/> 4.00	<hr/> 1.05	<hr/> 2.83	<hr/> 1.14

From the results of an analysis made two days later, or on the 17th day of incubation, it appears that at this period the yelk has yielded some of its phosphorus and sulphur to the other principles of the egg; that the chlorine has increased in the yelk, and diminished in the other principles; that the relative proportions of alkaline bodies remain much the same as on the 15th day; and that the earths have increased in quantity a little in the yelk, and very considerably in the other principles.

*Experiments on the Egg at the End of the third Week, or at the full Period of Incubation.*

At this period an egg has lost upon an average about 160

\* "About this time Harvey, and other observers, have noticed the appearance of a curdy or coagulated substance in the œsophagus, crop, stomach, and intestines of the animal. Is this a portion of the *modified* albumen above-mentioned?"

grains in 1000; and the weights of its constituent principles in their moist state and without boiling, are as follows :

	Grains.
Residuum of albumen, membranes, &c.	29·6
Animal .....	555·1
Yelk .....	167·7
Shell and loss. ....	247·7
	<hr/> 1000·0

“ At this period all the important changes of incubation are completed. The albumen has now disappeared, or is reduced to a few dried membranes and an earthy residuum (apparently consisting of the original earthy matter of the albumen which has remained unappropriated). The yelk is considerably increased in size,\* and is taken into the abdomen of the chick; while the animal has attained a weight nearly corresponding to the original weight of the albumen, added to that lost by the yelk, *minus* the total weight sustained by the egg during incubation. The alkaline matters and chlorine which have been decreasing from the commencement of incubation, have now undergone further diminution in quantity, while *the earthy matters have increased in the most striking manner*. The other principles seem to have suffered very little change in quantity.”

	Sulphuric acid.	Phosphoric acid.	Chlorine.	Potash, soda, and carb. of ditto.	Lime, magnesia, and carb. of ditto.
Residuum of albumen, membranes, &c. ...	0·04	0·12	0·09	0·23	0·12
Animal .....	0·44	3·02	0·55	2·26	2·58
Yelk .....	0·04	1·06	0·03	0·06	1·26
	<hr/> 0·52	<hr/> 4·20	<hr/> 0·67	<hr/> 2·55	<hr/> 3·96

Dr. Prout observes, that the analyses in his paper are selected as the most perfect, from a variety of others made at each period, all of which confirm the results he has given : and he then states his conclusions from them, thus :

“ These experiments, then, demonstrate, or render probable, the following circumstances :

\* “ This has been denied or doubted by some writers, especially Haller and Dr. Martney.”

"1. That the relative weights of the constituent principles of different eggs vary very considerably.

"2. That an egg loses about one-sixth of its weight during incubation, a quantity amounting to eight times as much as it loses in the same time under ordinary circumstances.

"3. That in the earlier stages of incubation, an interchange of principles takes place between the yelk and a portion of the albumen; that this interchange is confined on the part of the yelk to a little of its oily matter, which is found mixed with the above-mentioned albumen; that this portion of albumen undergoes some remarkable changes, and is converted into a substance analogous in its appearance, as well as in some of its properties, to the curd of milk; and lastly, that a portion of the watery and saline portion of the albumen is found mixed with the yelk, which becomes thus apparently increased in size.

"4. That as incubation proceeds, the saline and watery parts again quit the yelk, which is thus reduced to its original bulk; that in the last week of the process, it undergoes still further diminution in weight, and loses the greater portion of its phosphorus, which is found in the animal converted into phosphoric acid, and in union with lime, constituting its *bony skeleton*; and lastly, that this lime does not originally exist in the recent egg, but is derived from some unknown source during the process of incubation."

Dr. Prout concludes this valuable communication with some remarks on the uses of the yelk, and the apparent generation of earthy matter. The opinion "that the yelk is analogous to the milk of viviparous animals, but more concentrated, and that its chief use is to afford a pabulum to the young animal during incubation," is, he says, "corroborated in a striking manner by the present inquiry."

"With respect to the earthy matter found in the skeleton of the chick when it quits the shell," continues Dr. P. "I think I can venture to assert, after the most patient and attentive investigation, that it does *not pre-exist in the recent egg*; certainly not, at least, in any known state. The only possible sources, therefore, whence it can be derived, are from the shell, or transmutation from other principles. Whether it be actually derived from the shell, cannot be determined by chemistry; because, as we have seen, the shells of different eggs differ so much, that the application of averages is out of the question; and we are of course precluded from ascertaining the exact quantity of lime any particular shell originally contains. There are, however, very strong reasons for believing, that the earthy matter is not derived from the shell. In the first place, the *membrana putaminis* never becomes vascular, and seems analogous to the epidermis; hence the lime of the shell, which is exterior to this membrane, is generally considered by physiologists as *extra-*

vascular; \* it is, therefore, extremely difficult to conceive how the earth in question can be introduced into the economy of the chick from this source, particularly during the last week of incubation, when a very large portion of the membranes are actually separated from the shell. Secondly, both the albumen and yolk contain, at the end of incubation, a considerable proportion of earthy matter (the yolk apparently more than it did originally); why is this not appropriated in preference to that existing in the shell? In opposition to these arguments, it will be doubtless stated, that the shell of the egg becomes brittle at the end of incubation, and appears to undergo, during that process, some other changes not at present understood. To which it may be answered, that this brittleness has been attributed to the separation of the membrana putaminis, and the exsiccation of the parts by so long an exposure to the heat necessary to the process of incubation; and in this manner all the known changes produced in the shell by incubation may, perhaps, be satisfactorily accounted for. Until, therefore, it be demonstrated that some other changes take place in the shell, I confess this argument does not seem to me to have much weight. I by no means wish, however, to be understood to assert, that the earth is not derived from the shell; because, in this case, the only alternative left me is to assert, that it is formed by transmutation from other matter; an assertion, which I confess myself not bold enough to make in the present state of our knowledge, however strongly I may be inclined to believe that, within certain limits, this power is to be ranked among the capabilities of the vital energies."

Dr. Prout has requested me to insert the following correction of a passage in his paper:—In the twenty-second page of the paper itself, or p. 398 of the *Philosophical Transactions*, line 6 from the bottom, for "after incubation," read "after it had left the egg."—*Edit.*

\* "See an essay 'On the Connexion between the Vascular and Extra-vascular Parts of Animals,' by Sir A. Carlisle.—(Thomson's *Annals*, vol. vi. p. 174)."

## ARTICLE III.

*Summary of a Meteorological Table kept at Bushey Heath during 1822.* By Col. Beaufoy, FRS.

1822.	Barom.	Ther.	Hygr.	Rain.	Evap.	Mean temp.	N	NE	E	SE	S	SW	W	NW
	Inches.			Inches.	Inches.									
Jan...	29.614	37.0	71.0	0.420	1.702	38.5	3	2	0	1	0	7	8	10
Feb...	29.612	41.5	72.0	1.080	2.118	42.6	1	1	0	4	2	16	1	3
March.	29.561	45.6	69.0	0.715	3.780	46.6	0	1	0	2	0	18	7	3
April..	29.454	46.3	64.0	2.482	3.230	47.2	0	10	1	5	0	8	0	6
May...	29.523	56.6	59.0	1.666	4.240	57.2	0	14	3	6	0	5	1	2
June..	29.636	64.3	56.0	0.780	6.520	65.3	1	11	5	2	1	6	0	4
July...	29.391	62.0	59.0	2.504	—	62.4	0	2	0	4	2	13	5	5
August	29.445	62.4	62.0	1.654	4.860	61.3	0	3	1	2	1	13	6	5
Sept...	29.503	55.3	64.0	1.060	3.760	56.1	0	14	2	1	0	6	4	3
Oct...	29.197	51.0	73.0	3.479	1.930	51.4	0	3	2	12	0	13	0	1
Nov...	29.171	45.4	78.0	3.097	1.550	46.3	0	4	0	4	0	21	1	0
Dec...	29.572	32.0	71.0	1.400	1.000	33.2	0	12	4	4	0	6	1	4
Mean.	29.482	49.9	66.6	2.037	—	50.7	5	77	18	47	6	132	34	46

Greatest height of the thermometer during the year was  $85\frac{1}{2}^{\circ}$ , June the 10th; least height,  $18\frac{1}{2}^{\circ}$ , December the 30th.

This table is similar to the one published in the *Annals of Philosophy*, Feb. 1822.

## ARTICLE IV.

*Astronomical Observations, 1822, 1823.*  
By Col. Beaufoy, FRS.

*Bushey Heath, near Stanmore.*

Latitude  $51^{\circ} 37' 44.3''$  North. Longitude West in time  $1^{\circ} 20' 93''$ .

Dec. 21. Emersion of Jupiter's third satellite.....	7 <sup>h</sup> 43' 58"	Mean Time at Bushey.
Dec. 25. Emersion of Jupiter's first satellite.....	7 45 19	Mean Time at Greenwich.
Dec. 28. Egress of Jupiter's third satellite.....	9 28 49	Mean Time at Bushey.
Dec. 28. Emersion of Jupiter's third satellite.....	9 30 10	Mean Time at Greenwich.
Dec. 29. Emersion of Jupiter's second satellite.....	8 26 28	Mean Time at Bushey.
Jan. 8. Emersion of Jupiter's first satellite.....	8 27 49	Mean Time at Greenwich.
Dec. 28. Emersion of Jupiter's third satellite.....	11 45 47	Mean Time at Bushey.
Dec. 29. Emersion of Jupiter's second satellite.....	11 47 08	Mean Time at Greenwich.
Jan. 8. Emersion of Jupiter's first satellite.....	9 36 20	Mean Time at Bushey.
	9 37 41	Mean Time at Greenwich.
	13 19 53	Mean Time at Bushey.
	13 21 14	Mean Time at Greenwich.

## ARTICLE V.

*An Account of the Process of smelting Copper as conducted at the Hafod Copper Works, near Swansea.* By John Henry Vivian, Esq. FRS. MGS.

[Having lately been consulted by Messrs. Vivian and Sons, proprietors of the Hafod Copper Works, as to the means by which the inconvenience arising from the smoke of the copper works might be remedied, the following account, forming a part of Messrs. Vivian's statement, appeared to me to possess much general interest: I, therefore, requested, and readily obtained, permission to publish it.—*Edit.*]

THE copper ores smelted in the works in South Wales are for the most part raised in the mines of Cornwall and Devon. They consist chiefly of yellow copper ore or copper pyrites, and the grey sulphuret of copper. The yellow ore is a compound of sulphur, iron, and copper, in nearly equal proportions. The grey ore, at least what is known in Cornwall under that denomination, is almost a pure sulphuret of copper containing about 80 per cent. of metal. Yellow ore, which is by far the most abundant, is usually accompanied by iron pyrites, or sulphuret of iron. The earthy minerals that occur with these metallic substances are chiefly siliceous, although in some mines the veins are of an argillaceous or clayey nature, while in others they contain fluor spar, or fluete of lime. Thus the component parts of the Cornish copper ores, as prepared for smelting, may be said to be sulphur, copper, iron, and from 60 to 70 per cent. of earthy matter. To these may be added, as accidental, tin and arsenic; for although these substances are not chemically combined with the copper, still as the ores of tin and copper frequently occur in the same vein, it is impossible to effect their complete separation by mechanical means. The quantity, however, compared with the substances above enumerated, is inconsiderable; for as the miner is not paid for the tin when contained in copper ores, it is of course his interest to separate it as clean as possible. The arsenic is derived from the arsenical pyrites which usually accompanies tin ores. The average produce in copper may be stated at  $8\frac{1}{2}$  per cent.

The ores are conveyed from Cornwall to Wales to be smelted on account of the supply of fuel, as not only carrying the smaller quantity to the greater, the ore to the coal, but because the vessels load back coal for the use of the engines of the mines. The principal smelting works are situated on the navigable rivers of

Swansea and Neath. The processes in a copper work are simple: they consist of alternate calcinations and fusions. By the former the volatile matter is expelled, and the metals previously combined with the copper oxidized, the general fusibility of the mass being thereby increased. The calcination is in fact a preparatory process to the fusion, in which the metallic oxides and earthy matters, being rendered specifically lighter than the metals, float on the surface, and are skimmed off as slags.

The furnaces in which these operations are performed are reverberatory, and of the usual construction. The substance to be acted on is placed on the body of the furnace or hearth, which is separated from the fire place by a bridge of bricks about two feet in thickness. The flame passes over this bridge, and, reverberating along the roof of the furnace, produces the required temperature, and escapes with any volatile matter that may be disengaged from the ore or metallic sulphurets through a flue at the opposite extremity of the furnace, which flue communicates with a perpendicular stack or chimney.

These furnaces are of two descriptions, varying in their dimensions and internal form. The calcining furnaces, or calciners, are furnished with four doors or openings, two on each side the furnace, for the convenience of stirring the ore, and drawing it out of the furnace when calcined. They vary in their dimensions, but are commonly from 17 to 19 feet in length from the bridge to the flue, and from 14 to 16 in width; the fire-place from 4 to 5 feet across by 3 feet.

The melting furnaces are much smaller than the calciners, not exceeding 11 or 11½ feet in length by 7½ or 8 feet in the broadest part: the fire-place is larger in proportion to the body of the furnace than in the calciner, being usually from 3½ to 4 feet across, and 3 or 3½ feet wide, as a high temperature is required to bring the substances with as little delay as possible into fusion. These furnaces have only one door, which is in the front part of the furnace. The accompanying sketches may convey some idea of the construction of these furnaces; fig. 1 being a plan of a calciner; fig. 2 of a melting furnace.

Fig. 1. Plan of a calciner. The furnace is 17 feet long, 14 feet wide at the fire-place, and 16 feet wide at the flue. The fire-place is 4 feet across by 3 feet. The flue is 14 feet long, 14 feet wide at the furnace, and 16 feet wide at the chimney. The furnace is divided into four compartments by three transverse walls. The first compartment is the fire-place, the second is the hearth, the third is the flue, and the fourth is the chimney. The furnace is built of brick, and the flue and chimney are built of stone.

Fig. 2. Plan of a melting furnace. The furnace is 11 feet long, 7½ feet wide at the fire-place, and 8 feet wide at the front. The fire-place is 3½ feet across by 3 feet. The front is 11 feet long, 11 feet wide at the furnace, and 8 feet wide at the chimney. The furnace is divided into two compartments by one transverse wall. The first compartment is the fire-place, and the second is the hearth. The furnace is built of brick, and the chimney is built of stone.

Fig. 3. Plan of a melting furnace. The furnace is 11 feet long, 7½ feet wide at the fire-place, and 8 feet wide at the front. The fire-place is 3½ feet across by 3 feet. The front is 11 feet long, 11 feet wide at the furnace, and 8 feet wide at the chimney. The furnace is divided into two compartments by one transverse wall. The first compartment is the fire-place, and the second is the hearth. The furnace is built of brick, and the chimney is built of stone.

Fig. 1.

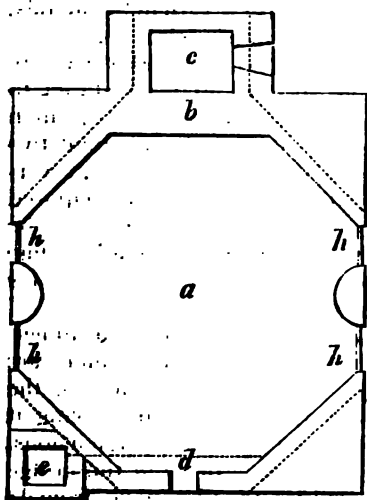
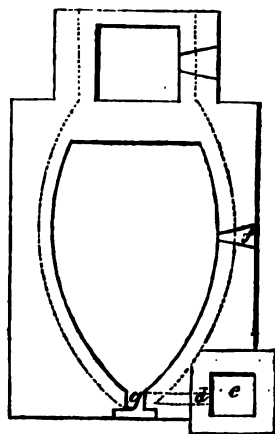


Fig. 2.



- a. Hearth or body of furnace.
- b. Bridge.
- c. Fire place.
- d. Flue.
- e. Stack, or chimney.
- f. Tapping hole.
- g. Skimming door in melting furnace.
- h h h h. Stirring doors in calciner.

The processes are conducted in the following order :

1. The copper ore calcined.
2. Calcined ore melted.
3. Coarse \* metal from Process 2, calcined.
4. Calcined coarse metal melted.
5. Fine† metal from Process 4 calcined.
6. Calcined fine metal melted.
7. Copper from Process 6 roasted.
8. Coarse or blistered copper refined.

\* Metal, means sulphuret when thus used ; and coarse metal, sulphuret of copper and iron.

† Fine metal, ditto with larger proportion of copper.

**PROCESS I.—The Calcination of the Ore.**

The copper ores, when discharged from the vessels in which they are brought from Cornwall, are wheeled into yards or plots contiguous to the works, and there deposited, one cargo over the other, so that, when cut down perpendicularly to be carried to the furnaces, a tolerably general mixture is formed of the ores of the county. This is always desirable in a smelting work, as the ores being of different qualities and component parts, the one acts as a flux for the other. A more accurate mixture, calculated from the chemical analysis of each parcel of ore, might be preferable; but this on a large scale cannot be practised, as it would be impossible to keep the ores separate, on the different wharfs in Cornwall, to ship them separately, and to have sufficient space on the wharfs at the works to receive them.

The ore in the yard is weighed over to the calciner-men in boxes, containing each one hundred weight. These are carried on men's shoulders to the calciners, and emptied into iron bins or hoppers, formed by four plates of cast iron tapering to the bottom, placed over the roof of the furnace, and supported by wrought iron frames resting on its sides. From these bins, the ore is passed into the calciner through a hole in the roof immediately below the bottom of the hopper. Two of these bins are usually placed over each calciner, and nearly opposite the side doors, so that the charge of ore, when let into the furnace, may be conveniently spread, which is done by means of long iron tools, called stirring rabbles.

This charge of ore usually consists of three to three and a half tons. It is distributed equally over the bottom of the calciner, which is made of fire bricks or square tiles. The fire is then gradually increased: so that towards the end of the process, which lasts 12 hours, the heat is as great as the ore will bear without being fused or baked together. To prevent this taking place, and to assist the disengagement of the sulphur, the ore is frequently stirred during the operation, and fresh surfaces exposed. At the expiration of the 12 hours, the charge is drawn out through holes in the bottom of the calciner, of which there is one opposite to each door, and, falling under the arch of the furnace, remains there till it is sufficiently cool to be removed, when water is thrown over it to prevent the escape of the finer particles; it is then put into barrows, and wheeled to the proper depots. In this state it is called calcined ore. If the process has been well conducted, the ore is black and powdery. The dark colour is no doubt owing to a portion of the iron being oxidized during the process, by which the ores acquire an increased degree of fusibility. The quantity of iron oxidized during the first calcination is, however, small in proportion to the contents in the ore, as the metal is protected from the action of the air by the mass of earthy matter with which it is combined, and by the sulphur

and other volatile substances. The sulphur that is expelled is in no part sublimed as flowers of sulphur, but is all acidified; and the process being conducted with admission of atmospheric air is properly a roasting.

PROCESS II.—*The Melting of the Calcined Ore.*

The calcined ore is delivered, as in the raw state, to the workmen in boxes containing 1 cwt. each. The charge is deposited in the same manner in a bin placed on the top of the furnace, and from thence passed into the interior as required. When the charge is let down and spread over the bottom, the door of the furnace is put up, and well luted. Some slags from the fusion of the coarse metal or sulphuret are added, not only on account of the copper they contain, but to assist the fusion of the ore, being chiefly composed of oxide of iron. These are thrown into the furnace through the front door.

After the furnace is charged, the fire is made up, and the main object of the smelter is to bring the substances into fusion: it is, therefore, in this respect different from the calcining process. When the ore is melted, the door of the furnace is taken down, and the liquid mass well rabbled, or stirred, so as to allow of the complete separation of the metallic particles from the slags or earthy matters, and to get the charge clear of the bottom of the furnace, which is made of sand, and soon becomes impregnated with metal. The furnace being ready; that is, the substances being in perfect fusion, the smelter takes an iron rabble, and skims off, through the front door, the sand or slags consisting of the earthy matters contained in the ore, and any metallic oxides that may have been formed, which, being specifically lighter than the metals in the state of sulphuret, float on the surface. When the metal in the furnace is freed from slags, the smelter lets down a second charge of ore, and proceeds with it in the same manner as with the first; and this he repeats until the metal collected in the bottom of the furnace is as high as the furnace will admit of without flowing out at the door, which is usually after the third charge; he then opens a hole, called the tapping hole, in the side of the furnace, through which the metal flows into an adjoining pit filled with water. It thus becomes granulated, and collects in a pan at the bottom of the cistern, which is raised by means of a crane. It is then filled into barrows, and wheeled to the place appointed for its reception.

The slags received into moulds made in sand in the front of the furnace, are removed after each charge, and wheeled out of the work to the *slag bank*, where they are broken, and carefully examined; any pieces found to contain particles of metal, are returned to the smelter to be remelted; and unless the slag is very thick and tenacious, the copper which they may contain is found at the bottom. What is clean or free of metal is rejected. These slags are composed of the earthy matters contained in the ore

and the oxides of iron and other metals that were mixed with the copper. The oxide of iron gives them a black colour. The siliceous or quartz remains in part unfused, and gives the slags a porphyritic appearance.

In this process, the copper is concentrated, and a mass of stuff with which it was combined in the ore got rid of. The granulated metal usually contains about one-third of copper. It is thus four times as rich as the ore, and must consequently have diminished in bulk in the same proportion. Its chief component parts are sulphur, copper, and iron.

The most important point to be attended to in this process is to make a fluid good mixture of earths and metals, so that the copper which is combined with the sulphur, may from its greater specific gravity, settle at the bottom, and yield clean slags. This object is effected chiefly by means of metallic oxides, the slags from Process 4, which are melted with the ore, being almost wholly black oxide of iron. When the ores are very stubborn or difficult to melt, a box of fluor spar is added to the charge, but it is not desirable to increase the bulk of matter in the furnace unless required.

The men work round the 24 hours, and commonly melt in this time five charges. Under favourable circumstances, as fusible ore, strong coal, furnace in good repair, they even do six charges. They are paid by the ton.

#### PROCESS III.—*Calcination of the Coarse Metal, the Product of the first Fusion.*

This is conducted in precisely a similar manner to the calcining of the ore. The charge is nearly of the same weight; but as it is desirable to oxidize the iron, which is more readily effected in this process than in the ore calciners, where it is protected from the action of the air by the earthy matters with which it is combined, the charge remains 24 hours in the furnace, and during that time is repeatedly stirred and turned. The heat during the first six hours should be moderate, and from that time gradually increased to the end of the operation.

#### PROCESS IV.—*Melting of the Coarse Metal after it has undergone Calcination.*

This is performed in furnaces precisely similar in construction to those in which the ore is melted; and with the calcined metal are melted some slags from the last operations in the works which contain some oxide of copper, as likewise pieces of furnace bottoms impregnated with metal, the proportion of each varying according to the stock or to the quality of the calcined metal.

The chemical effect which takes place is, that the oxide of copper in the slags becomes reduced by a portion of the sulphur which combines with the oxygen, and passes off as sulphurous acid gas, while the metal thus reduced enters into combination with the sul-

phoret. That there may be a sufficient quantity of sulphur in the furnace to promote these charges, it is sometimes necessary, when the calcined metal is in a forward state, to carry a small quantity of raw or uncalcined metal, so that a clean slag may be obtained.

The slags from this operation are skimmed off through the front door, as in the ore furnaces. They have a high specific gravity, and should be sharp, well melted, and free from metal in the body of slag. Such particles of metal as are drawn off, which, from the thinness of the slags, will commonly happen, should sink to the bottom of the slag while it is in the liquid state. These slags, as has been before observed, are melted with the ore, not only for the purpose of extracting the copper they may contain, but on account of their great fusibility, as, being composed chiefly of the black oxide of iron, they fuse readily, and act as solvents for other substances, earthy matters, &c. In some cases, the slags from the metal furnaces are melted in a distinct furnace with some small coal or carbonaceous matter, and in this case, the slags resulting therefrom are even sharper than those from the metal furnaces, they have a crystalline splendid appearance, and crystals are frequently to be observed in the interior.

The metal in the metal furnace, after the slag is skimmed off, is either tapped into water, as from the ore furnaces, or into sand beds, according to the mode of treatment it is to be subjected to in subsequent operations. In the granulated state, it is called fine metal; in the solid form, blue metal, from the colour of its surface. The former is practised when the metal is to be brought forward by calcination. Its produce in fine copper is about 60 per cent.

#### PROCESS V.—*Calcination of the Fine Metal.*

This is performed in the same manner as the calcination of the coarse metal.

#### PROCESS VI.—*Melting of the calcined Fine Metal.*

This is performed in the same manner as the melting of the coarse metal; the resulting product is a coarse copper from 80 to 90 per cent. of pure metal.

#### PROCESS VII.—*Roasting.*

This is chiefly an oxidizing process. It is performed in furnaces of the same description as the melting furnaces, although distinguished by the appellation of roasters.

The pigs of coarse copper from the last process are filled into the furnace, and exposed to the action of the air, which draws through the furnace at a great heat; the temperature is gradually increased to the melting point, and the expulsion of the volatile substances that remained is thus completed, and the iron or

other metals still combined with the copper are oxidized. The charge is from 25 to 30 cwt. The metal is fused towards the end of the operation, which is continued for 12 or 24 hours, according to the state of forwardness when filled into the furnace, and is tapped into sand beds. The pigs, are covered with black blisters, and the copper in this state is known by the name of blistered copper. In the interior of the pigs, the metal has a porous honeycombed appearance, occasioned by the gas formed during the ebullition which takes place in the sand beds on tapping. It is in this state fit for the refinery, the copper being freed from nearly all the sulphur, iron, and other substances, with which it was combined.

Another mode of forwarding the metal for the refinery, still practised in some works, is by repeated roastings from the state of blue or fine metal; this, however, is a more tedious method of proceeding.

The oxidizing processes, whether conducted in a calciner or furnace, depending on the admission of atmospheric air into the body of the furnace, are greatly assisted by a patent, the exclusive right to which we purchased of Mr. Sheffield, the inventor, by which a constant stream of fresh air is kept passing over the metal. This is effected by means of a channel formed in the middle of the bridge, communicating with the external air at its two extremities, and with the interior of the furnace by square holes formed in the bridge at right angles with the channel. This has a very powerful effect in forwarding the process, and not only promotes the oxidation of the metals, but has the effect of igniting and consuming the coal smoke, assists in the disengagement of the sulphur, and, by keeping the bridge cool, admits of a more regular heat throughout the furnace.

#### PROCESS VIII.—*Refining or Toughening.*

The refining furnace is similar in construction to the melting furnaces, and differs only in the arrangement of the bottom, which, is made of sand, and laid with an inclination to the front door instead of to one side, as is the case in those furnaces in which the metal is flowed out; the refined copper being taken out in ladles from a pool formed in the bottom near the front door. The pigs from the roasters are filled into the furnace through a large door in the side. The heat at first is moderate, so as to complete the roasting or oxidizing process, should the copper not be quite fine. After the charge is run down, and there is a good heat on the furnace, the front door is taken down, and the slags skimmed off; an assay is then taken out by the refiner with a small ladle, and broken in the vice; and from the general appearance of the metal in and out of the furnace, the state of the fire, &c. he judges whether the toughening process may be proceeded with, and can form some opinion as to the quantity of poles and charcoal that will be required

to render it malleable, or, as it is termed, to bring it to the *proper pitch*. The copper in this state is what is termed *dry*. It is brittle, is of a deep-red colour inclining to purple, an open grain, and a crystalline structure. In the process of toughening, the surface of the metal in the furnace is first well covered with charcoal. A pole commonly of birch is then held in the liquid metal, which causes considerable ebullition, owing to the evolution of gaseous matter, and this operation of *poling* is continued, adding occasionally fresh charcoal, so that the surface of the metal may be kept covered until from the assays which the refiner from time to time takes, he perceives the grain, which gradually becomes finer, is perfectly closed, so as even to assume a silky polished appearance in the assays when half cut through and broken, and is become of a light-red colour. He then makes further trial of its malleability by taking out a small quantity in a ladle, and pouring it into an iron mould, and when *set*, beating it out while hot on the anvil with a sledge. If it is soft under the hammer, and does not crack at the edges, he is satisfied of its malleability, or as they term it, that it is *in its proper place*, and directs the men to lade it out, which they do in iron ladles coated with clay, pouring it into pots or moulds of the size required by the manufacturer. The usual size of the cakes for common purposes is 12 inches wide by 18 in length.

The process of refining or toughening copper is a delicate operation, requiring great care and attention on the part of the refiner to keep the metal in the malleable state. Its surface should be kept covered with charcoal, otherwise it will go back between the rounds of lading, the cakes being allowed to cool in the pot, and others laded thereon. In this case, fresh poling must be had recourse to: over-poling is to be guarded against, as the metal is rendered thereby even more brittle than when in the dry state. Its colour becomes a light yellowish-red; its structure fibrous. When this is found to be the case, or as they say, *gone too far*, the refiner directs the charcoal to be drawn off the surface of the metal, and thus by taking down the side door, and exposing the copper to the action of the air, it is brought back to its proper pitch; that is, it again becomes malleable.

Are we to conclude from this, that copper in its dry state is combined with a minute portion of oxygen? or that some oxide of copper is diffused through, or combined with, the metal; that it is deprived of this by poling, and is then rendered malleable; and that, when gone too far, it is combined with a minute portion of carbon; that, like iron, either substance will render it brittle, and that it is only malleable in a certain intermediate state when free from both carbon and oxygen? \* Or is the effect of the pole merely

\* Another indication of the presence of oxygen in the dry copper, is afforded by the extraordinary action which it has upon the iron tools; they become bright, like iron in a smith's forge, and are consumed much more rapidly than when the copper is in a malleable state.

mechanical, that of closing the grain, and altering the texture of structure of the metal?

It is a remarkable circumstance that when copper is gone too far, it oxidizes slowly on the surface, thus strengthening the supposition of its being combined in that state with carbon, as that substance, from its union with the oxygen of the air, would prevent the oxidation of the metal. The surface of the liquid metal in the furnace is also more than usually splendid when over-poled, reflecting every brick in the roof, being then quite free from oxide.

Sometimes when copper is difficult to refine, a few pounds of pig lead are added to the charges of copper. The lead acts as a purifier, by assisting, on being oxidized itself, the oxidation of the iron or any metal that may remain combined with the copper, and not, as may be supposed, by uniting with the copper, and thereby increasing its malleability. This is a mistaken notion, and may lead to further embarrassment, as the smallest portion of lead combined with copper renders the metal difficult to *pickle*, or clean from oxide, when manufactured, as the scale or oxide will not rise clean from the surface of the sheets. The lead should be added immediately on the door being taken down preparatory to skimming, and the copper should be well rabbled, and exposed to the action of the air, so as to promote the entire oxidation of the lead.

Copper for brass making is granulated that its surface may be increased, so as to combine more readily with the zinc, or calamine.

This is effected by pouring the metal from the ladles in which it is taken out of the furnace into a large ladle pierced in the bottom with holes, and supported over a cistern of water. The water may be either hot or cold, according to the form to be given to the metal. When warm, the copper assumes a round form, and is called *bean shot*. When a constant supply of cold water is kept up, the metal has a light ragged appearance, and is called *feathered shot*. The former is the state in which it is prepared for brass wire-making.

Another form into which copper is cast, chiefly for exports to the East Indies, is in pieces of the length of six inches, and weighing about eight ounces each. These are called *Japan copper*. The copper is dropped from the moulds immediately on its becoming solid, into a cistern of cold water, and thus, by a slight oxidation of the metal, the sticks of copper acquire a rich red colour on the surface.

The charge of copper in the refining furnaces at Hafod is from three to five tons. The quantity of pure copper made weekly in the Hafod works, is from 40 to 50 tons. In the 12 months ending June 30, 1822, the purchases of ore amounted to 24,400 tons, containing 2144 tons of pure copper.

Contiguous to the Hafod smelting work is a powerful rolling mill.

It is worked by a steam-engine of 40 inch cylinder, and contains four pair of rolls. The cakes of copper are here manufactured into sheets and sheathings for export and home consumption. It is packed into cases which are lowered from the mills into vessels, and forwarded to the different markets. The *shruff*, that is, the edges (cut off on trimming the sheets), and the *pickle dust*, or oxide of copper, that is collected in the cisterns on clearing them, are sent back to the refinery, and remelted.

In other mills situated on the river about two miles above these smelting works, and worked by water wheels, are also two pairs of rolls for sheathing copper; a pair of rolls for cold rolled sheathing; a pair of bolt rolls, and two hammers. And at Hafod is a nail manufactory for casting mixed metal nails and spikes of all descriptions, brasses for engines, &c.

These establishments contain 84 furnaces; and are lighted by gas lights, the operations being continued day and night.

In these works with the engine, and the shipping dependent on them, from 1400 to 1500 tons of coal are consumed weekly, affording employment to nearly 1000 people; or support to 3000 in family, and producing a revenue to the port of Swansea from 400*l.* to 500*l.* per annum, causing a circulation in the country of 1000*l.* a week; in fact, we need only look to the rapid rise and prosperous situation of the town of Swansea, to see at once the manifold advantages of the smelting establishments to the neighbourhood. At the period of the establishment of the first copper work on the Swansea river, about a century ago, Swansea was a mere insignificant village. In 1801 its population amounted to 6099, and in 1821 to 10,255, making an increase in the last 20 years of 4156.

The trade of the port has increased to such an extent that the number of vessels entering its harbour now amount to 2600 annually, producing a great revenue for the improvement of the navigation, and employing a very considerable number of seamen. Reckoning 10 voyages a year to each vessel, the copper trade would require constantly upwards of 100 sail, of 100 tons each. The Swansea market is frequented from the country for the distance of 15 to 20 miles, such is the demand for agricultural produce of all sorts; and land in the neighbourhood, owing to the wealth and prosperity of the town, lets for double its real agricultural value. The current expenditure of the smelting works in South Wales cannot be less than 200,000*l.* and their consumption and export of coal upwards of 200,000 chaldrons; and in Cornwall from 50,000 to 60,000 souls are dependent on the mines.

It appears from the statement in the second volume of the Transactions of the Royal Geological Society of Cornwall, that

the produce of the different copper mining districts of the kingdom, in the 12 months ending in June, 1822, was as follows:

Cornwall .....	9331 tons.
Devonshire .....	537
Staffordshire. ....	38
Anglesea .....	738
Other parts of North Wales ....	55
Scotland .....	11
Ireland .....	738

11042\*

purchased by the following companies:

Vivian and Sons .....	2145 tons.
Williams, Grenfells, and Co. ....	2103
Daniell and Co. ....	1639
Crown Company .....	1257
Birmingham Company .....	1042
English Company .....	616
Fox, Williams, and Co. ....	580
Freeman and Co .....	504
Mines Royal Company .....	320
Rose Company. ....	98
Anglesea Company, smelted ....	738

11042\*

## ARTICLE VI.

*Examination of Mumia, &c.* By the Rev. J. J. Conybeare, MGS.

(To the Editor of the *Annals of Philosophy*.)

MY DEAR SIR,

Bath Easton, Jan. 14, 1823.

It is well known, that the substance found in the interior of Egyptian mummies, and thence termed *mumia*, once obtained not only a place, but a high reputation, in the *Materia Medica*. Its use was not entirely exploded so late as the days of Neuman, who justly ridicules the folly of such as believed it possessed of any virtues beyond those of other and *cleaner* preparations of a resinous and aromatic nature. Neuman too, was, as far as I am aware, the last person who subjected it to chemical examination.

\* There is an error in this account; the amount is 11448, instead of 11042.—*Edit.*

He obtained from four ounces of this substance

	Ounce.	Drams.	Scruple.
Fixed salt . . . . .	0	1	0
Gummy extract. ....	0	5	1
Resinous ditto. ....	1	3	1

The remainder (nearly two ounces) he considered as insoluble, and as composed (as I gather from a foregoing paragraph) of asphaltum and earthy matter. (See Neuman's Chemistry, p. 552.) Some later authors appear to suppose the *mumia* to consist entirely of asphaltum. (See Thomson's Chemistry, vol. ii.) Having obtained a portion of this substance taken from the skull of a mummy, and unquestionably genuine, I submitted it to a careful examination, and obtained the following results :

1. By digestion in water, it affords a small portion of extractive matter, of a dark reddish-brown colour, which, during evaporation by boiling, emits a disagreeable and somewhat urinous smell, readily oxidates, and thus becomes only partially soluble. The aqueous solution contains also a very minute portion of carbonate of soda.

2. Alcohol digested on the mass, previously acted on by water, dissolves a considerable portion. The solution is of a dark-brown colour; when mixed with water, it becomes turbid, and of a much lighter hue. No precipitate falls, nor does any alteration take place (except by evaporation) in the emulsion, if it may be so termed, thus produced. Evaporated to dryness in a glass vessel, the alcoholic solution leaves a residuum of a dark-brown colour, readily fusible by heat, semitransparent, of a glossy lustre, and powerful odour. This portion, therefore, may be considered as consisting of one, or perhaps more, of the gum-resins.

3. The portion which resisted the action of alcohol dissolved readily, and almost entirely in sulphuric ether. The solution was of a much deeper and blacker-brown than No. 2, and gave by evaporation a copious residuum, which, when dried, had all the characters of asphaltum.

4. There remained yet undissolved a very small portion of carbonaceous, and, perhaps, earthy matter, in which the lens detected some minute fibres not unlike those of decayed vegetables. As the mixture is evidently mechanical, and as I found by even rude experiments of comparison that of its principal ingredients (the resinous and bituminous), sometimes the one, and sometimes the other, was in excess, I have not attempted to give their proportions in numbers.

The *mumia* examined was taken, as before stated, from the interior of a skull. Herodotus informs us, that the so filling the skull (previously emptied of the brain) formed a part of that only which was deemed the most sumptuous mode of embalming.

The specimen, therefore, may be regarded as *mumia* in its most perfect state. It appears to differ from that examined by Neuman chiefly (supposing that I am right in considering his residuum of two ounces as asphaltum), in the small proportion which it gives of saline and extractive matter. It is probable that a mass taken from any other part of the body might retain the salt originally added for the purpose of preservation, and a greater proportion of decayed animal or vegetable matter, either of which would probably afford an extractive soluble in water. The carbonate of soda obtained in the present case could not have exceeded the proportion in which Neuman states himself to have found an alkaline salt in native asphaltum (seven grains in two ounces). Herodotus (it may be added), does not describe the substance used for filling the scull; he simply says, they injected certain *drugs* (*φαρμακα*). The compound used for filling the body, he states to have consisted of myrrh, cassia, and other aromatics. His editor Wesseling (who yet wrote after the publication of Neuman's works), has a note which insinuates a doubt of the accuracy of the historian on the authority of Nardius, and other early writers, who affirmed that they found nothing in the mummies which they examined but masses of bitumen. They were probably deceived by its external appearance, and unacquainted with the method of ascertaining its constituent parts. It is remarkable that Herodotus does not specify the use of bitumen, unless we regard him as including it in the general term aromatics (*Θυματα*). *Mumia*, though long since discarded from the Pharmacopœia, has, I believe, retained some value as a pigment, especially with those artists who are somewhat of dilettanti in the choice of their materials. I am not sufficiently acquainted with the practical part of oil-painting to say with what justice. It may possibly afford a somewhat richer brown than the common asphaltum.

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I have during the last week had an opportunity of ascertaining the indestructibility of common amber. A bead of that substance dug from a British tumulus on Mendip, where it must have remained on a low computation for 1600 years, though rifty, and covered externally with a thin crust nearly opaque, yet retains in the rest of the mass its peculiar fracture and transparency, and when exposed to the action of sulphuric ether, is dissolved as readily as specimens recently dug, and more carefully preserved.

Having obtained through the kindness of a friend (T. Grimes, Esq.) a further portion of the bituminous substance found near Merthyr Tydvil, to which I have ventured to give the name of

Hatchetine,\* I am confirmed in my opinion as to its differing specifically from every variety of bitumen yet described. As in its external aspect, it partially resembles the naphthaline obtained by Dr. Kidd,† I wished to ascertain whether it resembled that substance in one of its most striking empirical characters. I made, therefore, a small candle by covering a cotton wick with hatchetine, softened by the heat of a warm hand. On lighting it, I found that instead of producing the bulky smoke which issues from a candle of naphthaline, it gave a remarkably clear and bright light, and when blown out exhibited no trace of the beautiful phenomena described by Dr. K.

Sulphuric ether dissolves it, as I have before stated, entirely; but somewhat sparingly. If thus treated in somewhat larger portions than the solvent is capable of taking up, it first separates into small laminæ of great tenuity and brilliancy, not unlike the scales scraped from the bleak for the purpose of manufacturing artificial pearls. After a time, these increase in bulk, lose their brilliancy, and concrete by uniting with a portion of the ether into a light mass, much resembling common opodeldoc. The increase in bulk is very striking, especially in a substance already so light as hatchetine; it must at least be as 40 to 1. The mixture is now found to contain two distinct compounds; the one a fluid solution of hatchetine in ether; the other, a combination of the same substances in a more solid form. The addition of water to the ethereal solution separates the hatchetine as a thin greasy pellicle, transparent, and of a consistency between that of oil and tallow. Neither by this process nor by evaporation have I ever found it to reassume its flaky or semicrystalline aspect.

I am, my dear Sir, very truly yours,

J. J. CONYBEARE.

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## ARTICLE VII.

*On the Origin of the Accumulations of Bones in the Caves of the Vale of Pickering, in Yorkshire, and other Places.* By G. Cumberland, Esq. MGS.

(To the Editor of the *Annals of Philosophy*.)

SIR,

Bristol, Dec. 10, 1822.

MUCH has been ingeniously written and reviewed on the subject of the accumulation of bones of various animals found in the caves in the vale of Pickering, in Yorkshire, and the probable mode in which they were brought there; but the general dispo-

\* See *Annals of Philosophy* for Feb. 1821.

† See *Philosophical Transactions* for 1821.

sition to believe it to have been the work of the hyæna, I am free to confess does not satisfy me, since there is a much easier way to account for it, founded on a great event, now, I believe, disputed by no party of geologists whatever, and a strong proof of which we may acquire from the limestone caves near Plymouth, if any were wanted.

Wherever there are limestone rocks, or any usually termed secondary, immense irregular cavities are found, whose forms distinctly point out their origin to be *from subsidence*, and these are so numerous in Yorkshire, Derbyshire, and Somersetshire, that it is needless to insist on them. Those of the Peak, Pool's Hole, and those near Wells, are well known; but smaller ones are, in the neighbourhood of Bristol, continually opened by our quarrymen; and I have witnessed several, now broken up, particularly one at Redland, of some hundreds of feet in length, at different levels, yet connected by narrow passages with many smaller ones; the whole resembling, when the section was laid open, those settlements we often find in ancient castle walls, when undermined and sinking to decay.

These short of cavities are called in the west of England *swallowits*, from their being the conveyers of land-waters to the interior reservoirs in the limestone hills, and usually commence with a funnel-shaped cavity on the levels on the tops of the hills, and thus the great spring at Cheddar is fed, which, in the autumn, bursts out from overflowing reservoirs: thus decidedly proving that great and terrible subsidences have occurred at some time or other, among these stratified hills, whose interstices probably were once nearly horizontal, resting on intervening clay, marl, or schistous clay, the material of which was no doubt the body that helped to launch them from their original position when first sapped by that fluid which once covered the earth, and by its lateral and downward pressure produced those effects which must have been the cause of a considerable change in the position of the rocks, such as is made evident on the sides of the Avon, near Bristol.

Now, Sir, I apprehend what the quarrymen did at *Oreston*, near Plymouth, viz. opening a way into one of these cavities by art, nature had done in the vale of Pickering by the action of the retreating waters of the great Noahitic flood, which undoubtedly must have torn away many large fragments of the sides of the hills in its passage downwards; and hence the discovery of this winding cave, so common in limestone hills. Thus much for the facts of the *two* caves. And it so happens, that in the cave, one mile from Plymouth, so effectually examined by Mr. Joseph Coldle of this place, there were found (and I have seen them) teeth of horses in abundance, those of deer, of several species of kine, many connected with their jaws, several specimens of wolves' and hyænas', a few of tigers', and of other animals a great number, some resembling otters'; they are also

of great variety of sizes, and many much worn down, of which he will no doubt, by and by, give a particular account to the public. Suffice it for our purpose, that these and an immense number of bones of horses, many of the legs complete, though small, were all found enclosed in a cavern that might almost have been said to have been *hermetically sealed* until the quarryers broke into it on cutting down a wall of limestone for masses to complete the Breakwater. We see, therefore, that these could not have been brought there by either wolves or hyænas, as all were involved in one common ruin when the hill closed on them at its subsidence; and, like the Yorkshire cave, this had at its bottom mud in abundance, by carefully searching into which Mr. Cottle disclosed these interesting remains, after some of the larger bones on the surface had been extracted by earlier visitors.

Among the teeth and bones, many are very well preserved; even many teeth have their enamel perfect, especially those of the tiger and the hyæna; and many of the bones are so saturated with the matter of stalactite as to be almost fossil.

From all these circumstances, it is plain that these animals were at some time congregated in this cave for security (for only necessity could have brought that about on account of their very different habits), and it seems probable to me, that to this circumstance alone we owe these great monuments of the Noahtic flood; for all bones that have not been so preserved must inevitably have partaken of that friction which we see has even rounded the quartz fragments, and left only the debris of tusks and teeth, and other deposits, among the alluvial gravels; and as the remains are quite similar in both the caves, I think we may be allowed to conclude, that both assemblages of animals had once been in a similar situation; that is, *totally enclosed by the subsidence of the rocks under which they had taken shelter from the rising waters at the deluge*. Much stress has been laid on the bones having been gnawed in the Yorkshire cave; but that, I think, can only prove that some outlived the others; and, being shut up, made their last meals on them; as rats, it is known, will do, when long enclosed in a vessel without other food than their own species. And as to the dung of hyænas, dogs, or wolves, found in the Yorkshire caves, that would naturally have been preserved where bones were not decomposed, being chiefly, if not altogether, a phosphate of lime, I believe. With the greatest respect for the opinions of the gentlemen who have given us their very plausible conjectures on the first cave, I take the liberty to request your publication of mine; for truth must be the object of us all.

Yours, &c.

G. CUMBERLAND.

P.S. Animal matter of half an inch in thickness, it is said, covered the surface of the mud, which was about two feet deep; but none of this has been preserved for examination.

*New Series*, VOL. V.

K

## ARTICLE VIII.

*Account of a new Mineral.* By Mr. A. Levy, MA. of the University of Paris.

(To the Editor of the *Annals of Philosophy*.)

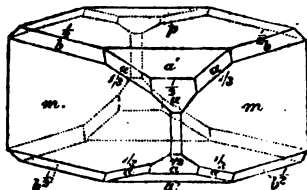
SIR,

Great Russell-street, Bloomsbury, Jan. 23, 1823.

IN the select collection of minerals lately Mr. Heuland's small private collection, but now the property of C. H. Turner, Esq. I found two specimens from the Seisser Alpe in the Tyrol, containing small brilliant transparent crystals, which were described as datolite; but which, from their crystallographical characters, ought, I believe, to be separated from that mineral species.

In order that the difference between the two substances may be the better shown, I shall first describe one of the new varieties of datolite which I have observed in the same collection. This variety is represented by fig. 2, and indicates for the primitive form a right rhombic prism, the lateral planes of which would correspond to the faces marked *m*, and the base to the face *p*. Moreover, in

Fig. 2.



order to make the face  $\frac{1}{2}a$ , the result of a simple decrement on the lateral angles of this prism, it is necessary to determine its height by assuming that the face *a'* is produced by a decrement by one row on the obtuse angle of the base. If the height were to be determined by any other condition, it would make the faces  $\frac{1}{2}a$  the result of an intermediary decrement. The dimensions of the right rhombic prism being thus determined, not only the modifications of the crystal represented in fig. 2, but all that I have observed, result from very simple decrements on its different parts; there can be no objection, therefore, to consider it as the primitive form. Now I find, by the reflective goniometer, that the incidence of

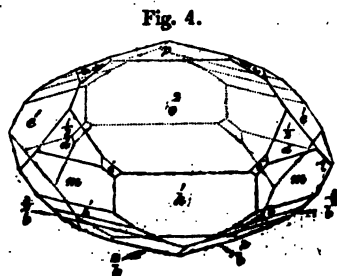
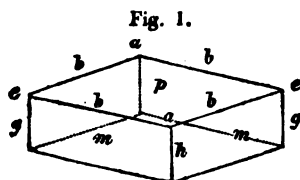
P on <i>m</i> is. ....	90°	0'
<i>m</i> on <i>m</i> . ....	103	25
<i>a'</i> on <i>p</i> . ....	147	46

The first of these incidences was to be expected from the law of symmetry: it results from the other two, that the primi-

tive form of datolite may be considered as a right rhombic prism, fig. 1, of  $103^{\circ} 25'$ , in which one side of the base is to the height, in the ratio of sec.  $51^{\circ} 42' 30''$  to tang.  $32^{\circ} 14'$ , or, nearly, as 13 to 15. Haüy, in the new edition of his *Traité de Minéralogie*, has preserved the determination he had previously given of the primitive form of datolite. It is, according to him, a right rhombic prism of  $109^{\circ} 28'$ , in which the length of one side of the base is to the height in the ratio of 3 to  $\sqrt{10}$ . This determination differs from the preceding by  $6^{\circ} 3'$  in the incidence of the lateral planes. It is not likely, therefore, he had measured the same angle as I have. I suppose that the lateral planes of his primitive form are those of some modification of the one I have adopted, composed of two pairs of parallel planes, and must consequently be the result of some decrement either on the angles of the base, or on the lateral edges. This modification, two planes of which should be inclined at an angle of  $109^{\circ} 28'$ , I have never observed, nor does any simple law of decrement give any thing very near it.

Prof. Mohs in his *Natural Historical System of Mineralogy*, has called the crystallization of this mineral *hemi-prismatic*, and as this denomination is applied only to those substances, the crystalline forms of which may be conceived to be derived from an oblique rhombic prism, it might be inferred that he does not consider the primitive form of datolite to be a right rhombic prism. But it appears to me that this inference would be in contradiction with the incidences he has given for the faces of the octohedron he takes for the *ground* form of this substance, since they are calculated in the hypothesis the result of a decrement by one row on the edges of the base of the primitive form adopted by Haüy. However, there can be no doubt, I presume, from what I have stated, that the forms of datolite can be derived from a right rhombic prism.

I shall now describe the crystals from the Tyrol which I have already mentioned. The greater number of them have the form represented by fig. 4, which is obviously a rhombic prism, the edges and angles of the base of which are not similarly modified. Consistently with the law of symmetry, this form cannot be derived, like those of datolite, from a right rhombic prism, but only from an oblique one; and, therefore, according to the



principles of classification generally adopted, these crystals should form a distinct mineral species.

The face marked  $p$  in fig. 4, is always very small; and in the crystals I have measured, it is entirely obliterated. The angles which I could measure with the greatest accuracy, were the incidences of  $m$  on  $m$ , that of  $d^{\frac{1}{2}}$  on  $m$ , and that of  $e'$  on the similar face below. I could, besides, deduce from my measurements, that, supposing the faces  $m$ ,  $m$ , to correspond to the lateral faces of the primitive, the plane  $p$  to the base of it, and  $e'$  to be the result of a decrement by one row on the angles  $e$  of the base, then  $d^{\frac{1}{2}}$  might be considered as produced by a decrement by two rows in height on the edges  $d$ . These were the data from which I had to determine the dimensions of the primitive form, as the other incidences that I measured could not be relied upon. For this purpose I made use of the following formulæ relative to an oblique rhombic prism; the investigation of which presents no difficulty.

$$x = \frac{h \sin. (p, m) \cdot \sin. (d^{\frac{1}{2}}, m)}{\sin. (m, m) \cdot \sin. (d^{\frac{1}{2}}, p)}$$

$$y = \frac{h \sin. (p, m)}{\sin. \frac{1}{2} (m, m) \cdot \tan. (e', p)}$$

$h$  is the length of the lateral edge, one side of the base being supposed to be equal to one, and  $(m, m)$ ,  $(p, m)$ ,  $(d^{\frac{1}{2}}, m)$ ,  $(e', p)$ , denote, respectively, the incidences of  $m$  on  $m$ , of  $p$  on  $m$ , of a face produced by  $x$  rows in breadth on the edge  $d$  of the base on  $m$ , and of a face produced by  $y$  rows in breadth on the angle  $e$  of the base on  $p^*$ . To apply these formulæ to the present case, it is sufficient to observe, that here  $x = \frac{1}{2}$  and  $y = 1$ .

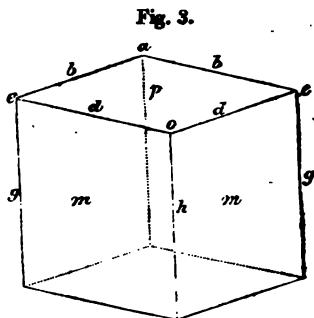
Substituting these values in the two preceding formulæ, and dividing the one by the other, there will arise

$$\tan. (e', p), \sin. (d^{\frac{1}{2}}, m), = \cos. \frac{1}{2} (m, m), \sin. (d^{\frac{1}{2}}, p).$$

The only unknown quantity in this equation is the angle  $(d^{\frac{1}{2}}, p)$ , as the others are precisely those which I could measure with accuracy. The angle  $(e', p)$ , is not the immediate result of observation, but is evidently the supplement of half the incidence of  $e'$  on the similar face below it. The angle  $(d^{\frac{1}{2}}, p)$ , will, therefore, be easily calculated, and by subtracting  $180^\circ$  from the sum of the two angles  $(d^{\frac{1}{2}}, p)$ ,  $(d^{\frac{1}{2}}, m)$ , the value of the angle  $(p, m)$ , will be obtained. This last angle being known, the first

\* I think that this notation, which consists in denoting the incidence of two faces, by placing their crystallographical signs, separated by a comma, between two brackets, if generally used, would considerably abridge the language of crystallography. I believe it will also be found very convenient to write, as I have done in figs. 2 and 4, on each face of the drawing of a crystal, its crystallographical sign. It is certainly preferable to the method used by Hally and the Count de Bournon; in which each new modification is designated by a new sign which has no reference to the decrement that produces it.

formula will give the value of  $h$ , and thus the dimensions of the primitive form will be completely determined. There will then be no difficulty in calculating from the measured incidences of the other faces, either on  $m$ , or some other known plane, the indices of the decrements from which they are derived. Thus I find that the primitive form may be supposed to be an oblique rhombic prism, fig. 3, in which the incidence of  $m$  on  $m$  is  $115^{\circ} 45'$ , that of  $m$  on  $p$   $91^{\circ} 25'$ , and one side of the base is to the lateral edge nearly in the ratio of 15 to 16. The letters placed on the faces of the crystal, represented in fig. 4, indicate respectively the decrements which produce them, and their incidences on  $p$  and  $m$  are as follows :



Incidence of $h^1$ on $p$ . . . . .	$91^{\circ}$	$41'$	$30''$
$e^1$ on $p$ . . . . .	128	45	0
$e^2$ on $p$ . . . . .	148	19	0
$o^2$ on $p$ . . . . .	138	3	0
$d^{\frac{1}{2}}$ on $m$ . . . . .	157	6	30
$b^1$ on $m$ . . . . .	138	55	10
$b^{\frac{3}{2}}$ on $m$ . . . . .	127	6	30
$b^2$ on $m$ . . . . .	119	13	0

I was unable to measure the faces marked  $i$  in the drawing : they result from an intermediary decrement. The intersections of these faces with the two faces  $m$  and  $o^2$  appear to be parallel. Therefore, by supposing the face  $o^2$  the result of a decrement by one row instead of two, the faces  $i$  would be produced by simple decrements on the lateral angles  $o$  of the prism. In this hypothesis, the ratio of  $h$  to  $d$  would be half what I have taken it, and the indices of all the modifications observed, except that of  $h'$  should also be divided by 2. The oblique rhombic prism, the dimensions of which have just been stated, does not possess the property which, according to Häuy, belongs to all primitive forms of this kind, and which consists in their having such dimensions that the diagonal joining the solid angle  $o$ , fig. 3, with its opposite, is perpendicular to the edge  $h$ . But in the present case, the incidence of  $p$  on  $m$  being nearly  $90^{\circ}$ , it is very obvious that if the ratio between  $h$  and one side of the base of the prism were determined by such a condition, it would be considerably less than I have made it ; and consequently that even on the supposition that the observed modifications could be

derived from a prism with such dimensions, there can be no doubt but that the indices of their decrements would be very complicated, when compared with those resulting from the ratio of the edge  $h$  to the edge  $d$ , which I have assumed. Thus, although the alleged property of oblique rhombic prisms may really exist in the primitive forms of some substances, yet this example proves that it is not of universal occurrence.

I am sorry that I cannot add to the determination of the primitive form of this substance, a satisfactory account of its physical and chemical characters, but the quantity of it which I could procure was so small, that no very decisive experiments upon it could be made. Dr. Wollaston, however, to whom I never applied in vain for assistance, was so kind as to examine a very minute quantity of it; and he found it to contain the same principles as datolite. It still remains to determine in what proportion these are combined, if in the same as in datolite, or as in botryolite, or whether it differs from both in this respect.

The mineral is sufficiently hard to scratch fluor spar, but it will not scratch glass. I found an indication of cleavage, parallel to a plane passing through the small diagonals of the bases of the primitive prism. In one of the two specimens in which I have observed it, it is mixed with carbonate of lime; and in the other, with the same substance, and with small flat crystals of apophyllite.

Should it be found necessary to designate this substance by a new appellation, when an exact and complete chemical analysis of it shall have been made, I would propose to call it Humboldtite, from that eminent philosopher to whom natural science is so much indebted; and I hope that he would see, in the liberty I had taken of associating his name with this new mineral, no other motive, than my desire to evince the respect and gratitude I have for him.\*

I am, Sir, yours, &c.

A. LEVY.

\* It may be useful to remark, that the name of Humboldt is already recorded in mineralogical nomenclature; for the protoxalate of iron found by M. Breithaupt in friable lignite at Kolowserux, in Bohemia, has been denominated *Humboldtine* by its analyst, M. de Rivero.—(See Ann. de Chim. xviii. 207, or Journal of Science, &c. xii. 422.)—*Edit.*

## ARTICLE IX.

*Memoir illustrative of a general Geological Map of the principal Mountain Chains of Europe.* By the Rev. W. D. Conybeare, FRS. &c.

(Continued from p. 16.)

[Some copies of the map having been, in the haste of publication, incorrectly coloured, the reader is requested to compare the colours with the dotted outlines. Where they do not coincide, the former are erroneous; the errors are generally of a nature which will admit a ready correction by these means. It will be seen that some parts of the present article are taken from a former publication by the same author.]

*Carboniferous Series.*

The carboniferous series, considered in the largest acceptance of the term, comprises, besides the coal measures properly so called, certain other formations of limestone and sandstone, so associated with them, that the attempt to separate them would materially interfere with the clearness of a natural division.

1. The coal measures, properly so called, consist of numerous alternations of seams of coal with beds of slate clay and sandstone.

2. These repose on an alternating system of shale and grit, in which the peculiar limestone characterising the coal districts makes its first appearance in any considerable quantity. The coal seams here become few and unimportant.

3. A system succeeds characterised by the predominance of the limestone called, from its relation to the coal fields, carboniferous. The coal seams in this system are reduced to slight traces which have never yet been worked.

4. A vast deposit of quartzose conglomerate and sandstone, commonly called old red sandstone, forms the lowest formation in this great natural order. No coal has yet been observed in or below this formation.\*

5. Trap rocks of various kinds (basalt, greenstone, porphyry, and amygdaloid) occur associated with the preceding formations in various modes of combination, as dykes, irregularly interposed or overlying masses, and sometimes as alternating beds. The phenomena are such as often to render it doubtful whether they are of cotemporaneous origin with the rocks among which they

\* The case of Portishead Point cited by Mr. Weaver as affording an example of this position is erroneous; the coal is there associated with the regular sandstone of the coal measures, and reposes very distinctly on the great carboniferous limestone. Mr. W. who had not visited the place himself, was misled by an incorrect report. I have repeatedly examined the spot in company both with Prof. Sedgewick and Prof. Buckland.

occur; and in the opinion of many geologists favour the hypothesis of their ignigenous formation.

All these formations are shown in their constant and regular order of succession in the greater part of the English and Irish coal fields; and the only Scotch coal field of which we have any distinct account (that of Clackmannan), entirely harmonises. The structure of the carboniferous districts of the Netherlands, and (if we may judge from Beudant's description), of Hungary also, is exactly parallel. In some instances, however, the third and fourth systems being deficient, the coal measures repose immediately on the transition limestones, slates, &c. (as in Colebroke Dale, and Dudley, in England; in the Hartz, and many places on the Continent.

The order of superposition, however, is never inverted, nor is there one single instance on record in which any workable seam of coal has been found below the third or fourth system above enumerated, unless we consider the anthracite sometimes associated with transition slates, and which may be readily distinguished from the true coal formation, as an exception.

The limestones of the third series, and the conglomerates and sandstones of the fourth, are generally referred by continental geologists to the transition class. (See Von Raumer on the Slate Mountains of the Netherlands, &c. Daubuisson, *Geologie*; Beudant, *Hongrie*; Humboldt, &c.). The representations of every one of the above writers render it absolutely impossible to confound the *rothetodteliegende* of Germany with the conglomerates of the fourth system above described.

I have, in another work, assigned my reasons at length for constituting a separate order to include the whole of these four systems rather than referring any part of them to the transition class with which, however, they are very nearly allied.

The geologists of the Continent very commonly consider the *rothetodteliegende* (a formation immediately *overlying* the coal measures), as forming part of this series.

I prefer the view entertained by Prof. Buckland, who has, in repeated journeys in the north of Germany, minutely examined the relations of the *rotheliegende* with reference to this question, and who considers it as the lowest member of the great sandstone series forming the next natural group above the carboniferous series. Though conscious how little weight my own evidence will add to this high authority, I may briefly state that my own personal examination of the Thuringerwald led me to the same conclusion. I have only at present to repeat my assertion, that Lehman, Freisleben,\* Von Buch, Von Raumer, Karsten, Keferstein, Daubuis-

\* Although I purposely avoid entering into details at present, I shall subjoin one extract from Freisleben, which is entirely decisive. He observes that some geologists are inclined to consider the coal rocks and the *rotheliegende* as subordinate members of one great formation; but opposes his own view of the subject to theirs, stating that he considers them "als zwey einander zwar ganz nahe stehende, aber dennoch von einander

son, and Humboldt, all concur in assigning to the *great mass of the rothelietliegende* a position above the coal measures. As that assertion has been questioned, I shall shortly support it by detailed quotations in their own words; and I have only to regret a controversy which renders inevitable a method of proof that must necessarily occupy so many pages usually devoted to much more interesting and valuable matter.

I have thought it necessary thus far to explain the principles on which I proceed in separating the consideration of the *rothelietliegende* from the description of the continental coal districts.

A rock placed exactly in the situation of the *rothelietliegende* (that is, between the coal measures and magnesian lime), is specified in Smith's map of Yorkshire, under the name of the Pontefract rock. He classes it with the coal measures; but the course he assigns to it is unconformable to these, and conformable to the superjacent magnesian limestone. This rock ought to be carefully examined and described.

It should be observed that as the rocks overlying the coal measures cover them unconformably throughout England, instead of succeeding in a regular and conformable series, we have no evidence that the highest beds of the coal formation are

*zu trennende formationen,"* as two very nearly allied formations, but nevertheless distinct from each other; and, conformably with this view, he carefully distinguishes certain coal seams which, according to him, actually are subordinate to the *rothelietliegende* from the true coal formation. "My observations," he states, "lead to the conclusion, that a part of the beds hitherto referred to the coal formation (properly so called) ought to be separated from that formation, and regarded as subordinate to the *rothelietliegende*. "Ein theil der bisher zu den eigentlichen Steinkohlengebirgen gerechneten Steinkohlen fosse von solchen abzusondern, und dem rothliegenden unterzuordnen ist." Mr. Weaver has suffered himself to be misled by neglecting to advert to the circumstance, that the passages he has quoted from Freisleben, as proving that the coal formation generally is included in the *rothelietliegende*, have no kind of reference to the "*eigentlichen Steinkohlengebirgen*," the proper coal formation, but are confined to that part of the series thus carefully separated from it by that author on account of this very position. The cause that seems to have led to this misconception is, that the two formations being contemporaneous, the true coal formation, and these subordinate beds, often occur nearly in the same localities. A careful collation of the map accompanying Freisleben's work with his text removes in the most convincing manner this source of confusion.

The plan of Freisleben's work does not extend to the coal formation properly so called. I should add that the passages above cited from Freisleben are not incidental notices; but form the express declaration of his opinion on this point deliberately pronounced in the section dedicated to the consideration of the relations to which they refer.

I refer to the account of the environs of Glatz by Von Buch for his testimony in this controversy. Dr. Anderson's notes in this translation of that work will convince Mr. Weaver that I am not singular (as he appears to suppose) in my interpretation of Freisleben's sentiments, but have the authority of those best versed in the geological writings of Germany.

Freisleben cites from an author (Karsten) who classes the coal and *rothelietliegende* together as subordinate members of one great sandstone formation, the following enumeration of the several beds included by him in that formation, taken in an ascending order. 1. Conglomerate of ancient rocks. 2. Siliceous conglomerate. 3 to 8. Coal sandstone and shale. 9. Trap rocks. 10. Clay ironstone. 11. *Rothelietliegende*. 12. Weisse-Argende. Now if we look among these rocks for the representative of our own old red sandstone, it must be sought in Numbers 1 and 2, not in Number 11.

in any part of the island displayed, inasmuch as all the beds of that formation come indifferently into contact with the superstrata. It is possible that were those highest beds visible, they might be found to exhibit a gradual transition (as is the case in so many other instances) into the characters of the next sandstone series. If we are to refer the rotheliegende to the coal formation in any manner, it can only be in considering it as thus intermediate in place and character between the regular coal measures and the sandstones of the next æra. *To identify it in any way with the lowest beds of the whole system is to invert every foundation of geological arrangement, and to unsettle all those principles to which the science owes its present precision.*

#### DISTRIBUTION OF THE CARBONIFEROUS SERIES.

##### (A.) *Coasts of the Baltic.*

Following the same course which was pursued in indicating the distribution of the ancient chains, we may first trace the coal formation, where it appears to rest against the most northerly of these chains, that of Scandinavia.

In this line, we find coal in the island of Bornholm, and again in Sweden on the south of the primitive tract near Helsingborg, at the mouth of the Baltic.

##### (B.) *Scotland.*

If we regard the Grampian mountains as a prolongation of the Scandinavian chains, the great coal district of Scotland must also be considered as similarly related to those above-mentioned. It occupies the tract forming what may be called the great central valley of Scotland (speaking relatively, for considered in itself its surface is very considerably varied), which lies between the great transition chain on the south, and the still loftier primitive ranges of the highlands on the north. The whole of this wide tract is occupied by the coal measures, the carboniferous limestone, and the old red sandstone, associated in every possible manner with vast accumulations of every variety of trap.

In the low district on the east of Sutherland, where the secondary formations again intrude among the primitive highland chains, coal has been discovered at Brora; but from the slight description incidentally given of this tract in the memoir of Mr. Bald (vol. iii. Trans. Wern. Soc.), before referred to, it may be conjectured that this does not belong to the principal coal formation, but to those beds which occasionally occur in more recent formations, being, perhaps, of the same æra with the coal of the Cleaveland district in Yorkshire.

In Dumfriesshire, near the southern or transition chain of Scotland, we find many limited coal-fields reposing against, or forming narrow basins in, the valleys of the latter chain; these are associated with, and rest upon as usual, thick beds of the carboniferous limestone.

Trap rocks (porphyry, greenstone, basalt, &c.) occur extensively associated with and overlying the coal districts of Scotland.

(C.) *Ireland.*

We shall next proceed to Ireland, postponing the enumeration for the present of the English coal districts; because the former country is more immediately connected in its geological relations with Scotland.

As we have already traced the primitive and transition chains from the south-west of Scotland to the north-east of Ireland, the formations of the great central valley of Scotland here likewise intervene, and among them the coal measures, which may be seen emerging from beneath the overlying basalt at Fairhead on the north-east, and again just beyond the south-west of the basaltic area at Coal Island and Duncannon; but other parts of Ireland present far more important coal districts. Indeed the island may generally be described (with the exception of the north-east basaltic area) as being almost surrounded with a series of primitive and transition groups, including a great central area entirely occupied by the old red sandstone, carboniferous lime, and coal measures. The structure of these districts very closely corresponds with the tracts of the same formation in England.

The coal district already mentioned is termed by Mr. Griffiths the Ulster coal district. There are three other principal coal fields.

2. The Connaught coal district occupies an extensive tract, in the centre of which Lough Allen is placed.

3. The Leinster coal district is situated in the counties of Kilkenny, Queen's county, and county of Carlow. It also extends a short distance into the county of Tipperary, as far as Killenale. This is the principal carbonaceous coal district. It is divided into three detached parts, separated from each other by the carboniferous limestone, which not only envelops, but in continuation passes under, the whole of the coal district.

4. The Munster coal district occupies a considerable portion of the counties of Limerick and Kerry, and a large part of the county of Cork. It is by much the most extensive in Ireland; but as yet there is not sufficient information respecting the number, extent, or thickness, of the beds of coal it may contain.

(D.) *England.*

The coal fields of England will, from geographical position, naturally fall under the following arrangement: 1. The great northern district, including all the coal fields north of Trent. 2. The central district, including Leicester, Warwick, Stafford, and Shropshire. 3. The Western district, which may be subdivided into North-western, including North Wales, and the South-western, including South Wales, Gloucester, and Somersetshire.

Physical circumstances also confirm this geographical arrangement.

1. *The Northern District.*—A chain of hills, denominated in the time of the Roman colonists of Britain the Penine Alps, forms a continuous ridge (locally, I believe, called the back lobe of England), through the northern counties, from Northumberland to Derbyshire. At its northern and southern extremities (that is to say, in Northumberland, and the West Riding of Yorkshire, and in Derbyshire), this chain exhibits the carboniferous limestone (No. 3 of the carboniferous series). All other parts of it are composed of the shale and grit (No. 2); round the sides of this chain the regular coal measures (No. 1) are disposed so as to constitute several distinct fields.

These may be enumerated in the following order: (a) The great coal field of Northumberland and Durham. (b) Some small detached coal fields in the north of Yorkshire. (c) The great coal field of South Yorkshire, Nottingham, and Derby. On the south, we find only (d) some successful trials for coal in the neighbourhood of Ashborne, which have, however, never been further prosecuted. On the west (e) the coal field of North Stafford. (f) The great Manchester, or South Lancashire coal field. (g) The North Lancashire coal field. (h) The Whitehaven coal field; between this and the preceding coal field, the transition mountains of Cumberland rise on the western side of the Penine chain, but a zone of carboniferous limestone is detached from the Penine chain, and mantles round this group. On the north-west of this zone, the last mentioned coal field reposes. (i) The indications of coal at the foot of the western escarpment of Cross Fell.

2. *The Central Coal Districts* include four detached fields emerging from beneath the great plain of new red sandstone covering our midland counties.

a. Ashby de la Zouch. b. Warwickshire, between Atherston and Nuneaton. c. South Stafford, round Dudley: the coal measures here repose on the transition limestone, Nos. 2, 3, 4, being here deficient. d. Indications of coal near the foot of the Tickey Hill; also in contact with transition limestone.

3. *Western Districts*, subdivided into

a. *North Western*, including, 1. A trough of coal measures reposing on carboniferous limestone traversing the town of Anglesea. 2. A coal field resting on the same rock in Flintshire; the carboniferous limestone range continuously on the north of the transition mountains of North Wales.

b. *Middle Western, or Shropshire.*—This includes several detached fields. 1. In the plain of Shrewsbury. 2. In Coalbrook dale (this reposes on transition limestone). 3. On the summits of the Clee Hills, and in South Shropshire. 4. Near the Abberley Hills (resting on transition limestone).

c. *South Western.*—This includes three principal basins occu-

pied by the coal measures, and surrounded by zones of carboniferous limestone, the whole reposing on one vast tract of old red sandstone. Of these basins, the largest is situated in South Wales, and occupies the greater part of Glamorganshire, extending into Monmouthshire on the east, and Caermarthen and Pembrokeshire on the west. The next in importance occupies the south of Gloucester, and the north of Somersetshire. It is particularly interesting to the geologist, because the more recent formations sweep over a part of it; and their relations with the coal series may here be ascertained by actual sections. Coal shafts being sunk even from the lower oolite through the lias and new red sandstone into the coal measures. The third basin in point of extent is intermediate in position between the other two occupying the forest of Dean, between the Severn and the Wye.

Trap rocks (principally basalt, greenstone, and amygdaloid), are associated with and overlie many of these districts, especially those of Northumberland, Staffordshire, and Shropshire.

(E.) *France.*

As we have already traced coal fields in the north of Europe, apparently connected with the central system of Scotland, so we might naturally look for the resumption of those of the south-western English counties in the opposite regions of France. It is true, indeed, that in the south of Somerset and through Devonshire, no coal has been observed; being probably concealed by the advance of the overlying deposits of new red sandstone which are there in close contact with the transition chains. These transition chains cross, as is well known, from Devon to the peninsula of the Cotentin in Brittany; and we find, as might be expected, a small coal field reposing against their eastern side at Litry on the south-west of Bayeux; further south, where the Loire enters between the continuation of these chains, between Angers and Nantes, are more extensive deposits of this formation.

In the centre and south of France, there are some limited coal deposits lying in the valleys of the Loire, the Allier, the Creuse, and the Dordogne, the Aveyron and the Ardeche between ridges proceeding from the primitive central group connected with the Cevennes, and also on the south-east between the Cevennes and the Rhone.

Several particulars concerning some of these districts may be found in the account of the geological speculations of Mr. Rouelle in the first volume of the *Geographie Physique*, forming part of the great *Encyclopedie Methodique*; the *Annales des Mines* for 1821 contains some information concerning of those near St. Etienne, department of the Loire, and a full account of those of the Aveyron.

From the south of France we may proceed to Spain, which could not so conveniently be included in any other part of our survey. Coal is here mentioned as occurring in eight places in

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Catalonia, in three in Arragon, and one in New Castile; but no  
particulars are given: a list of these localities may be found in  
Laborde's view of that country.

(F.) *Northern France, the Netherlands, and adjacent Parts of  
Germany.*

A great carboniferous tract occupies these countries.

It may generally be described as extending westwards from  
Hardinghen near Boulogne (only a few miles from the coast of  
the channel) by Valenciennes, and thence up the Scheldt and  
down the Meuse to Eschweiler beyond Aix la Chapelle; and  
still further west, many of the coal districts of Northern Ger-  
many may with great probability be considered as a prolongation  
of it.

On the east and north, the great deposits of chalk and the  
strata above the chalk, skirt and partially (particularly within  
the limits of France) overlie this tract. On the south, it is  
bounded by the transition ridges (of slate, greywacke, &c.)  
which occupy the forest of Ardennes, overhang the magnificent  
defile of the Rhine from Bingen to Bonn, and thence extend to  
the Westerwald. This tract does not consist of a single conti-  
nuous coal field, but of many insulated and basin-shaped deposits  
of this formation, encircled by carboniferous limestone and old  
red sandstone. In many respects it bears, even down to the  
character of its picturesque scenery, a remarkable analogy to the  
coal districts (likewise consisting of many insulated basins) in  
the south-west of England.

We find the most westerly point of this extended chain of  
coal fields at Hardinghen, in the great denudation exposing the  
beds beneath the chalk, which comprises the Boulonnais on the  
French side of the channel, and the Weald of Kent and Sussex  
on the English; of this we have before given a general descrip-  
tion. These coal mines, and the quarries of the carboniferous  
limestone associated with them, which appear at Marquise, are  
situated at the very foot of the escarpment of the environing  
zone of chalk hills; for the outcrop of all the intermediate for-  
mations crosses this part of the denudation to the south, and, as it  
were, withdraws to expose the coal; proceeding westwards, the  
coal is worked at several places within the general limits of the  
overlying chalk-formation. The environs of Aniche near Douay,  
and of Monchy le preux near Arras, present deposits of this  
nature; the mines surrounding Valenciennes are still more  
extensive.

In the environs of Mons, Charleroy, and Namur, in a tract,  
surrounding Liege; and lastly close to Eschweiler on the east of  
Aix la Chapelle, other very considerable coal fields are worked.

A general account of this line of coal formation may be found  
in Omalius d'Halloy *Geologie du Nord de la France*, *Journal des  
Mines*, and in Von Raumer's *Geognostisch Versuche*; many

interesting details are also given in Villefosse *sur la richesse minérale* (tom. ii. p. 432 & seq.) and illustrated in the magnificent atlas of that work by sections exhibiting the contortions, &c. of the beds, and the mode of working them, from Pl. 25 to 27.

Proceeding still further along the northern border of the same transition chain, against which all these deposits of coal repose, we find the more recent formations (probably of the tertiary class) intruding upon it, and concealing the coal till we cross the Rhine near Bonn. On the right bank of that river, these again recede to the north, and in this direction we again find an extensive coal field proceeding along the small river Ruhr a little above its junction with the Rhine: on the south the beds of this coal field describe the segment of a circle, cropping out against alternations of limestone, shale, and what is called greywacke (our old red sandstone probably), which separate them from the regular transition slate: on the north they are bounded by the overlying and more recent deposits. An account and plans of this district may be found in Villefosse, tom. ii. p. 424, and Pl. 24.

A little on the south of the same district of transition rocks, whose northern border we have been hitherto pursuing, limited coal fields occur in the country between the Moselle and Rhine; first, between Sarrebruck and Sarre Louis on the river Sarre, and, secondly, near Waldmohr on the banks of the Glane, extending to its confluence with the Nahe: the beds of the former coal field are described as ranging south-west and north-east, and dipping north-west; they are covered with red sandstone, and also surrounded by the same formation on the points to which they rise, appearing, therefore, to repose upon it; but these appearances are very vaguely described; "*sur ces grès rouges semblent quelquefois s'appuyer les couches de houille*;" they are probably deceptive. A careful examination of the country between this coal field and the northern transition chain is necessary to ascertain its true relations. Villefosse, tom. ii. p. 447, and Pl. 27, may be consulted on this district.

Keferstein has given a brief but very clear description of this porphyry and coal district, extending on the south of the slate mountains between the Rhine and the Moselle by Zweybruck and Sarrebruck (*Deutschland*, &c. p. 81, No. 1).

He describes the coal measures, exhibiting the ordinary members and characters of this formation as extending between Sarrebruck and Neukirch; they contain some alternating beds of carboniferous limestone. The stratification is extremely contorted and dislocated; masses of unstratified trap rocks (comprising porphyrywacke, amygdaloid, greenstone, and basalt), are interposed among the coal measures. Agates abound in the amygdaloid.

Quicksilver occurs both in the porphyritic conglomerate, and in the coal sandstone.

"The red sandstone (by this term, Keferstein always denotes the rothetodteliegende as distinguished from the bunte sandstein), lies in several places on these formations."

(G.) *Coal near the Vosges.*

Coal occurs on the west side of the Vosges, but I am not acquainted with any particular description of this tract.

(H.) *Coal of the Alps.*

Traces of the coal formation are rare in this mountain range; but such have been observed in some parts of it.

M. de la Beche thus notices one remarkable instance of this kind:

"The Col de Balme, which closes the valley of Chamouny to the north-east, and separates Savoy from Switzerland, has long been known for the superb view which it commands of the valley of Chamouny, with the Mont Blanc range in one direction, and the mountains of the Valais on the other. The iron cross on the highest part of the Col, or passage, is, according to M. de Saussure, 7086 French feet (7558  $\frac{1}{2}$  English feet) above the level of the sea. The Col is composed of beds of clay slate, of limestone, and of a few thin beds of sandstone. The rocks of the Col de Balme have been called primitive by M. Ebel, in his *Bau der Erde en dem Alpen-Gebirge*, but were with more justice named secondary by M. de Saussure. The beds which compose them seem to be a continuation of limestones, which are remarked in patches in the valley of Chamouny, and which probably once occupied the whole length of the valley.

"While crossing the Col de Balme, in the autumn of 1819, I picked up two portions of the thin beds of sandstone above-mentioned, which I found to contain vegetable impressions precisely similar to what have been termed coal plants, because they are usually found in coal formations. But I tried in vain to find these fossil plants *in situ*, many parts of the Col being too precipitous to allow of my approaching them."

From further conversation on the subject with M. de la Beche, I am persuaded that this spot presents a trough of nearly vertical beds belonging to the true coal formation squeezed in, as it were, between the primitive ridges.

Keferstein notices traces of coal associated with porphyry on the south side of the Alps near Botso. Humboldt also notices these coal deposits: they are on the banks of the Adige between Sais and St. Peter.

(I.) *Coal of Osnabruck.*

Pursuing the line of Northern Germany, a tract containing

Coal appears to range many miles on the south-west and south of Hanover between Osnabruck and Hildesheim, but we cannot refer to any description of it; it may probably form the prolongation of the northern line of coal fields which we lately traced as far as the Ruhr.

(K.) *Coal and Porphyry Formations surrounding the Hartz.\**

These associated formations succeed to the slate mountains of the Hartz, near the east end of which they present themselves in three points:

1. The Opperde district.
2. The Ilefeld district.
3. The Petersbirge district.

Both the first *lie immediately on the slate*, and form a portion of the Hartz chains. The third forms an insulated district in the Saale kreisse. The coal formation presents the ordinary alternations of bituminous shale and coal sandstone, of the usual varieties, and some calcareous beds with marine fossils. Three beds of coal are sometimes found; the principal variety is slate coal.

1. Near Opperde (on the north-east of the Hartz), the coal *lies under*, and in part† also in the red sandstone; no porphyry is found in this tract.

2. But in the Ilefeld district‡ (on the south-east of the Hartz), porphyry prevails near Neustadt. The *more ancient members* of this formation pass immediately into the greywacke through modifications, approaching to hornstone rocks. The *younger members* are mixed with more clay, become sandy and conglomerated, and thus *pass into the red sandstone*. The coal strata *lie between the porphyry and the slate hills, so that they have the first place in the order of superposition* (sie ersten zum hangenden haben). *In the upper formations, the conglomerates and sandstones exhibit themselves.*

3. The Petersbirge. This is a great mass of the porphyry formation bounded by Lobegun, Wettin, Halle, Landsberg, Bitterfeld, and Zorbig (lying a few leagues south-east from the Hartz).

\* I have abridged (but without any other alteration than that of condensation) this account from Keferstein (Deutschland, p. 138, No. 1). I have done this because the statements I formerly published have been controverted by Mr. Weaver. It is well, therefore, to call in an impartial witness as arbitrator. I must leave the reader to judge towards which side the evidence thus introduced inclines.

† This is the part which Freisleben separates from the true coal formation; it extends to Meisdorf and Endorf, and is coloured in the map accompanying his work as rothetode; but that map exhibits also the true coal formation as here interposed between the rothetode and the transition rocks.

‡ The true coal formation lies (see the map accompany Freisleben) considerably on the north of Neustadt. The coal beds described by him as subordinate to the rothetode are on the contrary on the south-east of that town, extending thence towards Ilefeld. The whole of this tract is coloured as rothetode in the map, which presents the regular series; transition rocks, coal, porphyry, rothetode, overlying each other in succession.

Porphyry is the prevalent rock ; the upper beds (as at Neustadt) become conglomerated and approximate to the red sandstone.

The coal measures occur at Lobegau, Wettin, Dolau, Brachwisch, and Giebichenstein ; they occur in the porphyry, sometimes covering, sometimes covered by it (the map accompanying Freisleben represents them as generally beneath the porphyry) ; the porphyry above and below the coal are distinguished by the more granular structure of the former, which is associated with the conglomerates ; the coal is stratified, but disposed in saddles, troughs, &c. and often swells into irregular masses. Where the coal measures are thickest, and no porphyry is found beneath them (as at Wettin), the coal sandstone prevails to the greatest depth, and is often much like the greywacke.

The red sandstone lies either upon the slate mountains of the Hartz,\* or upon the porphyry of Ilefeld and the Petersbirge, and forms a range of hills stretching through Mansfield between the two points.†

With regard to the porphyry associated in these coal districts, I have to add to the above account, that its relations with the formations among which it occurs are very obscure. Humboldt

\* That is where the porphyry and coal are wanting, as appears from the remainder of the sentence. See also the map in Freisleben.

† It appears from the statement of Freisleben, that the rothetodte of these hills *reposes upon the Wettin coal field*. His localities include *all the rothetodte* represented by the map accompanying his work in that quarter ; so that the fact of the superposition of the *great mass of the rothetodte* above that coal field cannot admit a doubt. It appears, however, that a conglomerate resembling the rothetodte also occurs in some places beneath these coal measures. On this account, Freisleben is inclined to rank this field, not with the true coal formation, but with the carbonaceous beds subordinate to the rothetodte. His inferences are, however, open to doubt, first, because, he *had never himself seen the inferior conglomerate in situ, and reasoned from specimens only* ; and, secondly, because the occurrence of conglomerates of similar character above and below the coal measures can never be admitted as a proof of identity of formation, otherwise our own conglomerates of the old and the new red sandstone must be considered as a single formation, and the carboniferous limestone and coal measures be treated as subordinate to that formation—a conclusion which, in the mind of every instructed geologist, will be fully equivalent to a *reductio ad absurdum*. I, therefore, demur to the statement that any part of these coal districts are really subordinate to the rothesandstein, as one requiring further confirmation. Be this as it may, however, it will not prove that the true coal formation (*which, as we have seen, is distinct from these subordinate beds*), is similarly related. The whole evidence shows, as clearly as any evidence can do, that *the appearance of this rock is confined to the upper regions of the coal formation* ; and if it be not referable, as (following Buckland), I believe it to be, to the sandstone series of the succeeding geological æra (with which I again assert, *from personal examination*, and not with our old red sandstone, it most closely agrees), it can only be considered as an upper member in the coal series. Humboldt inclines to consider the red sandstone as associated with the coal series : *La houille*, he adds, *paraît le plus souvent au dessous du grès rouge, quelquefois il est placée évidemment ou dans cette roche ou dans la porphyrie*. He adds no other facts than those above alluded to ; and I, therefore, adhere to the interpretation before given of the cases in which it is said to occur in the red sandstone.

In concluding this note, I have to correct an error which has arisen from hasty transcription in Mr. Weaver's account of the beds in the Lobegau colliery : he makes the lowest stratum rothetodteliegende, whereas it stands in his original rothethonartigeriegende ; not the rothetodte, but a red argillaceous stratum. The point is not very material ; because Freisleben probably considered it as belonging to that formation ; but questions of this kind can only be settled by minute accuracy.

observes that it penetrates the coal measures in various manners; sometimes it covers the coal immediately; more generally it reposes on the sandstone, elevating itself above this rock in domes, towers, and escarped rocks. When the transition rocks are immediately covered by the red sandstone, it is difficult to say whether the porphyry associated with the coal is referable to the transition or sandstone series. The porphyry seldom forms true beds in the coal measures, but rather transversal and interposed masses. He adds, that these masses offer many analogies with volcanic rocks, and inclines to favour the opinion, that the pretended passage of the porphyry into the sandstone is an illusion produced by regenerated porphyrics; i. e. by the subsequent reunion of porphyritic debris. These remarks are applicable generally to the porphyry associated in the coal districts of Germany. From its intimate connexion in geographical position with the coal districts, I have included it under the same colour, though strongly inclined to consider it as of subsequent formation.

(L.) *Coal and Porphyry of the Thuringerwalde.\**

The porphyry of the Thuringerwalde constitutes the highest portion of that chain; it contains, according to Keferstein, subordinate beds of coal between Ilmenau and Sahl.

(M.) *Coal, &c. on the Western Borders of the Chains extending from the Thuringerwalde.*

A small coal district occurs on the borders of the Saxon slate mountains, not far from Steinach, near the point where they join the Fichtelgebirge, and porphyry is found on the borders of the Bohemerwald, near Ratisbon.

(N.) *Great Saxon District of Coal and Porphyry.*

This is an extensive district which may generally be described as lying along the course of the Zwickau between Leipzig and the Erzgebirge; porphyry prevails. Coal accompanied by the usual rocks occurs near Zwickau, at Schonfeld, Planenschen grund near Tharand, &c. sometimes overlying, sometimes underlying, and sometimes associated with the porphyry.† This dis-

\* The following articles are chiefly from Keferstein.

† At Schonfeld, the coal alternates with porphyry, above which occur the following beds: 1. A conglomerate of porphyry and gneiss. 2. Bituminous shale with vegetable impressions. 3. Red sand. At Zwickau the beds (beginning with the lowest), 1. Wacke. 2. Basalt. 3. Nine or ten coal beds alternating with white grit and shale containing vegetable impressions. 4. Sandstone. 5. Red grit. The coal field of Planenschen grund is more extensive than either of the former; here a range of sienite, extending on the right bank of the Weisseritz, forms the fundamental rock on which a secondary porphyry reposes; then succeed four beds of coal alternating with grit and shale, and inclined at an angle of 65°.

There is no bitumen in the coal of Schonfeld; although it abounds at Planenschen grund: the same fossil vegetation is presented in all these mines. (See Sternberg *Flora zur Vorwelt*.)

148      *Rev. W. D. Conybeare on a Geological Map of* [FEB.  
tract is supposed to have a subterraneous connexion with that of  
Petersbirge near the Hartz.

(O.) *Coal and Porphyry of Bohemia, and Bohemian Silesia.*

A zone of these formations crosses these countries from south-west to north-east. It may be thus subdivided:

1. *The Pilsen Coal District.*—A series of detached coal basins reposing on transition slate, extending from Merklin, by Pilsen and Radnitz, to Prague. Porphyry does not occur in this tract.

2. *The Waldenburg District.*—Extending from Landslut to Waldenburg and Glatz between the Riesengebirge and Eulengebirge. It exhibits both porphyry and coal.\* These formations alternate; a chain of them follows the slate mountains; then succeeds the red sandstone. The coal is very irregular in its stratification; the principal variety is slate coal, the ordinary coal rocks occur: the beds are numerous.

3. *The Pilsen and Waldenburg Districts* have evidently a subterraneous communication beneath the intermediate plains in which the coal formation often makes its appearance. No porphyry is seen at these points.

4. *At Freiheit Semile and Liebenau*, the porphyry alone appears.

(P.) *Moravian Coal District.*

Coal, unaccompanied by porphyry, occurs in the environs of Brunn.

(Q.) *Upper Silesian Porphyry and Coal District.*

This is an extensive district including the following towns: Pless, Freystadt, Troppau, Iagerndorf, Kosel. It lies partly in Silesia, and partly in Poland: in the former, it exhibits coal only; in the latter, coal and porphyry. The coal formation reposes immediately on the slate mountains of the Sudetengebirge. The strata are elevated in approaching that chain, but more horizontal as they recede from it. The coal measures are covered by the porphyry, which, in its turn, supports the Alpine (magnesian) limestone. The coal measures pass by such a gradual transition into the greywacke on which they repose, that, according to Keferstein, it is difficult to ascertain the exact demarcation between them. The usual rocks of the coal formation prevent grits of various textures; millstone grit, shale, with

\* According to the map and sections of Von Raumer, the outcrop of the principal coal beds follows the border of the transition rocks; but this writer mentions a conglomerate and a limestone among the upper beds of the transition series; and as he (in common with Beudant, Humboldt, Daubuisson, &c.) always refers the old red sandstone and mountain limestone of English geologists to the transition class, he very possibly thus designates them here. The same map and sections show that a great part of the porphyry and the red sandstone generally are placed above the great coal formation. But he adduces instances of coal subordinate to the red sandstone, and inclines to consider them as referable to one great formation.

nodules of clay ironstone, and (carboniferous) limestone; the coal is mostly slate coal: there are numerous beds, and some of very considerable thickness.

(R.) *Coal of Hungary.*

The lower series of our coal formations; that is to say, our old red sandstone and mountain limestone, are expressly, and by name, recognised by Beudant, as reposing on both sides the Tatra and Kralova mountains (a portion of the Carpathian chain): he refers them to the transition class, and describes them as grit of a red or white colour, having a siliceous cement supporting a compact limestone with nodules of chert. On these repose the coarser coal grits; then some beds associated with limestone, and also containing traces of coal (which, however, from his description, I should rather refer to the rothetodte series); then amygdaloid and red grit; and, lastly, the saliferous sandstone. Coal is found at Balligorod and Rosocky, near Sanok.

Another coal district occurs at Funfkirchen, on the borders of Slavonia; it presents (beginning with the fundamental rock), 1. Black limestone. 2. Coarse coal grit. 3. Slaty coal grit. 4. Shale and fetid limestone. 5. Carbonaceous grit. 6. Coal. 7. Several varieties of grit. 8. Greenstone. 9. Red grit. 10. Red porphyry. The latter beds clearly belong to the rothetodte of Germany.

(S.) *Coal of Russia.*

Coal or other bituminous formations are represented by Mr. Strangways as occurring, 1. In the north of Russia near Yarousk. 2. On the east reposing against the Ural mountains, near the source of the Chusovnga. 3. In the centre of Russia, at Calouga and Toulá (where the great iron works are established). 4. In the south, at Bakmont on the Donetz. 5. In the Crimea, and reposing against the Caucasian chain; but these districts have not yet been examined with sufficient care to ascertain whether the beds really belong to the true coal formation.

(To be continued.)

ARTICLE X.

*Proceedings of Philosophical Societies.*

ROYAL SOCIETY.

Dec. 19.—A paper was read, On the Chinese Year, by J. F. Davis, Esq. FRS. The introductory portion of this paper was occupied, in proving that no scientific knowledge of astronomy existed in China prior to that which was introduced in early

times by the Arabians, and afterwards by the European missionaries. The 36 eclipses recorded by Confucius, are useful in determining points of chronology, but furnish no evidence of astronomical science; the Chinese had been in all ages attentive observers of the sky, and of the apparent changes of the heavenly bodies. The encouragement and promotion by that ever-jealous people, of foreign professors of astronomy, evince that they could not, originally, have been acquainted with it themselves; and this is also shown by their adoption of the errors of those foreign professors. The author has seen in an ancient Chinese book, a complete delineation of the Ptolemaic system, with its crystalline orbs, and the earth in the centre. The inscriptions in the Chinese language on some of their ancient instruments, do not prove their own knowledge of astronomy; those instruments were made for them by the Arabians: the cannon cast for the Chinese by Europeans have inscriptions on them in the language of the former.

It has ever been the ungrateful practice of that people to appropriate as their own every invention of other nations. When Mr. Pearson transmitted to China some of the vaccine matter, he sent with it a pamphlet in Chinese, containing directions for its use; an expurgated edition of this was published, shortly after its arrival, in which nothing appeared, from which it could be learned that vaccination was not a Chinese discovery.

The nature of Mr. Davis's astronomical details concerning the Chinese year, precludes us from giving an account of them; they were accompanied by an illustrative drawing, showing the 28 constellations of which the year consists, with the degrees they respectively occupy: the Chinese have no solar year.

At the same meeting a portion of the following paper was read:—On Rocks that contain Magnesia, by Charles Daubeny, MD. MGS. Professor of Chemistry, Oxford: communicated by Prof. Buckland; after which the Society adjourned over the Christmas vacation until Jan. 9, 1823.

Jan. 9, 1823.—The reading of Prof. Daubeny's paper was resumed and concluded. This paper commenced with an account of the localities of the secondary magnesian rocks in England and on the continent of Europe. The presence of magnesia in many simple minerals, and in the primary strata, had long been well known; but Mr. Tennant was the first to detect its presence in certain secondary limestones, which occupy a considerable space in the north of England; and corresponding magnesian beds have since been discovered to exist extensively on the continent. This earth appears to extend almost throughout the secondary strata; it occurs in the mountain or carboniferous lime series; in the zechstein of the Alps, referred by Prof. Buckland to the English magnesian limestone; it has been found by Mr. Warburton in the blue and in the white lias, and its presence in them is indicated by the springs containing sulphate of magnesia with which they abound. The presence of

this earth in the oolites has not been ascertained with certainty, and it has not yet been discovered in the chalk of this country; though Brongniart has detected it in that near Paris; and he ascribes the sterility of Champagne, to its presence in the chalk of that province. Magnesia also occurs in the tertiary formations which, in many countries, succeed the chalk; its presence in the London clay is shown by the efflorescence of its sulphate from the bricks which are made of it, and the springs of Epsom may also be adduced in proof of this: it has likewise been found in the calcaire grossier of France, and in a specimen of the same formation containing nummulites, from near Verona.

After some remarks on the importance of the subject, in its connexion with building and with agriculture, from the modification by the magnesia of the characters of the rocks into which it enters, and from the effect of that earth upon vegetation; Dr. D. proceeded to describe the chemical methods he employed in his examination of the magnesian rocks. He stated that our object be merely to detect the *presence* of magnesia in a limestone, we may adopt Dr. Wollaston's process, by carbonate of ammonia and phosphate of soda; or that in which oxalate of ammonia is used in conjunction with those salts; but these cannot be employed in ascertaining the *quantity* of it which may exist; for in both of them a portion of magnesia is thrown down; and in the first, a part of the lime remains unprecipitated. For the last mentioned purpose, Dr. D. suggested the following process, for the details of which he referred to his paper on the subject lately published in the Edinburgh Philosophical Journal. Dissolve the substance under examination in nitric acid, evaporate the solution to dryness, so as to expel the nitric acid; then dissolve the lime and magnesia in acetic acid, and separate them by converting them into sulphates: the only objection to this method of separating the two earths is, that by too great a heat, part of the acid may be driven off from the sulphate of magnesia, and so the proportion of that earth underrated; or, in consequence of not employing sufficient heat, some water may remain in the salt, and thus the quantity of magnesia may be overrated.

The paper concluded with a catalogue of the secondary rocks in which the author has found magnesia, with some account of their external characters; the primitive rocks were omitted because the presence in them of the earth in question is so well known. Some of the characters of magnesian limestones afford a presumption of their nature; their colour varies from honey-yellow to saffron-red, and their fracture presents small crystalline points, having a glimmering lustre: slight effervescence with acids, of course, is a test only when the proportion of magnesia is large; but it is a sufficiently accurate one for agricultural purposes.

At this meeting likewise, the following paper was read:  
 Corrections applied to the great Meridional Arc extending from Latitude  $8^{\circ} 9' 38.39''$ , to Latitude  $18^{\circ} 3' 23.64''$  to reduce it

to the Parliamentary Standard, by Lieut.-Col. William Lambton, FRS. In this paper, Col. Lambton expressed his satisfaction, on account of the uniformity thereby obtained, that the commissioners appointed to consider the subject of weights and measures have adopted Mr. Bird's standard. In order to reduce the above arc to this standard, the measurement of it determined by the brass scale must be multiplied by  $\cdot 0000018$ , and the product *subtracted* for the correction; and that by Ramsden's bar must be multiplied by  $\cdot 000007$ , the product being *added* for the correction. Col. Lambton has just completed some measurements, which, when the requisite calculations are made, he purposes to submit to the Royal Society. He is proceeding with his arc through Hindostan; if Scindiah's country continue quiet, a section of it will pass through Gwalior, his capital, and end at Agra on the Jumnah.

*Jan. 16.*—Some practical Observations on the Communication and Concentration of the Magnetic Influence, by Mr. J. H. Abraham, of Sheffield; communicated by the President. This paper was divided into three sections; the first being on the depth to which magnetism penetrates. In this, the author stated, as the result of a number of experiments with steel bars of various dimensions, that magnetism does not penetrate deeper than the 1-20th of an inch; and that bars of 1-10th of an inch thick are as powerful as those of greater thickness, the increase of surface in the latter being taken into consideration. The second section related to the communication of the magnetic influence: in this, some minute directions were given for the formation of magnets of the bar and horse-shoe kinds, both simple and compound; the construction of a very powerful compound magnet in the author's possession was explained by means of a drawing. It was stated that magnetism, like electricity, extends at once over the whole surface of the body exposed to its influence. If an octagonal bar of steel be magnetized by a stroke with a set of magnet-bars along one of its sides, each side, at the same distance from the centre, will acquire the same degree of magnetic power. In another respect, however, magnetism differs from electricity in a striking manner: if a charged Leyden jar be brought into contact with one that is not charged, the electric fluid will be immediately communicated to the latter, and it will be divided between the two in an equal proportion; but one bar of steel being placed upon another, though the uppermost become magnetized over its whole surface instantaneously by a single stroke with a set of magnets, yet the lower one did not acquire sufficient magnetic power for the attraction even of needle pointings, which are much finer than iron filings. It was the observation of the magnetic action on these pointings, that led the author to his invention for obviating the ill effects of dry grinding, which the Society of Arts had crowned with their gold medal. In the last section of the paper, some phenomena in polarity were described. The author has succeeded in giving

several poles to a magnet by the following method: he took a set of magnets, and went over a bar with them in one direction, from one end to the middle, then turning the set round went on from the middle to the other end, thus leaving off with the same pole that he began with: the bar so magnetized possessed similar poles at the ends, and exhibited opposite polarity in the middle: in this manner even five poles may be given to a single bar of steel.

At the same meeting, the reading was commenced, of Observations on Magnetism, by John Macdonald, Esq. FRS.

#### LINNEAN SOCIETY.

Jan. 21, 1823.—The following papers were read:—Description of three Insects of Nepaul, by Major-Gen. T. Hardwicke, FLS.

Description of a Tail-less Deer, Native of the Snowy Mountains of Nepaul, by the Same.

Perhaps the *Cervus pygargus* of Pallas: head the size of that of a full-grown stag; horns trifurcate, tuberculated at the base; neck curved, like that of a camel, with a mane on the back; when walking, carries its head in a horizontal position. Though called tail-less, it has the thick rudiment of a tail, four or five inches long. Colour, a brownish ash-colour; darkest along the dorsal line. The following are some of the dimensions of the specimen examined, which was presented by the Court of Camandu to the British resident, and is now in the menagerie of the Marquis of Hastings, in his park at Barrackpore. Length of head 1 foot 5 inches, ditto of neck 3 feet 5 inches, ditto of body 2 feet 5 inches, total length 7 feet 3 inches; height 4 feet 3 inches, circumference round the abdomen 4 feet 9 inches. Though the animal has been accustomed to the society of man for two years, yet it still brings its horns into a position of offence or defence when approached; it is not, however, fierce; but may easily be led by the horns.

#### GEOLOGICAL SOCIETY.

Nov. 1 and 15.—A paper was read "On the Geology of Hungary," by the Hon. W. T. H. Fox Strangways, MGS.

The author, after stating that *calcaire grossier* is found in the immediate vicinity of Vienna, follows the course of the Danube, and enters near Presburg, the lesser plain of Hungary; this plain is bounded on the west by the hills called Leitha Guberge, and a granitic chain, which is connected with the White Hills and the Carpathians. These ranges form the gorge of the Danube at Presburg; on the south is a branch of the Alps of Styria and Carinthia; on the north are the ramifications of the northern part of the Carpathians; on the east, the hills of the forest of Bakony, through a gorge of which, between Gran and Buda, the Danube finds its way into the great central plain of

**Hungary.** The author supposes the whole of the lesser plain of Hungary to be composed of calcaire grossier, and the several localities in which he observed it are enumerated; leaving this plain, and proceeding northwards, he crossed the granitic chain which lies between the valleys of Nyitra and Thuroez, and continuing his route, ascended Mount Fatra, the lower part of which is composed of shale, above which is a hard oolitic limestone which the author believes to be only the lower bed of the calcaire grossier.

On the right bank of the Vag, opposite the town of Rosenberg, are some conical hills of hard white calcaire grossier, on a rock of which is the ruined castle of Dyömbir. This rock continues to form all the hills on the right bank of the river, and behind them are seen the high summits of the Carpathians, among which the conical Mount Tepla is most conspicuous. The calcaire grossier during all this space resembles that which forms the left bank of the Danube between Ratisbon and Passau. Before arriving at Ocholicsna, the beds of calcaire grossier terminate on the north side of the valley, throwing up a bold escarpment towards the magnificent group called collectively the Mount Tatra, and of which the Krivan (esteemed the loftiest of the Carpathian chain), rising into the form of a cone, forms the western extremity.

Most of the high valleys in the north of Hungary are poor and ill cultivated; bare of wood, except a few stunted pines. Continuing his route eastward, Mr. Strangways found the country to consist chiefly of shale and grit, which extends nearly as far as the neighbourhood of Eperies. On the road to Bartpha, it is interrupted by a narrow band of a dark porphyritic granite, which rises into detached conical hills near the post Ternyo. On the westernmost is situated the fortified town of Szeben. Near the town of Bartpha is a reddish shale and sandstone, which constitute the mass of the continuous range of the Carpathians. Crossing this range by the pass of Dukla, he entered by the valley of the San, the vast plains of Poland, highly cultivated, and in every way a contrast to the poor, but varied mountains of Hungary. Galicia is a plain of vast extent; in this rich but uniform country, natural sections and interesting scenery are found alone on the banks of the rivers. The whole country appears to be a sandy lime, sometimes resembling chalk. West of Leopold, the soil is a light yellowish sandy earth, resembling that of the richest part of the calcaire grossier of the Netherlands. East of Leopold is a deep black soil exactly resembling that of the most fertile parts of Russia; both appear to cover the same formation, and they are equally productive.

*Dec. 6.*—A paper was read, "On the Geology of some Parts of Arabia, and some Islands in the Persian Gulph," by J. B. Fraser, Esq.

This paper contains geological observations on a part of the

coast of Arabia, and several islands in the Persian Gulph, and was accompanied by specimens collected in the countries which he describes. On a voyage from Bombay to Bushire (near the head of the Persian Gulph), Mr. Fraser had an opportunity of examining the country round Muscat, and for a distance of some miles in the interior; where there is a considerable extent of serpentine and of stratified calcareous rocks. He landed also on the island of Rishm, consisting of a formation probably very recent; while the rocks on Ormus seem almost wholly primitive.

Two letters were read from W. Hamilton, Esq. his Britannic Majesty's Minister Plenipotentiary at the Court of Naples, to Dr. Granville, MGS. giving a description of the late eruption of Vesuvius.

Dec. 20.—Part of a letter to Dr. Wollaston, VPGS. from Dr. Fitton, was read, containing an account of the geology of the vicinity of Boulogne, and notifying the author's intention to present a memoir on this subject to the Society. This notice was accompanied by a map and sections of the district included by the chalk, from the place where it leaves the sea near Wissant, to where it rejoins it to the west of Neufchatel and Samer.

A paper from George Cumberland, Esq. was read, describing the strata round Dursley, Stroud, and other places on the Banks of the Severn, and in the county of Gloucester, with a description of some fossils found in several beds of the oolite formation.

Jan. 3.—A letter was read from the late Rev. John Wright, rector of one of the principal parishes in Nassau, on the Bahamas, transmitted to Professor Buckland, in compliance with a late circular letter from Earl Bathurst to the colonies, giving an account of the geological structure of the Bahamas.

These islands, which stretch in length not less than 600 miles, appear to be a very recent formation. They are all calcareous, and have a strong resemblance to each other in their general features. A very full and interesting account of their physical structure, products, and geographical relations, is furnished by Mr. Wright's paper, with a description of some curious caverns and valuable salt lakes in which many of them abound.

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## ARTICLE XI.

### SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

#### I. *Electro-Magnetic Experiment.* By Prof. Oersted.

ALTHOUGH there are many proofs that every point in the circumference of a round galvanic conductor has an equal action on the magnetic needle, yet it appears that several authors are of the contrary

opinion. The reason of this may be, that no method has hitherto been devised of giving the needle any desired situation with respect to the conductor; and although Paggendorf has thrown much light on this subject, by employing magnets for the purpose of giving the needle any situation at pleasure, yet the respective intensity of action which each point exerted on the needle remained undecided in his experiment.

Prof. Oersted has endeavoured, by the following experiment, to remove all doubts on this subject. A brass wire, 10 feet long, and  $\frac{1}{36}$ th of an inch in diameter, is placed in a perpendicular position on the side of a column, at the middle of which is a stand to place the needle on. Both ends of the wire are placed in small vessels filled with mercury; by these it communicates with a single pair of plates on the Professor's construction, in which the vessel is of copper; and in order to prevent the influence of the other parts of the apparatus, this is so constructed that they remain far distant from the needle during the experiment. The galvanic apparatus is fixed on the middle of a pole 10 feet long; one of the conductors passes over the upper end of the pole, and the other beneath the lower one; and from thence they are brought, in a horizontal direction, each into contact with the mercury in one of the small vessels. The entire apparatus is of a rectangular figure.

The pole with the galvanic apparatus is now made to revolve in a circle around the perpendicular wire and the needle: during the experiment, therefore, every point in the wire will constantly change its position with respect to the galvanic apparatus, and will be, in succession, on the inner and on the outer side of the galvanic chain. In a series of experiments in which the apparatus went round at least three-fourths of the circumference, the needle constantly exhibited the same declination which it had at the moment when the galvanic circuit was completed. It appears, therefore, from these experiments, that no doubts can be entertained, but that every point in the circumference of the conductor has the same effect upon the magnetic needle.

## II. Existence of Metallic Veins in the Transition Limestone of Plymouth.

It has hitherto been believed, that the extensive strata of limestone in the neighbourhood of Plymouth, which belong to the Transition class of Werner, or to the Submedial order of the Rev. W. D. Conybeare's new arrangement of rocks, are altogether devoid of metallic veins. The Rev. Richard Hennah, in his "Succinct Account of the Lime Rocks of Plymouth," lately published, being the Substance of his communications respecting them to the Geological Society, says, p. 18, "The bed of Plymouth limestone appears to be entirely free from all *metallic veins*; and I have not seen, nor have I heard, of any thing tending to what miners would call a *lode*, of any of the metals, having ever been discovered in any one of the quarries that have hitherto been opened. It is true that indications of the presence of *iron*, and also of *manganese*, may occasionally be seen, as a colouring matter, in particular spots; but always thinly spread, and never in any quantity."

Mr. Hennah also states, that, in a single instance, he has observed "spots of pyrites in fragments of limestone and slate," found in sinking

as well at Stonehouse; and that he has been told, that in digging another well, on the road leading from Plymouth to Cat-Down; "particles of pyrites were observed by the workmen employed, and also a glimpse of copper;" but he expresses strong doubts of the accuracy of this relation.

It appears, from the following circumstance, that this subject demands a more rigorous investigation: in a collection of specimens from the Plymouth limestone, recently brought to London, is an assemblage of small quartz crystals, mingled with *crystalline galena*; upon the galena are small lenticular crystals of *carbonate of iron*, and the amorphous mass of quartz into which the crystals pass, is mixed with a greenish-grey substance not unlike chert, in which are disseminated minute cubes of *iron pyrites*. The entire specimen bears every character of having once formed part of the lining of a cavity in a vein: it was procured from a quarry at Cat-Down.

## ARTICLE XII.

### NEW SCIENTIFIC BOOKS

#### PREPARING FOR PUBLICATION.

Observations on the Functions of the Digestive Organs, especially those of the Stomach and Liver; with practical Remarks on the Treatment of some of the Diseases to which these Organs are liable. By W. Prout, MD. FRS. &c. &c.

\*.\* This Work will comprise the results of an experimental inquiry into the nature of some of the more important chemical changes which take place during the digestion and assimilation of the food. The practical remarks will principally relate to the proper adjustment and use of remedies, and to the pernicious effects liable to be produced in delicate habits by the constant operations of various slowly acting causes, especially impure or *hard* waters: illustrated by analyses of the principal waters in common use in the metropolis and its vicinity.

An Account of an Expedition from Pittsburgh to the Rocky Mountains, performed in the Years 1819, 1820, by Order of the Hon. J. H. Calhoun, Secretary of War, under the Command of Major S. H. Long, of the United States, Topographical Engineer. By Edwin James, Botanist and Geologist to the Expedition. In Three Volumes, with Maps and Charts.

An Account of a Voyage to Greenland in the Summer of 1822, with an accurate Map of that remote Region, by Mr. Scoresby.

#### JUST PUBLISHED.

A Universal Technological Dictionary, or familiar Explanation of the Terms used in all Arts and Sciences; containing Definitions drawn from original Writers, and illustrated by 60 Plates, and very numerous Wood-Cuts of Diagrams, Arms, &c. By George Crabb, AM. Author of "English Synonymes Explained." In Two Volumes, 4to. Price 5l. 8s.

Transactions of the Royal Geological Society of Cornwall, instituted Feb. 11, 1814. Vol. II. with Six Maps, &c. 8vo. 16s.

Dr. Faithhorn on Diseases of the Liver and Biliary System, comprehending those various, extensive, and often complicated Disorders of the Digestive, Internal Organs, and Nervous System, originating from these Sources. The Fifth Edition. With an Appendix of Cases, illustrative of the Principles of Treatment. 8vo. 9s. Boards.

A new Map of the Ear, exhibiting its external, intermediate, and internal Structure, together with the principal Nerves and Blood Vessels in its immediate Vicinity, designed as an Anatomical Illustration of that important Organ. By J. H. Curtis, Esq. Aurist to the King. 6s. coloured.

Researches respecting the Medical Powers of Chlorine, particularly in Diseases of the Liver; with an Account of the new Mode of applying the Agent, by which its Influence on the System can be secured. By W. Wallace, MRIA. Member of the Royal College of Surgeons in Ireland, Lecturer on Anatomy and Surgery, &c. 8vo.

A new View of the Infection of Scarlet Fever, illustrated by Remarks on other contagious Disorders. By W. Macmichael, MD. FRS. &c. Physician Extraordinary to the Duke of York, &c. 8vo.

### ARTICLE XIII.

#### NEW PATENTS.

J. Brindley, Finsbury, near Rochester, ship-builder, for improvements in the buildings of ships, &c.—Oct. 18.

T. Leach, of Blue Boar-coat, Friday-street, Cheapside, for an improvement in steam-engines. Communicated to him by a foreigner.—Oct. 25.

W. Piper, of Cookley Iron-works, Wolverley, Worcestershire, civil engineer, for several new anchors, for the use of shipping and other vessels.—Nov. 1.

A. Flint, of Uley, Gloucestershire, engineer, for a machine for scouring, pising, and washing of woollen cloth.—Nov. 1.

J. Oxford, of Little Britain, for an improved method of preventing premature decay in timber, metallic substances, and canvass.—Nov. 1.

J. D. Moxon, Liverpool, ship-owner, for improvements in the construction of bridges, and works of a similar nature.—Nov. 9.

F. Deakin, Birmingham, sword manufacturer, for an improvement in the manufacture of holster-cases, cartouch-boxes, and certain other description of cases.—Nov. 9.

J. Jekyll, of Roundhill House, Wincanton, Somersetshire, for improvements in steam or vapour baths.—Nov. 9.

R. Roberts, of Manchester, civil engineer, for improvements in the process of weaving plain or figured cloths, or fabrics.—Nov. 14.

J. Egg, Piccadilly, gun-maker, for certain improvements in the construction of guns and fire-arms, upon the self-priming and detonating principle.—Nov. 26.

## ARTICLE XIV.

## METEOROLOGICAL TABLE.

1892.	Wind.	BAROMETER.		THERMOMETER.		Evap.	Rain.	Daniell's hyg. at noon.
		Max.	Min.	Max.	Min.			
12th Mon.								
Dec. 1	S W	29.50	29.21	49	35	—	26	
2	S W	29.40	29.20	42	33	—	42	
3	N	29.70	29.40	43	30	—		
4	S W	29.92	29.70	49	31	—	35	
5	S W	29.70	29.59	46	35	—	33	
6	N W	30.08	29.70	45	31	—		
7	N W	30.35	30.08	40	28	—		
8	S W	30.35	30.25	48	32	—		
9	S W	30.47	30.23	48	32	—	—	
10	N W	30.65	30.47	44	26	—		
11	N E	30.66	30.63	33	24	—		
12	N E	30.63	30.50	40	29	—		
13	E	30.50	30.42	39	30	—		
14	S E	30.42	30.25	36	26	—		
15	N E	30.29	30.25	37	31	—		
16	N E	30.47	30.28	36	30	—		
17	N W	30.47	30.35	42	31	—	—	
18	N E	30.36	30.34	45	35	—		
19	N E	30.47	30.36	38	27	—		
20	N E	30.47	30.46	36	26	—		
21	N E	30.46	30.30	35	26	—		
22	N E	30.30	30.23	38	32	—		
23	N E	30.23	30.21	42	34	—		
24	N E	30.55	30.21	35	31	—		
25	E	30.57	30.55	34	23	—		
26	S E	30.55	30.49	36	18	—		
27	E	30.49	30.41	34	14	—		
28	N E	30.41	30.30	29	19	—		
29	N E	30.30	30.03	31	20	—		
30	E	30.03	29.93	30	23	—		
31	E	29.94	29.93	31	27	.98		
		30.66	29.20	49	14	.98	1.36	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

## REMARKS.

*Twelfth Month.*—1. Rainy. 2. Fine day: rainy night. 3. Fine. 4. Rainy. 5. Fine day; between eight and nine, p. m. rain commenced with a very brisk wind from the SW, which increased during the night to a violent gale. 6. Very windy morning: cloudy. 7. Foggy. 8. Fine. 9. Cloudy. 10. Very fine morning: fine day. 11. Hoar frost: very foggy day: cleared off at night. 12. Hoar frost: fine day. 13. Fine. 14. Foggy. 15. Cloudy. 16. Raw cold: overcast. 17. Foggy. 18. Cloudy: atmosphere heavy. 19. A strong bleak wind. 20. Bleak. 21. Fine and clear. 22. Fine. 23, 24. Cloudy. 25. Cloudy and fine. 26. Fine: bright moonlight night. 27. Fine: clear. 28. Hoar frost. 29, 30. Fine. 31. A little snow in the afternoon.

## RESULTS.

Winds: N, 1; NE, 13; E, 5; SE, 2; SW, 6; NW, 4.

Barometer: Mean height

For the month..... 30.208 inches.

For the lunar period, ending the 5th ..... 29.743

For 12 days, ending the 5th (moon north) ..... 29.590

For 15 days, ending the 20th (moon south) ..... 30.373

Thermometer: Mean height

For the month..... 33.548°

For the lunar period..... 37.724

For 29 days, the sun in Sagittarius..... 38.172

Evaporation..... 0.98 in.

Rain..... 1.36

*Laboratory, Stratford, First Month, 22, 1823.*

R. HOWARD.

# ANNALS

OF

## PHILOSOPHY.

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MARCH, 1823.

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### ARTICLE I.

*Memoir on the probable Situation and Prospects of the Expedition under Capt. Parry. By Henry Edmonston, Esq.\**

(To the Editor of the *Annals of Philosophy*.)

SIR,

Jan. 7, 1823.

THE last ships of the season have long since arrived from Davis's Straits, the year has closed, and time slips on; yet we hear no tidings of our countrymen, who, twenty months ago, sailed on the expedition for the discovery of a North West Passage. The public mind, seldom long alive to any thing, but lately awakened for a moment to this subject by the return of Capt. Franklin, seems again to have sunk into its usual state of repose. Did our adventurers but know how small a share of the general sympathy and attention their situation engrosses at any given period, it would not much tend to cheer the dulness of their long and dreary polar winter. But though in some measure forgotten by their country, they live in the hopes and fears of kindred and friends left behind, and in the anxieties of those who know how to estimate the magnitude and danger of the enterprise, and to do justice to their heroism and intrepidity. The indifference on the part of the public at large is peculiarly ill-judged, for at no stage in the history of this extraordinary project, since it was first set on foot by Cabot, did it ever possess such an intensity of interest as it does at the present

\* Read before the Literary and Philosophical Society of Newcastle-upon-Tyne.  
*New Series*, VOL. V.

moment. To every mind of reflection and sensibility, I do not doubt but that this sentiment will be sufficiently familiar. But for the purpose of rousing, and, if possible, of keeping alive, attention to the subject, it will be the business of the following remarks to place some of the circumstances connected with it in as prominent a point of view as the shortness of the time within which I have to prepare this article will allow. I obtrude no speculations on the importance of a north-west passage. This, together with the history of the various attempts made to effect it, are well known to all who are the least conversant with the study of maritime geography. I may, however, hazard one observation, that, setting aside every other consideration, it does appear to be, under suitable circumstances, an object worthy of the first maritime nation in the world, to take the lead in determining all questions of this kind, even though no other result should follow, than the ascertaining of a geographical fact. Such exploits are in strict accordance with many of our most interesting national associations and habits. Besides, to Great Britain belongs the glory of having first instituted voyages of discovery for the mere purposes of extending the boundaries of natural knowledge, and of benefiting mankind. Moreover, it belongs to the reign of George III. and will transmit that reign to posterity, with a lustre that will throw into shade many of the political events, singular as they have been, which have occurred during the sixty years that it lasted. Much, it is true, had been done previous to the accession of our late monarch towards perfecting the knowledge of the globe, but it had been done with selfish and sordid views, for the sake of national or individual emolument. It was reserved for this country, in our own times, to set an example of beneficence such as the world had never witnessed, viz. the exploring of new and distant regions in order that civilization and happiness might be more widely diffused.\* The north-west expedition then, had it no other claim to our regard, would be entitled to it on the ground of its having been sent out in furtherance of those laudable and enlightened designs. But it also has its own peculiar claims; such, indeed, as are calculated to call forth our utmost solicitude and admiration.

We know that a certain number of our countrymen have now been absent for a long period on a most perilous service; that they have already encountered one hyperborean winter, and must now be amidst the hardships and privations of another; in regions, perhaps, never before visited, certainly never before resided in by Europeans, and under circumstances, though not altogether new, in the highest degree trying and difficult. We

\* It is a fact not undeserving of notice, that the first expedition sent out with the views here stated, sailed shortly after the accession of his late Majesty; while the last, and, perhaps, the only important achievement of the kind remaining to be accomplished, left the country in the year in which he died.

know them to be bold and resolute; zealous and persevering in the discharge of their duty, dreading nothing so much as failure. On the other hand, there is the experience of a former voyage; the prospects thence opened out, a certain share of national reputation hanging on the event, the honour that must accrue from success; nay, that must attach even to failure. In short, a thousand considerations all of them most deeply interesting, and tending powerfully to fix on this expedition the notice of the world, and assuredly to engage our warmest sympathies on behalf of those who have embarked in it. A few conjectures then, respecting the probable situation and condition at present of this brave band, and also as to their prospects for the future, will not, we hope, be considered mistimed at this season; to us one of rejoicing and festivity, to them, probably, one of dreariness and darkness.

Next to the honour of participating in their hazardous pursuit, is the pleasure we derive from following them in our mind's eye, or from tracing their fancied route on the map; joining in their labours, and entering into their difficulties. While thus employed, we feel, as if by taking a part, we lightened their toils; and we at all events discharge the duty we owe them of taking a lively concern in their affairs, the belief of which on their part, we may be assured, helps, perhaps, more than any thing else, to cheer the dull monotony of their abode, and constitutes one of the principal means of enabling them to bear up under the pressure of hardships or misfortunes. Let us then endeavour to follow them, and, if possible, to find them out.

Various situations present themselves in which we may imagine them to be placed, some of them more or less difficult; some of them dangerous. Before pointing these out, however, we shall attempt to assign the limits so far as respects latitude and longitude, within which their course must have lain, and within which they must in all probability be, if alive, and their object still unattained. In doing this, although there be few data of any kind, and still fewer upon which we can approximate with any thing like precision to the spot that they are likely to be now occupying, we can at least say with some certainty where *they are not*; and even this negative knowledge, in the absence of something better, is not wholly unimportant.

It is not easy to guess accurately *where* the entrance from the North Atlantic has been made. The experience, however, of the former voyage—the fact of their not having been traced or heard of by any of the whalers—the views of Capt. Parry himself—and other considerations—lead to the belief that the place of entrance has been sought for, and found, either through Hudson's, Frobisher's, or Cumberland Straits, and thence northward by either Sir Thomas Rowe's Welcome, Repulse Bay, or Fox's Farthest. It will save some trouble if we at once take this for granted.

Cape Chidley in  $60^{\circ}$  north latitude, at the south entrance of Hudson's Straits, and Possession Bay at the south entrance of Lancaster Sound, will consequently be their utmost distance in latitude; while their space in longitude will not here considerably exceed  $20^{\circ}$ , which, at this distance from the equator, is not much. It is not till they reach the latitude of  $66^{\circ}$  or  $67^{\circ}$  that their longitude becomes considerable, extending to Icy Cape, or probably to Cape Prince of Wales; that is, as far as the  $168^{\text{th}}$  degree of west longitude, and comprising not less than from  $80^{\circ}$  to  $90^{\circ}$ ; while the latitude suffers a corresponding diminution, not including, at the furthest, more than  $6^{\circ}$  or  $7^{\circ}$ . This is owing to the particular direction of the land and water. Within the boundaries denoted by these rough numbers, our navigators, in all probability, must be. But we may, if we choose, very much circumscribe these limits; and, for the sake of aiding our speculations, it may be well to do so. We shall, therefore, confine them between the parallels of  $66^{\circ}$  and  $72^{\circ}$ , and between  $80^{\circ}$  and  $168^{\circ}$  west longitude. In this estimate it is assumed, that Capt. Franklin has given to the Copper Mine River a lower latitude than Hearne did; and we may venture to do this with safety, both from what has transpired respecting his voyage, and from a conjecture hazarded with great foresight by Capt. Parry in his Journal. It will also stretch as far north as Prince Regent's Inlet, explored in the last expedition.

On casting the eye over the map, there is one thing that cannot fail to strike the most superficial observer, viz. the near correspondence in latitude (or at least within a very few degrees of each other) of four cardinal points, now ascertained to exist; namely, Fox's Farthest, the mouth of the Copper Mine River, the mouth of Mackenzie's River, and Icy Cape. From well authenticated facts, as well as from many circumstances of a conjectural character, such as the probable direction of currents, tides, &c. it amounts to almost a certainty, that between the latitudes of  $67^{\circ}$  or even lower, and  $75^{\circ}$ , and for the whole longitude already mentioned, there is an extensive surface of water more or less open. Whether continuously so from the North Atlantic to the North Pacific, or whether permanently obstructed by land or ice, remains to be seen. So far as Capt. Parry's former survey goes, there has not yet been discovered any æstuary communicating with it between Cumberland Strait and Lancaster Sound, though it might be expected that a body of water of such magnitude would make itself known by an outlet not liable to doubt or mistake. We are not, however, to lay too much stress upon this; the Sound and the Straits of Gibraltar being familiar examples to the contrary. Still, no passage having been found, full liberty is allowed to presume that Hudson's Bay through Sir Thomas Rowe's Welcome and the neighbouring passages, forms the prolongation eastward of this vast expanse, and that Hudson's Straits generally, as comprehending Frobisher's and

Cumberland Straits, is the main channel by which it communicates with the Northern Atlantic. Through these Straits then we assume that Capt. Parry, agreeably to his own expectations and speculations, made good his entrance in the summer of 1821. The question comes, where is he now? and what is to become of him? It would indeed be most gratifying, could a reply of definite probability be given to this question; were it but to soothe the mind for a time into tranquillity, and to dissipate a portion of that uneasiness which must otherwise be felt. But this is not to be done, and we must be content with such conjectures as circumstances seem to warrant.

It is quite needless to imagine that they could have got through into Behring's Straits the first season. Such a supposition, though favourable to their object, is fatal to their existence; because they must have been either at home, or heard of, long ago. There can be no hesitation then in rejecting this notion.

The *first* supposition we shall hazard, then, is, that during the summer of 1821 they found admission somewhere in the vicinity of the channel already specified, and that favoured by their former experience, the accidental openness of the season, the lower degree of latitude, and other concurrences, and keeping the American coast fast on board, they penetrated to a considerable distance, say beyond the longitude of the Copper Mine River, and that on the return of summer 1822, they recommenced operations, went forward, passing Mackenzie's River, and pushed through into Behring's Straits, either by some outlet north of Cape Prince of Wales, such as Kotzebue's Sound, or by doubling Icy Cape, probably last September. This is clearly the most favourable supposition that can be made. Should it prove correct, we may begin to expect intelligence very soon, or in the course of the ensuing spring, either by the Isthmus of Darien and the West Indies, or we may see them by the East Indies, or Cape Horn, according to the route by which they return, which will doubtless be left to Capt. Parry's discretion.\* We cannot anticipate early news by Kamtschatka and Petersburg, the journey being so long and tedious.

The supposition now made, however, though a possible case, is a barely possible one. The presumptions are all adverse to it. We have placed our adventurers at the end of the first season beyond the mouth of the Copper Mine River, for the sake of making every allowance, rather than because it is probable they could have reached thus far. The probability is, they did not, and this is corroborated by another probability, that they may have been detained a long time, perhaps nearly a season, in doing little more than finding a channel by which they might advance, as happened to a certain extent in the former voyage,

\* From this view it is obvious, that the circumstance of our not yet having heard of them ought not, by itself, to occasion the least disquietude.

while exploring Prince Regent's Inlet. In the second place, it assumes the passage to be almost straight, and free, which assumption has little to countenance it. Thirdly, the new and more circuitous route; and although this may be compensated in some degree by a longer season in a lower latitude, and fewer obstructions than in Lancaster Sound, yet the compensation appears scarcely adequate to the purpose. We cannot make out from any satisfactory data the length of the working season under the parallels in question. In lat.  $51^{\circ}$ , at the south corner of Hudson's Bay, Capt. James found his movements impeded by ice so early as October, and was obliged to lay up for the winter by the end of November; it was June before he could stir, and August before the ship was disentangled, making little more than four clear months, and even during the whole summer, the bay was infested with ice. The entire working period in Lancaster Sound, at least in the meridian of Melville Island, which is exactly that of the Copper Mine River, did not exceed 49 days, according to Capt. Parry. Now the parallels under which we have placed the expedition are upwards of  $15^{\circ}$  north of Charlton Island, where James wintered, and certainly not more than  $7^{\circ}$  south of Lancaster Sound. If then we give three complete months for the open working season, it may be all that can well be allowed. If Capt. Franklin was able to survey 500 miles of coast, it would argue to be sure a pretty long season for active operations, though, on the other hand, the winter appears to have set in upon him so early as the end of August. But here we are all in the dark, not long to continue so we trust. Fourthly, if they kept hold of the American coast, which Capt. Parry deems essential to success, they might have been seen or heard of by Capt. Franklin, had they penetrated to within even 200 miles of the longitude of the Copper Mine River. Yet this of itself is not much; the natives may have destroyed their signals and beacons; besides, they might easily pass unseen by Capt. Franklin. Fifthly, admitting that they did get beyond the Copper Mine River the first season, still there is an unexplored way of equal or greater length lying between them and the longitude of Behring's Straits, which would occupy fully as much, if not more time in exploring. At least so we must conceive of it, especially if we keep in mind that the forward advance of one season is no absolute criterion by which we can judge of the advance made in another. Capt. Parry, not having, during the whole summer of 1820, pushed westward 10 miles beyond the spot to which he reached in 1819. Sixthly, there is a fact stated by this able commander, that the quantity of ice kept increasing as they advanced *westward*; and although he accounts for this with his usual penetration, by the increasing distance from the coast of the ocean, and expects it not to happen after reaching midway, but, on the contrary, that it would decrease as they approached the western sea coast of America, from the well-

known greater mildness of the temperature on the American than on the Asiatic coast; yet it is difficult to pronounce how far this calculation would be found to agree with actual experience. At all events, the accumulation of ice is likely to be sufficient, even in the lower latitude, to cause much obstruction; and to retard the progress so far, as to induce the necessity of wintering for the second time in these arctic latitudes. And this brings us to the second supposition, which we beg to hazard; namely, that they have proceeded beyond the Copper Mine, or even beyond Mackenzie's River during this last summer (1822), and have pushed forward; but though the passage may eventually prove to be pervious, that they have been obstructed by climate, season, and other impediments, before they could launch upon the Pacific by Behring's Straits; and that they are actually now passing the winter at some point short of that desirable goal. In this case no reasonable fears can be entertained for their safety. The wintering on Melville Island has set this question at rest. If all have gone well with them, not a doubt can arise as to their capability of sustaining another winter in a lower latitude, fortified by two years' experience, and doubtless amply provided. Though the risk to health will of course be somewhat greater (as appears by the reiterated apprehensions of Capt. Parry), yet we may confidently hope, that with the earliest return of summer, they will press forward, and penetrate into Behring's Straits, probably by next August or September (1823). In this case, it will be the end of 1823, or the beginning of 1824, before they, or any intelligence of them, can arrive in this country. This supposition embraces all the probabilities of the former one, with others, and the important adjunct of time superadded. It is the one to which we most incline, because it seems to meet many of the objections likely to be urged, and it is effectual for the accomplishment of a north-west passage, if such do exist in the direction they may have taken; while it is consistent with their perfect safety, which more than all is important; even granting that a little beyond the spot where they are now sojourning, they should find themselves impenetrably opposed by ice or land, or to have sought for the passage in a wrong direction, an occurrence, by the way, not unlikely to happen, they may still get back in the course of the ensuing summer; and by October or November, we may hail their arrival.

The next, or third supposition, is not so favourable, involving, as it does, the likelihood of some danger, and the certainty of some difficulty, if not hardship. It is this; suppose, as we have already done, that they last summer reached, and are now wintering somewhere beyond Mackenzie's River, but considerably short of Behring's Straits; and suppose that after having resumed their efforts next summer, and proceeded to some distance, they should not be so fortunate as to find a channel lower

than Icy Cape, and their attempts to pass that promontory be rendered fruitless by ice; in the same manner as at the end of Melville Island, though there appeared to be no want of sea; \* and that they should in consequence be obliged to return the way they went: then comes a question, can they retraverse in half a season; nay, perhaps, in little more than the mere remnant of a season, the space advanced in nearly three seasons?

This question we should hope may be settled in the affirmative, though there be some points connected with it which do not leave us entirely free from doubt. In Lancaster Sound, they sailed back in six days the distance advanced in six weeks, owing to the setting of the current from west to east; so that if upon a more southerly parallel the current set in the same direction, and with the same rapidity, we are unwilling to indulge any misgivings as to their ability to effect the run back, and we shall have the satisfaction of welcoming them next autumn; and though ever so unsuccessful, welcome them we shall; for highly as we may be disposed to account the discovery of a north-west passage, we set an incomparably higher price on the lives of those who have gone in search of it.

But let us imagine unexpected impediments to present themselves; severity of the weather, change in the direction, or increase in the quantity of the ice, a difference in the set or swiftness of the current compared with that in Lancaster Sound, various localities, and other matters of which we can form no adequate idea, rendering it impossible for them to reach the Atlantic in the course of next season: then comes a serious question, Can they subsist for another or a third winter in the polar regions? Are they supplied with provisions, fuel, and clothing, for this purpose? In the former voyage, they were equipped for two years, which, by the bye, was too short, and had nearly proved so, and shows the calculation to have been defective. For how long they have been this time equipped and victualled, we have not learned,† and, therefore, can found upon it no calculation. But if the supposition now thrown out should ultimately prove the correct one, it must be three years and a half from the time of their leaving this country before they can revisit the British shores, or obtain a fresh supply; that is, it will be, not next November, but November, 1824, before they come back. This is a long time for fuel and provisions to last. Under the impression that he might be compelled to winter a second time in Lancaster Sound, Capt. Parry put his people to a reduced allowance of every thing; that is, to two-thirds of the usual navy allowance, a measure which led him to entertain some uncomfortable anticipations, regarding its effect on health as well as the effects of so long a deprivation of fresh animal and

\* Cook and Clerk found themselves opposed about Icy Cape by ice in July and August.

† It has been said three years.

more especially of fresh vegetable aliment. The bad consequences of a third winter, therefore, might be fearfully augmented, were there not reason to expect that the supply of game will be more abundant further south than it was in Melville Island. Cartwright, in Labrador, shot plenty of deer and bears, even in the depth of winter, long and severe though it was. There will be besides, somewhat less gloom and darkness, and altogether a shorter winter. There will, probably, too, be more of esculent vegetables, and we have the satisfaction of knowing, that Donkin's preserved animal food, of which their stock must be abundant, undergoes no deterioration by any length of keeping to which it has yet been subjected. The article of fuel gives us more concern. One thing, however, is certain; that from the skill, prudence, and resource, of the commander; and from the zeal and devotedness of his followers, every thing may be expected. Thus we may still cherish well grounded hopes that they will be able, inured to the climate as they must then be, in some measure, to withstand even a third polar winter, should it be necessary. And this will tend to relieve us from much anxiety as to the preservation of their lives.

The views already taken have gone upon the principle that all is to continue prosperous (God grant it may!), so far as respects their health, subsistence, and means of protection, against the rigour of the climate; and without taking into the account any untoward accident. But let such occur, a deficiency in their resources from damage to the ships, injury to the provisions and stores, sickness disabling them from exertion; what sort of predicament would they then be in! What complicated miseries may they be made to endure! It is possible, then, and, perhaps, not so improbable as could be wished, that, according to the fourth and last, and worst supposition of all, they may have been overtaken by some calamity; their ships wrecked, or cut up by the ice; their stock of every thing wholly or partially destroyed; themselves thrust out with such means as the moment supplied, to find their way over frozen wastes, in a climate destructive to every thing that lives, save foxes, and wolves, and bears! From such a view, however distant, the imagination involuntarily recoils, and would gladly take shelter, if it could, in the brighter prospects already held out. But difficulties and dangers to be overcome must be looked, as our gallant heroes will look at them, full in the face. Some faint conception may be formed of such a situation from what appears to have befallen Capt. Franklin; with this difference, that *their* hardships would be enhanced tenfold, inasmuch as they would be ignorant of the country, and unable to avail themselves of its resources. It is then not impossible but that, in the course of the voyage, they may be plunged into this miserable state, perhaps at no great distance from Hudson's Bay; perhaps at some

two or three small ships of war on different routes; one to make for Sir Thomas Rowe's Welcome, or Repulse Bay; another for Fox's Farthest; and a third to look into every creek and corner on the coast of Baffin's Bay, as far even as Lancaster Sound or further; though there be no great likelihood of the entrance being made this time so much to the northward.

Why also might not the Davis's Straits' Whalers be encouraged by a bounty to sail a few weeks earlier than usual, and to employ the time in exploring the coast all the way to the fishing ground? In a word, suppose any thing and every thing to be done most likely to promote the great objects in view; namely, the preservation of our dear countrymen and the character of our country.

Should the apprehensions set forth in this memoir be treated as chimerical, I can only express my hearty prayers that the event may prove them to have been so; or should it be objected that the proposed measures would be premature, the time not having yet arrived when they may be called for, I reply, *that it is not the time to send help when it is needed.* Months must elapse before it can by possibility reach them, and months or weeks, nay days, are too precious when the time of their trouble comes. Every one will allow, at least, that the aid had better come too soon than too late; and that hundreds of thousands of pounds had better be expended in superfluous precautions than that a single man should perish by neglect or delay.

I trust that the department of the public service to which the management of all this affair has been exclusively confided, is fully and feelingly alive to the duty which it has to discharge on this occasion. Nay it is by no means improbable, that the measures now suggested have long been in contemplation. Most unfeignedly shall I rejoice should this prove true. But public boards cannot find leisure to attend to every thing; and I am so unreasonable as to think, that on such an occasion, we ought, if possible, to be prepared for all chances. We cannot forget that La Perouse might have been saved or heard of had ships been timely sent out in search, instead of waiting years beyond the extreme period allowed for the fulfilment of his instructions. But France was then in her political agony. For her, therefore, there was the appearance of excuse or palliation. For us there can be none. But even as it is, the name of Perouse can never be uttered or thought of without feelings of the deepest sympathy, regret, and indignation. Besides, nothing satisfactory could be anticipated from the labours of those sent out under Entrecasteaux; for they were evidently more engrossed by their party squabbles, than with the noble object of their search; and, in fact, when the commander died, they actually quarrelled on these paltry grounds, and separated. Again, the very character and mode of conducting the expedition were

sufficient to ensure disappointment. Their purpose was; or ought to have been, *search*, and *nothing else*; to touch or look in every where; to rest no where a moment longer than was indispensable. Instead of this, they were provided with naturalists, astronomers, geographers, engineers, and all the paraphernalia and *instructions* suited to a voyage of *discovery*; and consequently spent much of their valuable time in details foreign to that sacred duty which should have occupied their sole and undivided attention. And to crown the whole, the two ships were dispatched *together*; instead of taking separate routes, by which the chances of gaining their object would have been at least doubled. However, not to dwell any longer on the mistakes of our neighbours, let us endeavour to profit by them, and above all, let not the bitter reproach lie against this country of having "left undone that which she ought to have done."

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## ARTICLE II.

### *Astronomical Observations, 1822, 1823.*

By Col. Beaufoy, FRS.

#### *Bushey Heath, near Stanmore.*

Latitude  $51^{\circ} 37' 44.3''$  North. Longitude West in time  $1^{\circ} 30' 33''$ .

Jan. 23. Emerſion of Jupiter's ſecond ſatellite.....	{	6 <sup>h</sup> 46' 39.0" Mean Time at Buſhey.
Jan. 24. Immersion of $\gamma$ Gemini by the moon.....	{	6 48 00.0 Mean Time at Greenwich.
Feb. 13. Emerſion of $\lambda$ Piſces by the moon.....	{	5 58 05.5 Mean Time at Buſhey.
	{	6 16 55.0 Mean Time at Buſhey.

*Memorandum.*—The greateſt degree of cold laſt month at this place was  $16^{\circ}$  below freezing, and this occurred on the morning of the 20th.

On the 19th, when the thermometer here ſtood at  $17\frac{1}{2}^{\circ}$ , it ſunk at Buſhey Grove, the ſeat of David Haliburton, Eſq. to zero. Buſhey Grove is about 200 feet lower than Buſhey Heath.

## ARTICLE III.

Meteorological Register kept at Helston for 1822. By M. P. Moyle, Esq.

1822.	Barom.	Com. Ther.	Register. Max. Min.	Rain.	Wind.	Strength.	Weather.	Therm. in sun.	1822.	Barom.	Com. Ther.	Register. Max. Min.	Rain.	Wind.	Strength.	Weather.	Therm. in sun.
Jan.	1 29-755	47-00	—	—	—	—	—	—	Feb.	1 30-060	48-66	—	—	SW	—	—	—
	2 29-810	42-33	—	0-14	NW	Boisterous	Heavy rain	—		2 29-626	32-00	—	0-01	SW	—	—	—
	3 29-550	45-00	—	0-08	NW	Boisterous	Hail showers	—		3 29-500	46-00	—	0-47	W	—	—	—
	4 29-360	41-60	—	0-33	SW	—	Rain	—		4 29-413	47-30	—	—	SE	—	—	—
	5 29-940	39-60	—	—	NE	—	Cloudy; fine	—		5 29-516	48-00	—	0-36	W	—	—	—
	6 30-040	35-00	—	0-07	NE	—	Fine; showers	—		6 29-926	47-30	—	—	—	—	—	—
	7 29-950	40-60	—	0-03	NE	—	Frost; cloudy	—		7 29-606	50-06	—	—	SW	—	—	—
	8 30-130	40-30	—	—	NE	—	Hail; fine	—		8 29-753	48-60	—	0-37	SW	—	—	—
	9 30-186	41-60	—	0-07	NE	—	Cloudy	—		9 29-570	52-30	—	—	S	—	—	—
	10 30-186	46-66	—	—	NW	—	Cloudy; fog	—		10 29-706	46-60	—	0-37	W	—	—	—
	11 30-286	49-33	—	—	NW	—	Cloudy	—		11 29-930	41-00	—	0-09	N	—	—	—
	12 30-326	51-00	—	0-06	NW	—	Cloudy; rain	—		12 30-076	41-00	—	—	SE	—	—	—
	13 30-350	49-33	—	0-04	W	—	Cloudy; hazy	—		13 29-953	48-60	—	—	S	—	—	—
	14 30-410	49-33	—	0-02	NW	—	Slight clouds	—		14 29-880	53-00	—	—	SE	—	—	—
	15 30-336	49-00	—	—	NW	—	Cloudy	—		15 30-043	51-30	—	—	SE	—	—	—
	16 30-183	48-00	—	0-09	NW	—	Showery	—		16 30-330	50-00	—	—	S	—	—	—
	17 30-246	47-30	—	0-06	N	—	Cloudy	—		17 30-335	54-00	—	—	W	—	—	—
	18 30-400	44-30	—	—	NW	—	Cloudy; rain	—		18 30-315	53-60	—	—	SW	—	—	—
	19 30-376	48-30	—	0-02	W	—	Cloudy	—		19 30-346	47-30	—	—	NW	—	—	—
	20 30-273	50-60	—	0-02	NW	—	Cloudy; hazy	—		20 30-150	48-00	—	—	NW	—	—	—
	21 30-350	47-00	—	0-01	NW	—	Cloudy; fine	—		21 30-376	44-00	—	0-46	N	—	—	—
	22 30-350	48-60	—	—	W	—	Cloudy	—		22 30-216	50-30	—	—	SW	—	—	—
	23 30-116	51-00	—	—	S	—	Cloudy; rain	—		23 30-293	46-30	—	—	SW	—	—	—
	24 29-986	50-66	—	0-21	WNW	—	Rain	—		24 30-243	51-30	—	—	W	—	—	—
	25 30-216	48-30	—	—	N	—	Cloudy	—		25 30-220	50-30	—	—	SW	—	—	—
	26 30-276	49-30	—	—	NW	—	Fine	—		26 30-193	50-00	—	—	SW	—	—	—
	27 30-276	50-30	—	0-12	NW	—	Rain	—		27 30-550	48-00	—	0-04	SW	—	—	—
	28 30-253	49-00	—	—	NW	—	Cloudy	—		28 30-390	42-30	—	0-02	N	—	—	—
	29 30-250	48-50	—	—	N	—	Cloudy; rain	—		29 30-390	42-30	—	—	SSE	—	—	—
	30 30-313	48-00	—	0-08	NE	—	Fine	—		Mean	30-018	48-46	—	—	—	—	—

1892.	Barom.	Com. Ther.	Register. Max. Min.	Rain.	Wind.	Strength.	Weather.	Therm. in sun.
Mar.	1.30-176	50-60	—	—	SSW	—	Fine	—
	2.30-173	53-00	—	—	S	—	Cloudy; rain	—
	3.30-153	50-68	—	0-03	SSW	—	Cloudy; fine	—
	4.29-883	50-30	—	0-47	SW	—	Rain	—
	5.29-910	58-00	—	0-07	SW	—	Rain	—
	6.29-670	54-24	—	0-44	SW	—	Rain	—
	7.29-690	46-60	—	—	W by S	—	Showery	—
	8.29-683	46-30	—	0-16	W by S	—	Cloudy; showers	—
	9.29-820	53-30	—	0-11	SW	—	Cloudy	—
	10.29-746	55-30	—	0-4	SW	—	Cloudy	—
	11.30-016	49-00	—	—	W	—	Cloudy	—
	12.30-180	48-30	—	—	W	—	Fine	—
	13.29-873	53-60	—	—	SE	—	Fine	—
	14.29-993	48-60	—	—	SE	—	Very fine	—
	15.30-113	52-60	—	—	S	—	Cloudy	—
	16.30-070	53-60	—	0-15	SSW	—	Rain	—
	17.30-183	53-60	—	—	SW	—	Misty rain	—
	18.30-320	52-60	—	0-04	NW	—	Very fine	—
	19.30-340	54-60	—	0-03	W by N	—	Rain; cloudy	—
	20.30-293	54-00	—	—	NW	—	Fine	—
	21.30-243	52-40	—	—	NW	—	Very fine	—
	22.30-400	50-20	—	—	SW	—	Very fine	—
	23.29-956	51-03	—	—	SW	—	Very fine	—
	24.29-733	48-00	—	0-05	W	—	Showers	—
	25.29-853	46-00	—	0-20	W	—	Rain	—
	26.30-140	55-57	—	—	SW	—	Foggy; fine	—
	27.30-030	59-50	—	—	SE	—	Fine	—
	28.30-103	53-00	—	0-01	SW	—	Misty rain	—
	29.30-306	53-00	—	—	S	—	Cloudy; rain	—
	30.29-936	51-00	—	0-08	NW	Stormy	Cloudy	—
	31.30-426	44-30	—	—	N	—	Very fine	—
Mean	90-046	51-55	1-88	—	—	—	—	—

22.	Barom.	Com. Ther.	Register.		Rain.	Wind.	Strength.	Weather.	Therm. in sun.
			Max.	Min.					
May	130-146	58-3	—	—	—	E by N	Brisk	Very fine	—
	230-050	57-6	—	—	—	E	Brisk	Very fine	76-0
	329-726	60-3	—	—	—	E	Mild	Cloudy	78-0
	429-508	62-6	—	—	—	E	Moderate	Very fine	91-0
	529-566	63-6	—	—	0-05	SE	Mild	Showers	91-0
	629-620	66-0	—	—	0-02	SE	Mild	Fine	99-0
	729-726	64-0	—	—	—	EWS	Variable	Thunder; cloudy	96-0
	829-630	66-3	—	—	—	NE	Fresh	Very fine	86-0
	929-580	57-3	—	—	—	S	Brisk	Gloomy; rain	86-0
	1029-353	51-3	—	—	0-12	N	Brisk	Cloudy; rain	81-0
	1120-410	56-3	—	—	0-18	SE	Brisk	Cloudy; rain	93-0
	1229-306	54-6	—	—	0-17	N	Fresh	Cloudy; showers	78-0
	1329-350	57-0	63	47-0	—	N	Fresh	Fine	86-0
	1429-360	58-3	67	48-0	—	NW	Gentle	Very fine	91-0
	1529-396	55-6	60	49-0	—	NW	Brisk	Fine; fog	85-0
	1629-316	59-0	64	43-0	—	N	Mild	Fine	87-0
	1729-943	58-3	65	49-0	—	N	Fresh	Very fine	84-0
	1830-040	53-6	60	48-0	—	NW	Brisk	Very fine	88-0
	1930-040	55-3	64	48-0	—	NW	Moderate	Very fine	88-0
	2030-116	57-3	64	47-0	—	NW	Brisk	Very fine	80-0
	2130-226	57-3	65	51-0	—	NW	Very fresh	Very fine	86-0
	2230-216	61-0	69	47-0	—	E	Gentle	Very fine	89-0
	2330-043	58-3	63	57-0	—	E by S	Brisk	Fine	82-0
	2429-306	56-0	64	50-0	—	E by N	Very brisk	Very fine	88-0
	2529-833	60-0	67	48-0	—	E by N	Fresh	Very fine	88-0
	2629-836	54-0	59	53-0	0-14	SW	Fresh	Fine; rain	80-0
	2730-026	59-0	64	46-0	—	SW	Gentle	Fine; rain	78-0
	2830-180	56-0	65	52-0	0-14	NW	Gentle	Cloudy	77-0
	2930-226	59-3	65	48-0	—	SW	Gentle	Very fine	70-0
	3030-230	60-3	67	52-0	—	SW	Gentle	Very fine	72-0
	3130-250	60-6	66	51-0	—	W	Gentle	Very fine	83-8
Mean 29-378			58-2	49-3	0-82				
Mean 30-029			64-35	71-8	1-93				

1822.	Barom.	Com. Ther.	Register. Max. Min.	Rain.	Wind.	Strength.	Weather.	Therm. in sun.
July	130-103	60-3	66	55	—	NW	Brisk	74-0
1	29-920	60-6	68	49	0-03	SW	Gentle	75-0
2	29-960	61-0	68	58	0-03	SW	Gentle	77-0
3	29-890	60-6	73	59	—	S	Cloudy	72-0
4	29-773	61-6	68	56	0-36	NW	Rain	65-0
5	29-943	59-6	66	52	—	NW	Brisk	67-0
6	29-136	62-6	67	60	—	NW	Fine	67-0
7	29-103	60-6	66	54	—	NW	Brisk	77-0
8	29-993	64-6	75	58	0-04	SW	Brisk	88-0
9	29-896	63-0	69	56	0-27	SW	Gentle	83-0
10	29-610	60-6	67	50	—	S	Brisk	82-0
11	29-680	56-6	62	50	0-67	NW	Boisterous	72-0
12	29-993	63-3	68	60	—	SW	Brisk	80-0
13	29-903	67-6	74	59	0-17	SW	Gentle	84-0
14	29-756	60-6	62	57	1-02	E	Rain	83-0
15	29-780	59-6	68	50	0-39	NW	Brisk	82-0
16	29-640	62-0	69	59	—	SE	Brisk	89-0
17	29-570	64-0	70	57	0-25	SE	Brisk	82-0
18	29-513	63-6	67	58	0-08	SW	Fresh	89-0
19	29-363	63-0	67	56	0-41	SW	Very fresh	94-0
20	29-570	63-6	67	58	—	W	Fresh	101-0
21	29-726	62-0	67	59	0-08	SW	Fresh	85-0
22	29-550	64-0	68	59	0-19	SW	Gentle	87-0
23	29-586	60-3	63	53	0-45	SW	Brisk	69-0
24	29-676	61-6	67	53	0-24	SW	Fresh	76-0
25	29-730	61-6	69	50	—	SW	Gentle	76-0
26	29-570	60-3	65	55	0-60	SE	Heavy rain	79-0
27	29-446	60-6	65	51	1-03	SW	Heavy rain	73-0
28	29-540	60-3	65	52	0-36	SW	Fine; showers	75-0
29	29-650	57-0	61	50	—	NW	Very fine	83-0
30	29-736	56-0	66	51	0-15	NW	Rain; fine	82-0
31	29-741	61-4	66	55	6-74	—	—	80-3
Mean	29-883	60-9	67-3	51-5	1-65	—	—	—



1822.	Barom.	Com. Ther.	Register.	Rain.	Wind.	Strength.	Weather.	Therm. in sun.
129-7160	56.6	62.0	55.0	0.33	S	Boisterous	Rain	58.0
129-7686	55.3	63.0	49.0	0.11	SW	Boisterous	Rain; cloudy	70.0
130-1820	50.6	65.0	42.0	0.13	W	Fresh	Showers	71.0
430-3336	50.3	55.0	50.0	—	W	Brisk	Fine	74.0
530-3253	51.3	56.0	52.0	0.01	SW	Fresh	Cloudy	63.0
630-1793	50.8	56.0	51.0	—	S	Gentle	Cloudy	66.0
729-9306	54.0	57.0	50.0	—	SW	Gentle	Cloudy	64.0
829-9980	51.0	58.0	48.0	—	NE	Gentle	Cloudy	62.0
929-7913	53.6	58.0	47.0	0.14	SE	Gentle	Rain	58.0
1030-1438	58.6	53.0	46.0	0.55	NW	Very fresh	Rain; fine	57.0
1130-1900	53.3	57.0	44.0	—	S	Very brisk	Cloudy	56.0
1230-0130	52.6	53.0	39.0	—	S	Brisk	Rain	53.0
1320-9826	52.0	53.0	43.0	0.32	SW	Brisk	Rain	67.0
1429-5283	49.3	53.0	43.0	0.23	SW	Stormy	Heavy showers	58.0
1529-3920	45.6	49.0	40.0	0.30	SW	Boisterous	Thun.; showers	49.0
1629-4493	45.6	48.0	39.0	0.05	N	Fresh	Showers	61.0
1729-6160	50.6	53.0	48.0	0.05	SW	Very brisk	Showers	54.0
1829-7526	51.0	54.0	50.0	0.20	W	Moderate	Small rain	34.0
1929-7680	53.3	55.0	50.0	0.01	SW	Stormy	Cloudy	59.0
2029-6380	50.6	53.0	44.0	0.68	SW	Gentle	Heavy rain	55.0
2129-8306	47.3	50.0	46.0	0.09	SW	Brisk	Showery	64.0
2229-7166	52.6	53.0	46.0	0.31	SW	Boisterous	Rain	52.0
2329-7346	49.3	51.0	47.0	0.31	SW	Boisterous	Showery	59.0
2429-4400	50.8	55.0	47.0	0.21	SW	Very brisk	Heavy showers	57.0
2529-5440	50.0	54.0	46.0	0.89	SW	Stormy	Heavy showers	58.0
2629-6993	47.0	50.0	41.0	0.10	W	Very fresh	Small rain	55.0
2729-5106	47.0	52.0	41.0	0.26	W	Stormy	Heavy showers	59.0
2829-4616	43.3	47.0	36.0	0.63	SW	V. boister.	Heavy showers	54.0
2929-6106	44.6	48.0	41.0	0.31	W	Boisterous	Heavy showers	54.0
3029-7900	50.33	53.9	45.9	6.62	—	Stormy	—	59.3
Mean	30-7058	33.9	31.1	43.7	33.9	1.69	—	49.3

1822.	Barometer.				Register Thermometer.			Reg. Therm. in Sun.			Wind.								Weather.				
	Maximum observed.	Minimum observed.	Mean of the month.	Monthly range.	Common thermometer monthly mean.	Maximum observed.	Minimum observed.	Monthly mean.	Maximum observed.	Minimum observed.	Monthly mean.	North-east.	East.	South-east.	South.	South-west.	West.	North-west.	Prevailing winds.	Wet days.	Dry days.	Rain in inches.	
January ...	30.4200	29.1500	30.3140	01.2700	46.54	—	—	—	—	—	—	3	8	0	0	1	2	3	14	NW	11	20	1.45
February...	30.3800	29.1800	30.0180	01.4000	48.46	—	—	—	—	—	—	3	0	0	5	4	9	5	2	SW	9	19	2.19
March....	30.4500	29.6200	30.0460	00.8300	51.55	—	—	—	—	—	—	1	0	0	2	6	9	8	5	SW	13	18	1.88
April....	30.4300	29.1000	29.8580	01.3300	51.88	—	—	—	—	—	—	6	3	2	1	4	4	1	9	NW	16	14	3.07
May.....	30.2600	29.2300	29.5780	01.0300	53.20	69.0	43.0	57.1	82	43.0	76.8	7	1	8	3	0	5	3	5	N	7	24	0.82
June.....	30.2000	29.6300	30.0290	00.5500	64.35	84.0	52.0	64.2	99	52.0	70.2	1	1	12	4	1	4	0	7	E	12	18	1.93
July.....	30.2200	29.1600	29.7410	01.0600	61.40	75.0	49.0	60.5	88	49.0	64.8	0	0	1	3	2	14	2	9	SW	19	12	6.74
August....	30.2300	29.4500	29.8330	00.7800	60.90	77.0	44.0	59.4	101	44.0	76.0	2	1	5	0	4	9	4	6	SW	15	16	1.65
September.	30.2000	29.1800	29.8580	01.0200	58.09	72.0	41.0	55.7	93	41.0	62.0	1	5	11	1	2	6	1	3	E	9	21	1.41
October....	30.0720	29.1460	29.9397	00.9260	53.95	65.0	39.0	52.34	83	39.0	59.54	2	1	4	4	6	9	2	3	SW	21	10	4.86
November..	30.3800	29.3060	29.7900	01.0740	50.33	63.0	36.0	49.94	74	36.0	59.30	1	1	0	1	4	17	5	1	SW	25	5	6.62
December..	30.5000	29.0780	30.0558	01.4220	39.91	51.0	26.0	38.50	62	26.0	44.0	1	1	1	5	2	1	4	2	SE	10	21	1.60
Annual Means, &c.	30.32416	29.2708	29.9258	01.0576	53.796							2831	4820	3592	3669	SW	167	198					34.31

*Barometer.*

Highest, Feb. 27.	Wind, N.....	30.5800
Lowest, Dec. 2.	Wind, SW.....	29.0780

*Register Thermometer.*

Highest, June 5.	Wind, SE.....	84°
Lowest, Dec. 16.	Wind, NE.....	26

*Register Thermometer in the Sun.*

Highest, Aug. 21.	Wind, ENE.....	101
Lowest, Dec. 16.	Wind, NE.....	26

*Common Thermometer.*

Highest, June 5.	Wind, SE.....	83
Lowest, Dec. 20.	Wind, NE.....	28

Wet days comprehend rainy, showery, snowy, and those in which there was a fall of hail.

My pluviometer is situated on the top of a chimney, 30 feet above the ground, and free from the operation of any local circumstances.

*January.*—The winter has been remarkably mild; no snow, and only a few showers of hail and sleet, which were dissolved as fast as they fell. The last 24 days of this month, the barometer stood uniformly (one day excepted) above 30 inches. If the addition for the elevation of the barometer was made, the last 27 days of the month, the mercury stood above 30 inches.

*February and March.*—These months also were remarkably mild. Only one day on which we had any frost.

*April.*—This month was rather stormy, with many hail showers.

*May.*—This month was remarkably fine; many days excessively hot for this season of the year.

*June.*—A very fine month. Heat very oppressive, particularly on the 5th.

*July.*—More rain in this month than has ever before been recorded, being 19 days rain.

*August and September.*—Generally speaking fine months.

*October.*—Very stormy on the 7th and 8th; wind from the SW.

*November.*—Several days in this month particularly rough; blowing very strong on the 23d, 24th, 25th, and 28th. The latter day was attended with very sudden squalls, raging to a perfect hurricane, and in a minute subsiding, to be renewed with equal violence. On the evening of this day, there was a very beautiful lunar rainbow.

*December.*—Generally speaking very mild. A few days sharp frost, with gentle wind: 23 days of this month, making the proper addition, the barometer stood more than 30 inches high.

N. B. As my house, or rather the site of my barometer, is situated 105·9 feet above the sea level, it must be necessary to add 0·104 inch to all the barometrical heights in the foregoing tables for the correct heights of mercury. The tables contain the means of three daily observations; viz. 8 p.m.; 1 a.m.; and 10 a.m. The most prevailing wind of the 24 hours is only given; and the register thermometer placed in the sun is insulated six feet from any thing capable of reflecting heat, and the scale of it is marked on the glass tube itself.

Helston, Jan. 4, 1823.

M. P. MOYLE.

## ARTICLE IV.

### *Essays on the Construction of Sea Harbours.*

By Mr. J. B. Longmire.

(To the Editor of the *Annals of Philosophy*.)

SIR,

Whitchaven, Jan. 9, 1823.

THIS subject being new to the public, you may deem the following essays worthy of insertion in the *Annals*, more especially as you occasionally have papers in it on civil engineering.

The matter under consideration naturally falls under two parts; namely, entrances into sea harbours; and the situations and relative positions of the piers of such harbours.

### I. *Of Entrances.*

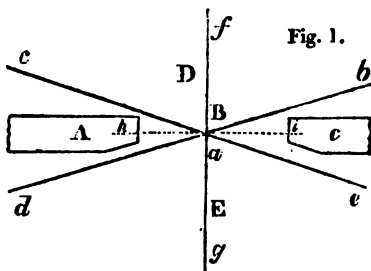
All entrances into sea harbours may be classed under two heads: in the first, they face the sea, and admit the surf;\* and in the second, they face the calmest quarter, while the surf passes to the lee shore without entering the harbour.

In constructing a harbour to make a proper entrance is a very important task; as a harbour that is safe within loses much of its value, if not accessible in every wind that a ship can approach it from the main ocean.

Before describing the theory of entrances, it is necessary to show how vessels approach a harbour in different winds. A ship can sail best with the wind, and her course to any object to the leeward is in a straight line; and although she cannot sail directly against the wind, she can either reach or pass any object to the windward, by making alternate approaches, which have angles of  $6\frac{1}{2}$  points, or  $73^{\circ} 7' 30''$ , with the wind: this is called tacking. When a vessel is sailing with the wind from the

\* The surf is a term applied to express the state of the sea's surface near the shore in a high wind, or in a gale; and signifies the rapid succession of great waves that pass to and strike the shore.

north point, having her bowsprit N,  $73^{\circ} 7' 30''$  W, she is on the starboard tack; and on the larboard tack, if in the same wind, her bowsprit point N,  $73^{\circ} 7' 30''$  E. Thus if a vessel sail along the line  $ce$ , fig. 1, to  $a$ , the wind blowing in the direction  $ga$ , she is on the starboard tack, the line  $de$  making an angle of  $73^{\circ} 7' 30''$  with the line  $ga$ ; and for the same reasons, in the same wind, she is on the larboard tack, when sailing along the line  $ba$ . Hence by sailing with the wind, or by tacking, a vessel can pass from any one place to any other; and of course in one way or the other she approaches a harbour.



As that entrance which presents itself to the open sea, and admits the surf, can be passed by vessels in all winds; and easier in any wind than the other kind of entrance, I will first treat of it. Let B, fig. 1, be this entrance into the harbour E; D the open sea; A C two piers forming the exterior wall of the harbour;  $hi$  a line passing through the middle of the piers; and  $gaf$  another line at right angles to them, and in the middle of the entrance meeting with the line  $hi$ . Now the wind blowing in the direction  $gd$  is the most adverse to a ship approaching the entrance B from the open sea; and, as before shown,  $ba$  and  $ca$  are the lines of approach for this wind. These lines  $ba$  and  $ca$  make an angle of 13 points, or  $146^{\circ} 15'$ , with each other; and, being the larboard and starboard tacks for the most contrary wind, they have within them all the necessary lines of approach for winds from all points of the compass; and the harbour E having in front of its entrance 13 points, or  $146^{\circ} 15'$  of clear sea room, is accessible in any wind whatever. \*

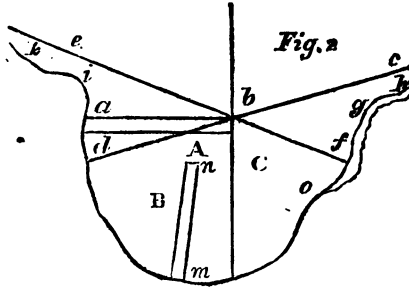
The directly contrary wind, and winds within a point on each side of it, would be in gales the worst of all to enter with; but, that coming from the adjoining land, they have not space to raise a high sea near the harbour; otherwise a vessel in attempting to enter against a gale out of the harbour-mouth, would, to a certainty, be driven out to sea again.

Vessels passing through entrances of this kind can in all winds take shelter within the pier-heads without any assistance from harbour boats to draw them into the entrance, and in gales from any quarter are not, as in entrances of the other kind, in danger of being driven on the lee shore, provided they are in a good sailing condition and come to the proper place to make the entering tack in side winds. But if vessels arriving in distress by violent storms, by negligence, or want of local knowledge of

the commanders, miss the harbour altogether, the loss in consequence is not chargeable upon the entrance.

Entrances that face the calmest quarter, and that do not receive the surf, are at right angles, or nearly so, to the main shore. Let

A, fig. 2, be such an entrance to the harbour B, facing the high shore *o g*; *a b* is the main pier, and *n m* the inner pier. The exterior lines of approach, *e b*, *c b*, are obtained by allowing such space in front of the shore *a t*, *i k*, and *g h*, as will keep ships sailing on these lines from rocks and shallow water; and if they have within them  $146^{\circ} 15'$ , then a vessel can reach the pier-head *b* in any wind. But as the side *c* is open in strong sea gales, vessels are some times carried too far past the head *b* to turn into the harbour-mouth A; and the same happens in gales out of the harbour-mouth A: vessels so driven aside are forced upon the contiguous rocks or sandy shore, and are destroyed or much injured. So it has happened after an entrance of this kind has been tried to a large harbour, and where money could be obtained, that outworks have been erected on the exposed side *c*, which assimilate the principle of this entrance, when so modified, very closely with that of the first kind of entrance.



I am, Sir, yours, &c.

J. B. LONGMIRE.

## ARTICLE V.

*On the Geology of Devon and Cornwall.*

By the Rev. J. J. Conybeare, MGS.

(To the Editor of the *Annals of Philosophy*.)

MY DEAR SIR,

*Bath Easton, Feb. 2, 1823.*

THE notice of such geological travellers as first visited Cornwall and Devonshire was of course most strongly attracted by those which may be termed their metalliferous districts, and these still do and must always continue to present the most immediate and interesting objects of such research. Soon also the attention was directed to such points of the coast as are

distinguished either by the magnificence of their scenery, or the extent of the sections and singularity of the phenomena which they exhibit. Many and valuable as the contributions to the geological history of both counties have unquestionably been, yet our knowledge of their structure (especially in the case of such districts as do not fall under the above-mentioned heads) is by no means so perfect as to preclude even in a casual visitor the hope of adding somewhat of information to the present stock. It may be useful too to point out the deficiencies which yet remain to be supplied by those who have better opportunities and leisure. Such is the object of the following memoranda collected chiefly during the summers of 1809 and 1812, and partly verified in that of 1819. On many points they are of necessity very imperfect, and some parts of the original manuscript have been omitted in consequence of the same phenomena having been far more accurately and fully described in the essay contributed by Mr. Sedgwick to the first part of the Cambridge Philosophical Transactions. What is retained, I have arranged for convenience's sake geologically, rather than topographically; and in this arrangement I venture to propose the following division of the principal rock masses as one which, if not strictly scientific, will yet, I think, be found useful for the purpose.

1. Granite, including some porphyritic beds and mineral veins, and shorl rock.

2. Metalliferous, or, more strictly, cupriferous and stanniferous slate, including various porphyritic and felspathic rocks (elvans), and occasionally greenstone. This I will venture for brevity's sake to term the *inferior slate*.

3. Slate (which I shall venture for the same reason to term *superior*), containing no elvans, but abounding much more in greenstone, especially in its obscurer varieties, and in dark coloured limestones. Sparingly metalliferous, containing no tin, and more productive of lead than the inferior. Contains occasionally organic remains.

4. Stratified rock, exhibiting the general character of a conglomerate or sandstone, alternating with tender slate, and occasionally associated with coralline or shelly limestone. Contains no metallic veins, and few if any rocks of the greenstone species. This rock might, perhaps, be regarded as forming the upper portion of No. 3, and both would probably by most geologists be termed greywacke. As any attempt at restricting that term might produce confusion, I shall venture for the present to term this (No. 4) sand slate.\*

\* It is, perhaps, almost needless to remind those who are practised in geological research, that the division of slates here adopted is purely arbitrary, and, for convenience, as the inferior do in fact pass by so imperceptible a gradation into the superior as to render it impossible to assign any determinate limits to each variety, however an examination of points distant *inter se* may satisfy us of their characteristic difference. It appears to be pretty generally agreed that such a gradation is observable in most of our schistose ranges.

1. *Granite*.—The character of this rock is, so far as I am acquainted with it, remarkably uniform through the whole extent of its range. That of Waterloo Bridge may serve as a specimen.\* Near the points of junction with the incumbent slates, it occasionally becomes small grained, and of a redder hue, resembling the granite of veins. Examples may be found at a junction near Ivy Bridge, at Buckland on the Moor, above Belstone (near Okehampton), and near Bovey Tracey at the spot which produced the fine specimens of tourmaline and apatite. As noticed by Mr. Sedgwick in these cases, there appears to be a diminution, sometimes a total loss, of the mica. The predominant variety of granite contains also in many places patches of a smaller grain, generally of a form more or less spherical. These differ so much in their aspect from the general mass, that, by a casual observer, they might be taken for imbedded portions of another rock; a more accurate inspection will soon show that this is not the case, even in those instances in which (as near St. Just) the predominance of dark coloured mica or chlorite in these patches gives them much the aspect of a kind of gneiss. Other instances may be found near the Land's End, near Moreton Hampstead, above Henoch, and on the road leading from Bovey Tracey to the depot of tourmalines, &c. already mentioned.†

The disintegration of granite *in situ*, as exhibited on a large scale at the porcelain clay pits in St. Stephen's, and the open mine of Carglaise,‡ near St. Austel, has been often noticed. Another large tract of the same character will be found on Dartmoor, in the neighbourhood of a hamlet termed (from the nature of the soil thus produced) Sandy Park. Other partial instances occur, and the granitic tors, the formation of which has been so ably illustrated by Dr. Macculloch, afford abundant proof that the action of the causes which produced this phenomenon has been at some time or other nearly universal.§ It has, I believe,

\* See Mr. Sedgwick's paper (p. 10). It may here be noticed that the constituent minerals of granite are seldom found in the west (as in many similar tracts) distinctly or separately crystallized. The *imbedded* crystals of felspar have been noticed by Mr. S. and others. I found it crystallized in rhombs in small cavities on St. Michael's Mount, and in small rhombs and in larger crystals of a more complicated form at the tourmaline pit near Bovey Tracey. I have also from Cornwall, but without the exact locality, two specimens of perfectly crystallized mica, the one a rhomboidal, the other an hexagonal tablet. Both are in a small grained reddish granite (possibly an elvan), the aspect of which does not resemble that of the specimens brought from Scilly by Mr. Majendie. I may add, that on St. Michael's Mount, I observed in a highly felspathic portion of the granite insulated crystals of felspar rendered as tender as the softest clay by some process of decomposition which had not affected the imbedding mass.

† Mr. Sedgwick mentions the same phenomenon as observable near Castle Trereen (the Logging Rock): to his remarks I would refer the student for much fuller information.

‡ It is, perhaps, hardly worth while to state, that the finest porcelain clay which I obtained in Cornwall was from a vein in Carglaise. It occurred in very small quantities.

§ Mr. Taylor (Report, p. 1) affords an interesting illustration of one result of this process. "If the ground, 80 fathoms south-east of Carn-bræ, and at Wheal Druid, had not been penetrated by the mines, it is very possible that the whole of it extending

been noticed, that these disintegrated tracts exhibit frequent patches of various dimensions retaining their original compactness. It would be a point of some geological interest to ascertain, as accurately as circumstances permit, the extent to which this disintegration actually obtains, and to enquire whether the tracts in question exhibit any phenomena which might guide our speculations as to its probable cause. That that cause is not to be sought in the mere superficial action of weather and moisture, we may, I think, argue from the great depth to which (as at Carglaise) the disintegration is found to extend. I have heard it suggested, that this state of granitic rocks is to be regarded rather as the mode of their original formation than as the result of causes acting subsequently. This hypothesis, however, seems hardly reconcileable to the appearances presented by Carglaise. Some may, perhaps, incline to view the phenomenon as analogous to the disintegration which is known to obtain largely in crystalline rocks forming acknowledged dykes,\* and to these it may appear to countenance the theory which attributes to granite an igneous origin. At all events, this species of decomposition seems for the most part peculiar to rocks of a crystalline structure. In some cases, if my observation be correct, the granite is traversed by porphyritic dykes, or elvans, similar to those occurring in the slate. I noticed one (in company with Mr. Buckland) in 1812 on the summit of Kitt Hill, near Callington. In some cases too the saalbande, or wall of metalliferous veins traversing the granite, appears to be of this class. At Bean Mine, about one mile east of Roche, the surrounding country is granite of the usual aspect; while the walls of the tin lode which are of considerable thickness have all the character of an elvan dyke, the paste of which is chiefly quartzose with a mixture of mica, talcite, felspar much disintegrated, and crystallized shorl, the tin occurring in veins with shorl towards the centre of this elvan (the veins occasionally send out small lateral branches at various angles), the elvan itself is said to dip three feet in the fathom, and in its general character much resembles some of those found to the east of St. Agnes. The same inclusion (if I may so term it) of a metallic vein by a rock differing from the surrounding granite I recollect to have noticed also at Newbridge, and in the Gwennap cluster. It would be desirable to

to a much greater distance from the top of the hill, would have been taken to be granite, as the surface has every appearance of a grown soil intermixed with granite stones and rocks plentifully scattered about. The same has been observed on the sides of Carn Marth and other granite hills bordering on the Killas country." We were assured that at Newbridge they had sunk above the granite country in a mass of granitic rubble with occasional blocks of killas intermixed to the depth of nearly 30 yards, and above the killas country in a rubble of that rock with a small admixture of killas to about half that depth.

\* See Mr. Henslow's Account of Anglesea. (Cambridge Philosophical Transactions, Part II.)

ascertain how far this is the case in veins which traverse granite.

At Kit Hill above-mentioned, at Carglaise, and at some other places, we were assured by miners apparently of competent information, that the granite did in some places overlie the killas. In these instances, it is possible that granitic dykes may have been mistaken for the central mass, or that our informants might have been misled by some of those deceptive appearances which are familiar to most geologists. Mr. Taylor, in his excellent Survey of the Mining District, mentions an analogous fact, and his observation is too important to be omitted. "In Dolcoath and five other mines situate near the junction, the strata of granite and killas *appear alternately overlying each other*, the divisions between them being at various inclinations;\* but sufficient information cannot be obtained to state with accuracy the particulars."†

On the subject of granitic veins, my memoranda afford nothing which can add to the copious and masterly observations of Mr. Sedgwick. I may mention, however, that the country near the junctions of Carn-brae, of Kit Hill, of the upper part of the river Okement, and of some other spots of the same character,‡ will afford the collector hand specimens well calculated to illustrate on a small scale many of the phenomena which present themselves so fully and magnificently at the cliffs of Tre-wavas and Polmear.

It may here be noticed, that the mineral substances confined to the granite (or rather not yet discovered in the superincumbent rocks also), are few in number. Apatite, pinite, andalusite, and, I believe, uranite, at present complete the list.

*Short Rock.*—This binary compound, too generally known to need any description here, is usually found on the confines of the granite in such large masses as to render us doubtful what name to assign to them. Indeed our geological guides have not as yet furnished us with a very definite or accurate terminology for the purpose. What one observer would describe as a large dyke or patch, another would, perhaps, elevate to the dignity of a formation. Roche has been constantly noticed as presenting a magnificent display of this rock; and Mr. Sedgwick mentions it as occurring in veins traversing the granite between Lemorna Cove and the Land's End (p. 19). I have observed it on the large scale forming the whole of the tor named Carn Mewan,

\* Therefore, if there is any parallelism in the stratification of the killas, the granite must be considered not as an alternation but a vein. At Dolcoath, the floor of granite is said to be nearly horizontal; most of the killas in the neighbourhood is, if I do not err, considerably inclined. (See Thomas's Report, p. 34.)

† Mr. Sedgwick appears to have determined this point as far as our present means of examination permit. (P. 34.)

‡ We obtained some highly illustrative at Costellow's Mine, near Roche.

near St. Austle, and of another tor (the name of which I have lost), overtopping the upper road, which leads to Buckland on the Moor. If my memory does not fail me, I observed it on a scale equally large in more than one other spot on the Cornish Downs, and on Dartmoor; but I either neglected to mark at the time, or have since mislaid the exact localities. I would venture to recommend to the examination of future tourists two points connected with this rock.

1. Whether some other of the tors on the confines of the granitic tracts (besides those specified) do not consist of shorl rock. From its external configuration and neighbourhood to the granite, it may be easily confounded with that rock until examined more closely.

2. Is it possible from the careful examination of the country surrounding Roche Rocks, to ascertain whether that remarkable eminence has been produced by the disintegration and washing away of some less durable beds which once enveloped it? or whether it may be regarded as an original inequality of surface?\*

Most of the varieties of external appearance produced in this rock by the different modes of aggregation, have been enumerated by Mr. Sedgwick (p. 18). To his list, I am enabled to add, as Nos. 5 and 6,

5. Alternate layers generally of great tenuity, of very minutely granular quartz and shorl, having all the aspect of a stratified mass.

6. Real or pseudo breccia consisting of small patches of compact shorl imbedded in quartz, or *vice versa* of quartz imbedded in shorl.

These varieties occur in the neighbourhood of the crystalline and more predominant form. No. 5 appears to offer another exemplification of a law, which I believe to obtain pretty generally, "that crystalline rocks when they occur in large masses are most usually accompanied by schistose rocks composed of the same mineral ingredients in a state of greater alternation." To the verification of this law, I would venture to solicit the attention of those who join a knowledge of mineralogy and chemistry to that of geology. The establishment of its probability might lead to some important theoretical results. Nor should I omit to mention, that the varieties of shorl rock above noticed are peculiarly interesting from the examples which they afford, even in hand specimens, of the various phenomena of configuration incident to rocks of the schistose character. The marked distinction of colour and aspect existing between the two constituents (tourmaline and quartz) render these very striking and intelligible. Even the very small collection which I possess myself, offers within the scale of a few inches highly instructive examples of contortion, dislocation, crossing, and

\* I would suggest a like examination of two singularly insulated masses of rock occupying opposite sides of a ravine near Camelford, known by the name of the Devil's Leap.

heaving of veins. Neither this, however, nor my observations, are sufficiently extensive to justify the theorizing, with any confidence, on the subject. As far as I can conjecture, the appearances offered by my own specimens would be most readily accounted for by an hypothesis which should admit, 1. That the laminated arrangement of the rock is not in all cases the result of successive depositions. 2. That disturbances have taken place subsequently to the formation and contortion (if these be not synchronous) of the laminæ. 3. That at the period of these disturbances, the degree of consolidation varied in different portions of the mass. I would, however, conclude by strongly recommending this rock to the more accurate examination of future travellers; and am, dear Sir, very truly yours,

J. J. CONYBEARE.

## ARTICLE VI.

*On Hatchetine.* By the Rev. J. J. Conybeare, MGS.

(To the Editor of the *Annals of Philosophy*.)

MY DEAR SIR,

Bath Easton, Feb. 10, 1823.

I HASTEN to acknowledge that a perusal of Mr. Brande's elementary work on Chemistry (which has only of late fallen into my hands), has shown me that I have been anticipated by that gentleman in the examination of the mineral substance which I ventured to name *hatchetine*, and which is enumerated by him under the varieties of bitumen, as *mineral adipocire*. Mr. Brande's work was published, I believe, early in 1821. My own experiments, made the autumn before, were transcribed for the *Annals* in the January of that year. Strictly, therefore, I have to apologize to your readers only for the second of my communications (the short note appended to the examination of *mumia*), which an earlier perusal of Mr. Brande's work would certainly have caused me to suppress. Allow me to express my satisfaction, that the examination of the substance in question has fallen into better hands than my own; and that my opinion as to its specific difference from every other known variety of bitumen is corroborated by so competent an authority. With Mr. Brande's permission, however, I would still contend for the superior propriety of the name *hatchetine*, both for the reason formerly assigned, and from the feeling that it is rather desirable to banish from our nomenclature all such *significant* names as are not indicative of some actual property of the substance to which they are applied.

Believe me, dear Sir, very truly yours,

J. J. CONYBEARE.

*Results of a Meteorological Register kept at New Malton, Yorkshire, in the Year 1822. By Mr. J. Stockton.*

1922.	BAROMETER.					THERMOMETER.					WINDS.										WEATHER.			RAIN. Inches, &c.		
	Maximum.	Minimum.	Mean.	Range.	Spaces described in inches and parts.	Number of changes.	Maximum.	Minimum.	Mean.	Range.	North-east.	North.	East.	South-east.	South.	South-west.	West.	North-west.	Variable.	Britk.	Boisterous.	Rainy days.	Snow.		Hail.	
January ...	30-27	29-10	29-870	1-17	6-38	17	50	30°	38-677°	20°	3	2	1	0	0	7	10	6	2	1	1	9	1	0	2-36	
February ...	30-70	28-25	29-784	2-45	11-69	19	54	29	40-714	25	1	0	0	0	0	7	11	5	2	2	4	4	7	0	0	1-70
March ...	30-30	28-80	29-687	1-50	10-13	17	60	29	44-516	31	1	0	0	1	4	13	4	0	9	3	5	2	9	3	3	3-81
April ...	30-34	28-95	29-720	1-39	5-50	10	63	32	46-585	31	4	5	2	6	5	1	3	3	1	5	2	10	1	5	2-73	
May ...	30-35	29-30	29-837	1-05	5-73	16	74	37	53-822	37	0	15	7	2	0	2	2	0	5	2	1	5	0	0	3-36	
June ...	30-17	29-50	29-938	0-67	4-64	15	84	42	61-593	42	6	7	2	2	0	2	2	1	8	1	0	6	0	0	2-40	
July ...	29-87	29-10	29-514	0-77	5-71	15	71	44	59-984	27	3	4	3	1	2	3	13	3	2	4	3	1	13	0	0	6-00
August ...	30-10	29-02	29-616	1-08	4-89	13	75	44	59-615	31	0	3	1	2	3	13	3	2	4	3	1	13	0	0	3-60	
September ...	30-20	29-09	29-764	1-11	5-76	14	68	42	58-633	26	11	7	0	0	0	12	0	0	0	6	1	4	0	0	0-67	
October ...	29-90	28-92	29-396	0-98	7-21	19	63	34	49-209	29	2	2	2	2	2	12	7	2	1	1	6	4	14	0	0	4-32
November ...	30-06	28-51	29-360	1-55	8-58	25	59	30	43-950	29	0	0	0	3	10	13	0	2	2	9	7	20	0	0	3-87	
December ..	30-65	28-03	29-935	2-60	9-63	15	45	25	34-500	20	1	6	5	6	0	8	1	2	2	2	6	0	0	0	1-28	
Annual means, &c.	30-70	28-03	29-703	2-65	85-85	195	84	25	48-901	59	32	51	23	25	41	94	39	26	34	48	27	125	5	8	37-10	

## ANNUAL RESULTS.

### *Barometer.*

	Inches.
Highest observation, Feb. 27. Wind, NW. ....	30·700
Lowest observation, Dec. 5. Var. and tempestuous ..	28·050
Range of the mercury .....	2·650
Mean annual barometrical pressure. ....	29·703
Greatest range of the mercury in December . ....	2·600
Least range of the mercury in June .....	0·670
Mean annual range of the mercury. ....	1·360
Spaces described by the mercury .....	85·850
Total number of changes in the year . ....	195·000

### *Six's Thermometer.*

Greatest observation, June 5. Wind, variable. ....	84·000°
Least observation, Dec. 28. Wind, SE. ....	25·000
Range of the mercury in the thermometer .....	59·000
Mean annual temperature. ....	48·901
Greatest range in June. ....	42·000
Least range in January and December .....	20·000
Mean annual range . ....	29·000

### *Winds.*

	Days.
North and East . ....	55·000
North-east and South-east .....	76·000
South and West .....	80·000
South-west and North-west. ....	120·000
Variable. ....	34·000

### *Rain.*

	Inches.
Greatest quantity in July . ....	6·000
Least quantity in December. ....	1·280
Total amount for the year .....	37·100

### *Observations.*

*Pressure.*—The greatest range of the mercury, which is nearly that for the year, took place in December, and notwithstanding the amount of rain, the spaces described in inches, and the number of changes in the direction of the column, are less than usual; while the mean is higher than for some time past.

*Temperatures.*—The mean temperature for the year just elapsed is 1° higher than that of the preceding one. The months of April, May, August, September, and December, were considerably warmer in 1821 than the corresponding ones of last year; while the three first months, and June and July, are as much the reverse. The means of October and November in both periods are nearly alike.

*Rain.*—The amount of rain exceeds that of last year upwards of eight inches, and is the wettest we have had since 1816. The greatest monthly quantity fell in July, attended on five successive days with most tremendous thunder and lightning.

I am, Sir, your most obedient servant,

New Malton, Feb. 5, 1823.

J. STOCKTON.

## ARTICLE VIII.

### *On Hannibal's Passage through the Alps.*

By Thomas M'Keever, MD.

(To the Editor of the *Annals of Philosophy*.)

SIR,

HARDLY any passage in the writings of Livy has been more frequently the subject of comment than that in which Hannibal is stated, during his celebrated and interesting march through the Alps, to have effected the removal of a large cliff, which impeded the progress of his army, by means of the joint agency of fire and vinegar.

As the particulars of this very remarkable event may have escaped the recollection of some of your readers, it may not be amiss to mention a few of the leading circumstances. Livy informs us that Hannibal having, after a series of the most frightful difficulties and dangers, reached the summit of the Alps, rested there two days, in order to recruit his exhausted troops: that he then commenced his descent, but had not proceeded far, when he came to a projecting cliff of great height, which completely obstructed his further progress. Having in vain searched for a more convenient route, he at length determined on levelling this formidable barrier, and for this purpose ordered a number of large trees to be felled, and placed in a pile at its base: they were then set on fire, and in a short time (the wind proving favourable), so intense was the action of the heat, that the solid rock became as red as the blazing fuel with which, it was surrounded. Hannibal now thought of applying a quantity of vinegar to its surface, which, it is asserted, had the effect of softening its substance, and in this way a passage was speedily effected.

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dily opened, by which his army with their elephants and baggage were enabled to proceed.

Some writers have contended that the action of the acid was the principal, if not the sole agent in producing the effect here described; others suppose that the solid rock was actually fused, by the intense action of the fire on its surface; while another set of commentators, from the difficulties attendant on its explanation, consider the whole statement as an idle fabrication, for which the author had no foundation whatever. As, however, the entire process may, I conceive, be explained on plain and obvious principles, I can see no necessity for calling in question the accuracy of a writer, who "for probity, candour, and impartiality, has been so much distinguished above every other historian." The difficulty of procuring in those wild retreats a sufficient number of trees for making a huge pile such as Livy describes, has been advanced as one of the principal objections to the statement of this historian. We are to consider, however, that although this objection might apply to the very summit of the Alps, where vegetation is nearly, if not altogether, suspended: it is by no means applicable to the sides or skirts of those mountains, which all travellers, and among the rest Polybius, agree in describing, as being in several places clothed with large woods. We are also to bear in mind, that the various passes through those sequestered regions, run, not across the ridges or summits of the mountains, as some have supposed, but that they are conducted through many defiles, and were probably traced out by paths that have served from time immemorial, as means of communication between the fertile valleys that lie interspersed up and down the windings of this immense chain.\* Now as it can be satisfactorily proved, from the testimony of the two principal historians who have recorded this memorable march, that Hannibal never reached the very summit of those dreary solitudes, but that he merely ascended to the top of one of the lesser ridges, it is obvious that he could have had no difficulty in procuring any quantity of fuel that he might have required. In proof that Hannibal never attained the summit of the Alps, it may be observed that Polybius and Livy, although they differ materially as to the route which the Carthaginian general took, agree in stating, that, having completed his ascent, he conducted his troops, exhausted and broken down with the innumerable hardships they had encountered, to a convenient spot, where he pointed out to them the rich and fertile plains of Italy as the reward of all their toils and privations;† but had Hannibal ascended to the upper range of the Alps, it is altogether impossible he could have indulged his army with such a cheering prospect; lesser mountains would have intercepted their view; and instead of smiling luxuriant plains, an immeasur-

\* See Eustace's *Classical Tour through Italy*, vol. i. p. 12.

† See Hook's *Roman History*, vol. ii. p. 126.

able expanse of desolation must have met the eye. Besides, it may be asked, is it at all probable that Hannibal, while traversing an unknown and savage region, where every object, clothed with the dread magnificence of heaven, was calculated to excite feelings of astonishment and terror,\* would attempt to deviate from the ordinary passes pointed out to him by experienced guides: he must have felt convinced that inevitable destruction would be the consequence of such a rash unjustifiable proceeding.

The improbability of Hannibal having at hand a sufficiency of vinegar to effect the decomposition of the rock, has also been brought forward as an insurmountable objection to Livy's account of this achievement. Although I am fully satisfied that the action of the vinegar had no share whatever in accomplishing the destruction of the cliff, still I see no absurdity in supposing that a Carthaginian army should have been supplied with a considerable quantity of this liquid. Several historians inform us, that, from a very early period, vinegar and water constituted almost the sole drink of the military during their long and fatiguing marches: thus Cæsar, on one occasion, while conducting his troops through the Alps for the purpose of encountering a branch of Pompey's army, then in Spain, under the command of Afranius and Petreius, is described as having supported them for several days, principally by means of this simple beverage; and Crevier, in his History of the Roman Emperors, tells us, "that the Emperor Severus had altogether proscribed the use of wine, which had for some time crept in among his soldiers, and reduced them to vinegar and water, which formed the common drink of the military in ancient times."

Having thus briefly stated such arguments as have occurred to me in support of the credibility of Livy's statement, I proceed to explain in what manner I conceive this important undertaking to have been accomplished.

Water, it is well known, will insinuate itself into the minute pores and crevices of the most solid bodies, and being expanded by variations of temperature, is capable either of rending them asunder, or of detaching portions from their surface. Expansion, as is well known, may take place in this liquid from two opposite causes, an increase or a diminution of temperature; that very remarkable enlargement of volume which water undergoes during congelation, is capable of producing very powerful effects, and enables us to explain a number of interesting phenomena. The

\* Nullum ver usquam, nullique ætatis honores;  
Sol jugis habitat diris, sedesque tuetur  
Perpetuus deformis hyems; illa undique nubes  
Huc atras agit et mixtos cum grandine nimbos.  
Jam cuncti flatu, ventique furentia regna  
Alpina posuere domo, caligat in altis  
Obtutus saxis, abeuntque in nubila montes.

(Sill. Ital. lib. 3.)

Florentine academicians were enabled to burst a brass globe, the cavity of which was an inch in diameter, by means of this expansive power, and calculating from the tenacity of brass, and the thickness of the sides of the globe, this must have required a force exceeding 27,720 pounds.\* Vast masses of rock are from this cause frequently detached from the cliffs, and precipitated with great velocity into the adjoining valleys. The rending of trees in northern latitudes during the winter months, the bursting of water pipes, the raising of pavements, the increased fertility of the soil after frost (owing to the facility with which the delicate fibres of the roots of plants are enabled to extend themselves), may all be referred to the same principle. The expansion which water undergoes at elevated temperatures is, however, still more considerable, and is capable of producing the most astonishing effects. When heated to  $212^{\circ}$  of Fahrenheit, at the mean pressure of the atmosphere, it becomes converted into steam, an elastic substance, the bulk of which, even at that temperature, is about 1800 times that of the water from which it originated. When heated beyond this point, vapour is expansible to a wonderful degree. Count Rumford ascertained by incontrovertible experiments, that its elasticity became doubled by every addition of temperature, equal to  $30^{\circ}$  of Fahrenheit's scale. With the heat of  $212 + 30 = 242$ , its elasticity he found equal to the pressure of two atmospheres; at the temperature of  $242 + 30 = 272$ , it will be equal to four atmospheres, and so on.† When suddenly generated, this powerful agent is capable of producing the most violent, and at times the most destructive effects. Thus instances ‡ have occurred, where, in consequence of a person carelessly spitting into a copper foundry, the entire building has been destroyed, and even when the moulds contain the slightest moisture, the melted metal is driven back with a loud report, and is violently dispersed in every direction. A drop of water also by falling into a vessel of boiling linseed oil, where it became instantly converted into vapour, has been known to produce the most violent explosions. On this principle then, namely, that of sudden expansion, I conceive the whole process admits of explanation. The minute, but numerous interspersed globules of water, which had for ages been percolating through the pores and fissures of the rock, undergoing, by the application of a large fire to its base, a sudden enlargement of volume in a short time acquired such a degree of expansive energy as to burst asunder the strong confines by which they had so long been surrounded. Now were Hannibal, while the fire was imparting

\* See Murray's Chemistry, vol. i. p. 250.

† The elastic force of steam does not increase in quite so rapid a progression as Count Rumford affirms; according to the late experiments of Dr. Ure, it is equal only to 53.6 inches of mercury at  $242^{\circ}$ , and to about 89 only at  $272^{\circ}$ .—(See Philosophical Transactions, 1818, or *Annals*, vol. xiii. p. 215.)—*Edit.*

‡ See Black's Lectures, vol. i.

this energetic influence to the particles of confined moisture, to apply a quantity of vinegar to the surface of the rock, there would surely be nothing unnatural in his attributing at least a part of the effect to the liquid he employed, particularly when we take into account the very low state of chemical knowledge, in those early ages, even among men of science.

A phenomenon of annual occurrence in the Polar Seas, bears, I conceive, considerable analogy to the point we have been discussing, and appears to me to confirm the explanation I have ventured to advance; I allude to the frequent disruptions, or icequakes, as they are termed, which take place among the icebergs, on the return of the summer season. In those remote latitudes, as soon as the long and dreary winter has passed away, the hot weather sets in with unusual rapidity, in consequence of which the numerous globules of air, that had been incased in those vast accumulations, soon begin to expand,\* and struggling, as it were, for liberty, in a short time acquire such irresistible force as to occasion the disseveration of the mountain. In this way large masses separate, and are precipitated into the ocean with a tremendous noise, not unlike that of a distant peal of thunder. Volcanic eruptions, explosions in coal mines, and several other phenomena, together with the common operation of blasting, might likewise be adduced to prove the wonderful power possessed by elastic fluids, whether in the form of vapour, or of gas, when exposed to high temperature; but to enlarge upon a principle now so well understood, and so extensively applied, appears altogether unnecessary.

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## ARTICLE IX.

### *On the Presence of Oil in the Serum of the Blood.*

By Thomas Stewart Traill, MD.

(To the Editor of the *Annals of Philosophy*.)

DEAR SIR,

*Liverpool, Royal Institution, Jan. 10, 1823.*

UPWARDS of two years ago, I detected the presence of a considerable proportion of oil in the serum of the blood of a man labouring under internal inflammation; and a second attack of decided hepatitis in the same individual afforded me another opportunity of verifying the observation in the spring of 1821. These facts were communicated to the public in the 17th volume of the *Edinburgh Medical and Surgical Journal*. In both instances, the singularly *white* colour of the serum induced the

\* See Leslie on Heat and Moisture.

198 *Dr. Traill on Oil in the Serum of the Blood.* [MARCH, chemical examination; and a similar appearance having recently occurred in a case under the care of my friend Dr. M<sup>c</sup>Cartney, of this place, he sent to me a portion of the serum, which yielded on analysis a still larger proportion of oil than had been obtained in the former instances. Dr. M<sup>c</sup>Cartney's patient is a stout young man, who was attacked by acute hepatitis.

#### *Examination of the Serum.*

This serum separated spontaneously from the crassamentum; but in appearance, it strikingly resembled that described in the journal above-mentioned. Its colour is a fine yellowish-white; its consistence (in this cold weather) is that of very thick, rich, cream; but it becomes more fluid by a gentle heat. It did not make any deposit, on being left at rest for several weeks. Indeed serum of this sort seems little disposed to spontaneous change; for some which has been two years in my possession still retains its colour; and though it has a putrid smell, it exhibits visible signs of decomposition only by a very slight separation of nearly colourless water, from a coagulum resembling new cheese.

The specific gravity of the serum now under consideration = 1·0187, which nearly agrees with that last examined, though it be somewhat less than that of the first.

One hundred grains of the serum were slowly evaporated by a moderate temperature. The residue, when the watery part was wholly dissipated, = 21·1 grs. A yellowish transparent oil was observed to flow beneath the solid residue, when the glass capsule was heated; but it became solid, and of a greyish-white colour, at the ordinary temperature of my apartment. The oil was taken up, while fluid, by bibulous paper, which had been previously weighed with due attention to ensure uniformity in the state of its hygrometric moisture. A portion of the paper was deeply stained with the oil, and fresh portions were employed until the broken coagulum no longer soiled the paper. The weight gained by the paper = 4·5 grains.

The albumen was soaked for six hours in distilled water, and well washed on a filter. The dried albumen = 15·7 grains. The washings were evaporated, and saline matter, consisting chiefly of muriates and lactates, were obtained, amounting to 0·9 gr. of which 0·7 appeared to be of the former.

From these results, we may state the constituents of this remarkable fluid to be:

Water.....	= 78·9
Albumen.....	= 15·7
Oil.....	= 4·5
Salts.....	= 0·9
	<hr/>
	100·0

The peculiarities of this serum consist in its containing oil, which exists in the form of an *emulsion*, and in this case amounts to 4 per cent.; in its having about double the usual quantity of albumen assigned to the serum of blood by the experiments of Berzelius and others; and in its diminished proportion of saline ingredients. It is worthy of remark, that these peculiarities in the three cases were connected with inflammatory disease; and in the two last were accompanied by decided inflammation of the liver. I have had an opportunity of examining the blood of one of the patients when he was in health, and found nothing peculiar in it. I may also state, that I examined some time ago a serum which resembled, in colour and consistence, thin *water-gruel*; but in this I could detect no oil; and it seemed to owe its peculiarities to an excess of albumen.

I am, respectfully yours,

THOMAS STEWART TRAILL.

## ARTICLE X.

*On the Alloys of Steel.* By J. Stodart, Esq. FRS. and Mr. M. Faraday, Chemical Assistant in the Royal Institution.\*

THE alloys of steel made on a small scale in the laboratory of the Royal Institution proving to be good, and the experiments having excited a very considerable degree of interest both at home and abroad, gave encouragement to attempt the work on a more extended scale, and we have now the pleasure of stating, that alloys similar to those made in the Royal Institution, have been made for the purpose of manufacture; and that they prove to be, in point of excellence, in every respect equal, if not superior, to the smaller productions of the laboratory. Previous, however, to extending the work, the former experiments were carefully repeated, and to the results were added some new combinations, namely, steel with palladium, steel with iridium and osmium, and latterly, steel with chromium. In this last series of experiments, we were particularly fortunate, having, by practice, acquired considerable address in the management of the furnaces, and succeeded in procuring the best fuel for the purpose. Notwithstanding the many advantages met with in the laboratory of the Royal Institution, the experiments were frequently rendered tedious from causes often unexpected, and sometimes difficult to overcome; among these, the failure of crucibles was, perhaps, the most perplexing. We have never

\* From the Philosophical Transactions for 1822. Part II.

yet found a crucible capable of bearing the high degree of temperature required to produce the perfect reduction of titanium; indeed we are rather disposed to question whether this metal has ever been so reduced: our furnaces are equal\* (if any are) to produce this effect, but hitherto we have failed in procuring a crucible.

The metals that form the most valuable alloys with steel are silver, platina, rhodium, iridium, and osmium, and palladium; all of these have now been made in the large way, except indeed the last named. Palladium has, for very obvious reasons, been used but sparingly; four pounds of steel with 1-100th part of palladium, has, however, been fused at once, and the compound is truly valuable, more especially for making instruments that require perfect smoothness of edge.

We are happy to acknowledge the obligations due from us to Dr. Wollaston, whose assistance we experienced in every stage of our progress, and by whom we were furnished with all the scarce and valuable metals; and that with a liberality which enabled us to transfer our operations from the laboratory of the chemist, to the furnace of the maker of cast steel.

In making the alloys on a large scale, we were under the necessity of removing our operations from London to a steel furnace at Sheffield; and being prevented by other avocations from giving personal attendance, the superintendence of the work was consequently intrusted to an intelligent and confidential agent. To him the steel, together with the alloying metals in the exact proportion, and in the most favourable state for the purpose, was forwarded, with instructions to see the whole of the metals, and nothing else, packed into the crucible, and placed in the furnace, to attend to it while there, and to suffer it to remain for some considerable time in a state of thin fusion, previous to its being poured out into the mould. The cast ingot was next, under the same superintendence, taken to the tilting mill, where it was forged into bars of a convenient size, at a temperature not higher than just to render the metal sufficiently malleable under the tilt hammer. When returned to us, it was subjected to examination both mechanical and chemical, as well as compared with the similar products of the laboratory. From the external appearance, as well as from the texture of the part when broken by the blow of the hammer, we were able to form a tolerably correct judgment as to its general merits; the hardness, toughness, and other properties, were further proved by severe trials, after being fashioned into some instrument, or tool, and properly hardened and tempered.

It would prove tedious to enter into a detail of experiments made in the Royal Institution; a brief notice of them will at present be sufficient. After making imitations of various speci-

\* We have succeeded in fusing in these furnaces rhodium, and also, though imperfectly, platina in crucibles.

mens of meteoric iron by fusing together pure iron and nickel, in proportions of 3 to 10 per cent. we attempted making an alloy of steel with silver, but failed, owing to a superabundance of the latter metal; it was found, after very many trials, that only the 1-500th part of silver would combine with steel, and when more was used, a part of the silver was found in the form of metallic dew lining the top and sides of the crucible; the fused button itself was a mere mechanical mixture of the two metals, globules of silver being pressed out of the mass by contraction in cooling, and more of these globules being forced out by the hammer in forging; and further, when the forged piece was examined, by dissecting it with diluted sulphuric acid, threads or fibres of silver were seen mixed with the steel, having something of the appearance of steel and platina when united by welding; but when the proportion of silver was only 1-500th part, neither dew, globules, nor fibres, appeared, the metals being in a state of perfect chemical combination, and the silver could only be detected by a delicate chemical test.

With platina and rhodium, steel combines in every proportion; and this appears also to be the case with iridium and osmium: from 1 to 80 per cent. of platina was perfectly combined with steel, in buttons of from 500 to 2000 grains. With rhodium, from 1 to 50 per cent. was successfully used. Equal parts by weight of steel and rhodium gave a button, which, when polished, exhibited a surface of the most exquisite beauty: the colour of this specimen is the finest imaginable for a metallic mirror, nor does it tarnish by long exposure to the atmosphere: the specific gravity of this beautiful compound is 9.176. The same proportion of steel and platina gave a good button, but a surface highly crystalline renders it altogether unfit for a mirror. In the laboratory, we ascertained that, with the exception of silver, the best proportion of the alloying metal, when the object in view was the improvement of edge tools, was about 1-100th part, and in this proportion they have been used in the large way. It may be right to notice, that in fusing the metals in the laboratory no flux whatever was used, nor did the use of any ever appear to be required.

Silver being comparatively of little value with some of the alloying metals, we were disposed to make trial with it as the first experiment in the large way: 8 lbs. of very good Indian steel was sent to our agent, and with it 1-500th part of pure silver: a part of this was lost owing to a defect in the mould; a sufficient quantity was, however, saved, to satisfy us as to the success of the experiment. This, when returned, had the most favourable appearance both as to surface and fracture; it was harder than the best cast steel, or even than the Indian wootz, with no disposition whatever to crack, either under the hammer, or in hardening. Some articles for various uses have been made from this alloy; they prove to be of a very superior quality; its application will probably be

extended not only to the manufacture of cutlery, but also to various descriptions of tools; the trifling addition of price cannot operate against its very general introduction. The silver alloy may be advantageously used for almost every purpose for which good steel is required.

Our next experiment made in the large way was with steel and platina: 10 lbs. of the same steel, with 1-100th part of platina, the latter in the state produced by heating the ammonia muriate in a crucible to redness, was forwarded to our agent, with instructions to treat this in the same way as the last named metals. The whole of this was returned in bars remarkable for smoothness of surface and beauty of fracture. Our own observation, as well as that of the workmen employed to make from it various articles of cutlery, was, that this alloy, though not so hard as the former, had considerably more toughness: this property will render it valuable for every purpose where tenacity, as well as hardness, is required; neither will the expense of platina exclude it from a pretty general application in the arts; its excellence will much more than repay the extra cost.

The alloys of steel with rhodium have also been made in the large way, and are, perhaps, the most valuable of all; but these, however desirable, can never, owing to the scarcity of the metal, be brought into very general use. The compound of steel, iridium, and osmium, made in the large way, is also of great value; but the same cause, namely, the scarcity and difficulty of procuring the metals, will operate against its very general introduction. A sufficient quantity of these metals may, perhaps, be obtained to combine with steel for the purpose of making some delicate instruments, and also as an article of luxury, when manufactured into razors. In the mean time, we have been enabled, repeatedly, to make all these alloys (that with palladium excepted), in masses of from 8 to 20 lbs. each; with such liberality were we furnished with the metals from the source already named.

A point of great importance in experiments of this kind was to ascertain whether the products obtained were exactly such as we wished to produce. For this purpose, a part of each product was analysed, and in some cases the quantity ascertained; but it was not considered necessary in every case to verify the quantity by analysis, because, in all the experiments made in the laboratory, the button produced after fusion was weighed, and if it fell short of the weight of both metals put into the crucible, it was rejected as imperfect, and put aside. When the button gave the weight, and on analysis gave proofs of containing the metal put in to form the alloy, and also on being forged into a bar and acted on by acids, presented an uniform surface, we considered the evidence of its composition as sufficiently satisfactory. The processes of analysis, though simple, we shall briefly state; the information may be desirable to others who

may be engaged on similar experiments; and further, may enable every one to detect any attempt at imposition. It would be very desirable at present to possess a test as simple, by which we could distinguish the wootz, or steel of India, from that of Europe; but this, unfortunately, requires a much more difficult process of analysis.

To ascertain if platina is in combination with steel, a small portion of the metal, or some filings taken from the bar, is to be put into dilute sulphuric acid; there will be rapid action; the iron will be dissolved, and a black sediment left, which will contain carbon, hydrogen, iron, and platina; the carbon and hydrogen are to be burnt off, the small portion of iron separated by muriatic acid, and the residuum dissolved in a drop or two of nitro-muriatic acid. If a piece of glass be moistened with this solution, and then heated by a spirit-lamp and the blowpipe, the platina is reduced, and forms a metallic coating on the glass.

In analysing the alloy of steel and silver, it is to be acted on by dilute sulphuric acid, and the powder boiled in the acid; the silver will remain in such a minute state of division, that it will require some time to deposit. The powder is then to be boiled in a small portion of strong muriatic acid;\* this will dissolve the iron and silver, and the latter will fall down as a chloride of silver on dilution with water; or the powder may be dissolved in pure nitric acid, and tested by muriatic acid and ammonia.

The alloy of steel and palladium, acted on by dilute sulphuric acid, and boiled in that acid, left a powder which, when the charcoal was burnt from it, and the iron partly separated by cold muriatic acid, gave on solution in hot muriatic acid, or in nitro-muriatic acid, a muriate of palladium; the solution, when precipitated by prussiate of mercury, gave prussiate of palladium; and a glass plate moistened with it and heated to redness, became coated with metallic palladium.

The residuum of the rhodium alloy obtained by boiling in diluted sulphuric acid, had the combustible matter burnt off, and the powder digested in hot muriatic acid: this removed the iron; and by long digestion in nitro-muriatic acid, a muriate of rhodium was formed, distinguishable by its colour, and by the triple salt it formed with muriate of soda.

To analyze the compound of steel with iridium and osmium, the alloy should be acted on by dilute sulphuric acid, and the residuum boiled in the acid; the powder left is to be collected and heated with caustic soda in a silver crucible to dull redness for a quarter of an hour, the whole to be mixed with water, and having had excess of sulphuric acid added, it is to be distilled,

\* Although it is a generally received opinion, that muriatic acid does not act on silver, yet that is not the case; pure muriatic acid dissolves a small portion of silver very readily.

and that which passes over condensed in a flask: it will be a solution of oxide of osmium, will have the peculiar smell belonging to that substance, and will give a blue precipitate with tincture of galls. The portion in the retort being then poured out, the insoluble part is to be washed in repeated portions of water, and then being first slightly acted on by muriatic acid to remove the iron, is to be treated with nitro-muriatic acid, which will give a muriate of iridium.

In these analyses, an experienced eye will frequently perceive, on the first action of the acid, the presence of the alloying metal. When this is platina, gold, or silver, a film of the metal is quickly formed on the surface of the acid.

Of alloys of platina, palladium, rhodium, and iridium and osmium, a ready test is offered when the point is not to ascertain what the metal is, but merely whether it be present or not. For this purpose we have only to compare the action of the same acid on the alloy and on a piece of steel; the increased action on the alloy immediately indicates the presence of the metal; and by the difference of action, which on experience is found to be produced with the different metals, a judgment may be formed even of the particular one present.

The order in which the different alloys stand with regard to this action is as follows: steel, chromium alloy, silver alloy, gold alloy, nickel alloy, rhodium alloy, iridium and osmium alloy, palladium alloy, platina alloy. With similar acid, the action on the pure steel was scarcely perceptible: the silver alloy gave very little gas, nor was the gold much acted on. All the others gave gas copiously, but the platina alloy in most abundance.

In connection with the analysis of these alloys, there are some very interesting facts to be observed during the action of acids on them, and, perhaps, none of these are more striking than those last referred to. When the alloys are immersed in diluted acid, the peculiar properties which some of them exhibit, not only mark and distinguish them from common steel, and from each other, but also give rise to some considerations on the state of particles of matter of different kinds when in intimate mixture or in combination, which may lead to clearer and more perfect ideas on this subject.

If two pieces, one of steel, and one steel alloyed with platina, be immersed in weak sulphuric acid, the alloy will be immediately acted on with great rapidity and the evolution of much gas, and will shortly be dissolved, while the steel will be scarcely at all affected. In this case, it is hardly possible to compare the strength of the two actions. If the gas be collected from the alloy and from the steel for equal intervals of time, the first portions will surpass the second some hundreds of times.

A very small quantity of platina alloyed with steel confers this property on it:  $\frac{1}{100}$  increased the action considerably; with  $\frac{1}{200}$  and  $\frac{1}{100}$  it was powerful; with 10 per cent of platina it

acted, but not with much power; with 50 per cent. the action was not more than with steel alone; and an alloy of 90 platina with 20 steel was not affected by the acid.

The action of other acids on these alloys is similar to that of sulphuric acid, and is such as would be anticipated: dilute muriatic acid, phosphoric acid, and even oxalic acid, acted on the platina alloy with the liberation of more gas than from zinc; and tartaric acid and acetic acid rapidly dissolved it. In this way chalybeate solutions, containing small portions of protoxide of iron, may be readily obtained.

The cause of the increased action of acids on this and similar alloys, is, as the President of this Society suggested to us, probably electrical. It may be considered as occasioned by the alloying metal existing in such a state in the mass, that its particles form voltaic combinations with the particles of steel, either directly, or by producing a definite alloy, which is diffused through the rest of the steel; in which case the whole mass would be a series of such voltaic combinations; or it may be occasioned by the liberation, on the first action of the acid, of particles which, if not pure platina, contain, as has been shown, a very large proportion of that metal, and which, being in close contact with the rest of the mass, form voltaic combinations with it in a very active state; or, in the third place, it may result from the iron being mechanically divided by the platina, so that its particles are more readily attacked by the acid, analogous to the case of protosulphuret of iron.

Although we have not been able to prove by such experiments, as may be considered strictly decisive, to which of these causes the action is owing, or how much is due to any of them, yet we do not hesitate to consider the second as almost entirely, if not quite, the one that is active. The reasons which induce us to suppose this to be the true cause of the action, rather than any peculiar and previous arrangement of the particles of steel and platina, or than the state of division of the steel, are, that the two metals combine in every proportion we have tried, and do not, in any case, exhibit evidences of a separation between them, like those, for instance, which steel and silver exhibit; that when, instead of an acid, weaker agents are used, the alloy does not seem to act with them as if it was a series of infinitely minute voltaic combinations of steel and platina, but exactly as steel alone would do; that the mass does not render platina wire more negative than steel, as it probably in the third case would do; that it does not rust more rapidly in a damp atmosphere; and that when placed in saline solutions, as muriate of soda, &c. there is no action takes place between them. In such cases it acts just like steel; and no agent that we have as yet tried, has produced voltaic action that was not first able to set a portion of the platina free by dissolving out the iron.

Other interesting phenomena exhibited by the action of acid

on these steels, are the differences produced when they are hard and when soft. Mr. Daniel, in his interesting paper on the mechanical structure of iron, published in the *Journal of Science*, has remarked, that pieces of hard and soft steel being placed in muriatic acid, the first required five fold the time of the latter to saturate the acid; and that when its surface was examined, it was covered with small cavities like worm-eaten wood, and was compact and not at all striated, and that the latter presented a fibrous and wavy texture.

The properties of the platina alloy have enabled us to observe other differences between hard and soft steel equally striking. When two portions of the platina alloy, one hard and one soft, are put into the same diluted sulphuric acid, and suffered to remain for a few hours, then taken out and examined, the hard piece presents a covering of a metallic black carbonaceous powder, and the surface is generally slightly fibrous, but the soft piece, on examination, is found to be covered with a thick coat of grey metallic plumbaginous matter, soft to the touch, and which may be cut with a knife, and its quantity seven or eight times that of the powder on the hard piece: it does not appear as if it contained any free charcoal, but considerably resembles the plumbaginous powder Mr. Daniel describes as obtained by the action of acid on cast iron.

The same difference is observed if pure steel be used, but it is not so striking; because, being much less rapidly attacked by the acid, it has to remain longer in it, and the powder produced is still further acted on.

The powder procured from the soft steel or alloy in these experiments, when it has not remained long in the acid, exactly resembles finely divided plumbago, and appears to be a carburet of iron, and probably of the alloying metal also. It is not acted on by water, but in the air the iron oxidates and discolours the substance. When it remains long in the acid, or is boiled in it, it is reduced to the same state as the powder from the hard steel or alloy.

When any of these residua are boiled in diluted sulphuric or muriatic acid, protoxide of iron is dissolved, and a black powder remains unalterable by the further action of the acid; it is apparently in greater quantity from the alloys than from pure steel, and when washed, dried, and heated to  $300^{\circ}$  or  $400^{\circ}$  in the air, burns like pyrophorus, with much fume; or if lighted, burns like bitumen, and with a bright flame; the residuum is protoxide of iron, and the alloying metal. Hence, during the action of the acid on the steel, a portion of hydrogen enters into combination with part of the metal and the charcoal, and forms an inflammable compound not acted upon by the acid.

Some striking effects are produced by the action of nitric acid on these powders. If that from pure steel be taken, it is entirely dissolved; and such is also the case if the powder be

taken from an alloy, the metal of which is soluble in nitric acid; but if the powder is from an alloy, the metal of which is not soluble in nitric acid, then a black residuum is left not touched by the acid; and which, when washed and carefully dried, is found, when heated, to be deflagrating; and with some of the metals, when carefully prepared, strongly explosive.

The fulminating preparation obtained from the platina alloy, when dissolved in nitromuriatic acid, gave a solution containing much platina, and very little iron. When a little of it was wrapped in foil and heated, it exploded with much force, tearing open the foil, and evolving a faint light. When dropped on the surface of heated mercury, it exploded readily at 400° of Fahrenheit, but with difficulty at 370°. When its temperature was raised slowly, it did not explode, but was decomposed quietly. When detonated in the bottom of a hot glass tube, much water and fume were given off, and the residuum collected was metallic platina with a very little iron and charcoal. We are uncertain how far this preparation resembles the fulminating platina of Mr. Edmund Davy.

In these alloys of steel the differences of specific gravity are not great, and may probably be in part referred to the denser state of the metals from more or less hammering; at the same time it may be observed, that they are nearly in the order of the specific gravities of the respective alloying metals.

The alloys of steel with gold, tin, copper, and chromium, we have not attempted in the large way. In the laboratory, steel and gold were combined in various proportions; none of the results were so promising as the alloys already named, nor did either tin or copper, as far as we could judge, at all improve steel. With titanium we failed, owing to the imperfection of crucibles. In one instance, in which the fused button gave a fine damask surface, we were disposed to attribute the appearance to the presence of titanium; but in this we were mistaken; the fact was, we had unintentionally made wootz. The button, by analysis, gave a little silice and alumine, but not an atom of titanium; menachanite, in a particular state of preparation, was used: this might possibly contain the earths or their basis, or they may have formed a part of the crucible.

M. Berthier, who first made the alloy of steel and chromium,\* speaks very favourably of it. We have made only two experiments: 1600 grains of steel, with 16 of pure chrome, were packed into one of the best crucibles, and placed in an excellent blast furnace: the metals were fused, and kept in that state for some time. The fused button proved good and forged well: although hard, it showed no disposition to crack. The surface being brightened, and slightly acted on by dilute sulphuric acid,

\* Annales de Chimie, xvii. 65.

exhibited a crystalline appearance; the crystals, being elongated by forging, and the surface again polished, gave, by dilute acid, a very beautiful damask. Again, 1600 grains of steel with 48 of pure chrome were fused: this gave a button considerably harder than the former. This too was as malleable as pure iron, and also gave a very fine damask. Here a phenomenon rather curious was observed: the damask was removed by polishing, and restored by heat without the use of any acid. The damasked surface, now coloured by oxidation, had a very novel appearance: the beauty was heightened by heating the metal in a way to exhibit all the colours caused by oxidation, from pale-straw to blue, or from about  $430^{\circ}$  to  $600^{\circ}$  of Fahr. The blade of a sabre, or some such instrument, made from this alloy, and treated in this way, would assuredly be beautiful, whatever its other properties might be; for of the value of the chrome alloy for edge tools we are not prepared to speak, not having made trial of its cutting powers. The sabre blade, thus coloured, would amount to a proof of its being well tempered; the blue back would indicate the temper of a watch spring; while the straw colour towards the edge would announce the requisite degree of hardness. It is confessed, that the operation of tempering any blade of considerable length in this way, would be attended with some difficulty.

In the account now given of the different alloys, only one triple compound is noticed; namely, steel, iridium, and osmium; but this part of the subject certainly merits further investigation, offering a wide and interesting field of research. Some attempts to form other combinations of this description proved encouraging, but we were prevented, at the time, by various other avocations, from bestowing on them that attention and labour they seemed so well to deserve.\*

It is a curious fact, that when pure iron is substituted for steel, the alloys so formed are much less subject to oxidation. Three-per cent. of iridium and osmium fused with some pure iron, gave a button, which, when forged and polished, was exposed, with many other pieces of iron, steel, and alloys, to a moist atmosphere: it was the last of all showing any rust. The colour of this compound was distinctly blue; it had the property of becoming harder when heated to redness and quenched in a cold fluid. On observing this steel-like character, we suspected the presence of carbon; none, however, was found, although carefully looked for. It is not improbable that there may be other bodies, besides charcoal, capable of giving to iron the properties of steel; and though we cannot agree with M. Bous-singault,† when he would replace carbon in steel by silica or its

\* It is our intention to continue these experiments at every opportunity, but they are laborious, and require much time and patience.

† *Annales de Chimie*, xvi. 1.

base, we think his experiments very interesting on this point, which is worthy further examination.

We are not informed as to what extent these alloys, or any of them, have been made at home, or to what uses they have been applied; their more general introduction in the manufacture of cutlery would assuredly add to the value, and consequently to the extension of that branch of trade. There are various other important uses to which the alloys of steel may advantageously be applied. If our information be correct, the alloy of silver, as well as that of platina, has been, to some considerable extent, in use at His Majesty's Mint. We do know, that several of the alloys have been diligently and successfully made on the Continent; very good specimens of some of them having been handed to us; and we are proud of these testimonies of the utility of our endeavours.

To succeed in making and extending the application of these new compounds, a considerable degree of faithful and diligent attention will be required on the part of the operators. The purity of the metals intended to form the compound is essential; the perfect and complete fusion of both, must in every case, be ascertained: it is further requisite, that the metals be kept for some considerable time in the state of thin fusion; after casting, the forging is with equal care to be attended to; the metal must on no account be overheated; and this is more particularly to be attended to when the alloying metal is fusible at a low temperature, as silver. The same care is to be observed in hardening: the article is to be brought to a cherry-red colour, and then instantly quenched in the cold fluid.

In tempering, which is best performed in a metallic bath properly constructed, the bath will require to be heated for the respective alloys, from about 70° to 100° of Fahrenheit above the point of temperature required for the best cast steel. We would further recommend, that this act of tempering be performed twice; that is, at the usual time before grinding, and again just before the last polish is given to the blade. This second tempering may, perhaps, appear superfluous; but upon trial its utility will be readily admitted. We were led to adopt the practice by analogy, when considering the process of making and tempering watch springs.

## ARTICLE XI.

*Memoir illustrative of a general Geological Map of the principal Mountain Chains of Europe.* By the Rev. W. D. Conybeare, FRS. &c.

(Continued from p. 149.)

## GREAT SALIFEROUS SERIES,

Including, *a.* The rothetodteliegende.

*b.* The alpine or magnesian limestone.

*c.* The variegated or new red sandstone.

In comparing the series of deposits which immediately succeed the great carboniferous order as presented in England, and in some parts of the Continent, we shall, perhaps, perceive rather a parallelism than identity of formations in the representatives of rothetodteliegende and the alpine limestone; but in the variegated sandstone an unequivocal identity is manifested.

*a. Rothetodteliegende.*—Conglomerates, including rolled fragments of the neighbouring rocks, characterise this formation: the cement is sometimes argillaceous, sometimes quartzose; by the diminution in size of the fragments, the rock often passes into a sandstone more or less granular. Beds of red micaceous shale alternate in the formation; it contains subordinately in some places carboniferous beds (distinct, however, according to Freisleben, from those of the true coal formation). Trap rocks (amygdaloid and porphyry) are often associated with these deposits, usually, as it should appear, in their lowest members where they rest on the true coal series. Beds of limestone also occur, though sparingly. The upper members where they support the alpine limestone, pass into a calcareous conglomerate called in the Hartz and neighbouring districts, the Weissliegende. This is by some authors considered as a distinct formation; by others (with whom I am inclined to agree), it is included as the last deposit of the rotheliegende.

Ores of iron, cobalt, and copper occasionally occur.

The petrifications of this rock appear to belong chiefly, if not exclusively, to the vegetable kingdom.

All circumstances indicate a near connection, but not an identity, of formation between the rothetodte and the great carboniferous series.\* It seems rather to hold an intermediate place

\* I have to add to the authorities I have previously adduced in confutation of the arrangement which would identify the rothetodte and the old red sandstone of Britain, that of Dr. Boué, particularly important, because he has minutely examined the old red sandstone of Scotland, which, it has been contended (and principally on the ground of his description), presented the closest analogies to the rothetodte, and is equally acquainted with all the foreign localities of the latter rock, from recent journals. I have now a series of letters of the highest interest from this author to Prof. Daubeny before me, in which he states, that "he agrees with Prof. Buckland in dis-

between this and the following deposits, and to introduce, as it were, the latter.

*b. The Alpine Limestone; Zechstein, or Magnesian Limestone. (Werner's first Floetz Limestone.)*—This formation is very variable in different places, and may be said generally to present in distant districts a parallelism rather than identity of deposits. The prevailing rock in the continental deposits (zechstein) is a grey compact, and generally argillaceous limestone; sometimes slightly granular, and having a small degree of lustre from the facettes of the calcareous crystals imbedded: its colour is grey or bluish, sometimes acquiring a reddish tinge; it contains subordinate beds of magnesian limestone, of ferriferous limestone, of cellular and crystalline limestone (rauchwacke), of fetid limestone, and of bituminous and cupriferous marl slate. Copper, galena, calamine, and mercury, are among the metallic products of this formation; and the association of rock salt and gypsum attest its connexion with the superimposed sandstones which also abound in these minerals.

Organic remains are scarce in this formation, speaking generally, but yet in particular spots and beds occur even abundantly: the following species are enumerated by Scotthheim; but I am not at present able to identify all the names employed with the synonyms in English writers.

Gryphites aculeatus,  
G. gigas,  
G. arcuatus,  
Pecten textorius,  
Pecten salinarius,  
Mytilus rostratus,  
Terebratula alata,  
Terebratula lacunosa,

distinguishing the rothetodite from the old red sandstone." I hope hereafter to communicate from these letters a sketch of the recent discoveries of this active and persevering geologist; and here then for the present, at least, I shall close this controversy. I had originally intended to have given more minute extracts from the original works referred to; but unless the accuracy of the general statements above given shall be questioned, it will, perhaps, be superfluous to do so; if they should be controverted, I am then prepared to support them. En resumé against the proposed identification of the rothetodite and our oldest sandstone. I have the express testimony of Buckland, Humboldt, Boné, Daubuisson, and Boudant, and (as I contend) the implied evidence of every other writer on the subject, excepting, perhaps, of Prof. Jamieson. Whether it be more properly referable to the upper part of the carboniferous, or the lower part of the saliferous series, is a distinct question, on which much division of opinion exists, and which is after all not very material. I have endeavoured to compromise matters by treating it as an intermediate link between them.

I have thus endeavoured to support, as I may, the opinions which to me appear most probable against an adversary from whom, however, I can never dissent without hesitation. Mr. Weaver's memoir on the South-east of Ireland will long remain as one of the most perfect models of geological description, and must excite our regret that we have not been favoured with more original communications from the same pen; while his extracts from the writings of continental geologists, published in the *Annals*, equally attest his power of generalising, and presenting under their most luminous point of view, the observations of others.

*Terebratula trigonella*,  
*Ammonites ammonius*,  
*Ammonites amaltheus*,  
*Ammonites hircinus*,  
*Nautilus coactus*,  
*Orthoceratitæ*,  
 Joints of encrinites and pentacrinites,  
 A species of trilobite.

Bones of monitors and of fish, a separate line belonging to the vegetable kingdom, leaves of dicotyledonous plants (in which respect there is a distinction between these remains and those of the coal fields which are all monocotyledonous), and fragments related to the lycopodium and bamboo, but no true ferns.

*c. The Variegated or new Red Sandstone* exhibits a series of friable sandstones and argillaceous marls, in which a red colour varied by streaks and spots of a greenish-yellow, &c. prevails. The sandstone predominates in the lower members, and is associated with conglomerate beds. Gypsum and rock salt occur in this deposit as in that of the zechstein. Iron is the only metal, I believe, which it has yet yielded.

*Distribution.*—It is easy to trace generally the distribution of the whole series, but much obscurity still hangs over the exact demarcation of its individual members. This it may now be expected will shortly be dissipated in as far as it arises from our hitherto imperfect knowledge of the structure of the countries in which it occurs, by the publication of the works alluded to in the beginning of these essays; but in part it results from a physical cause which cannot so readily be surmounted; namely, the difficulty, if not impossibility, of ascertaining in localities where the middle series of limestones (*b*) may happen to be wanting; whether the sandstones and conglomerates belong to the upper class (*c*), or the lower (*a*): following the same route which has been traced in the preceding articles, we have first to examine,

#### (A.) *The Coasts of the Baltic.*

The red marl containing gypsum, which occupies a very extensive tract in the centre of Russia, appears to skirt the transition district of Esthonia, near Riga. Its continuation ought to be sought for between the transition and more recent districts in Scania and the Danish isles; but these districts have not yet been described with sufficient precision to enable us to speak of them with certainty.

#### (B.) *The British Isles.*

1. It is possible that the sandstones of the Orkneys may either wholly, or in part, belong to this series.
2. The same observation will apply to those of the Sutherland coast on the north-east of Scotland.

3. In the Hebrides, that portion of the sandstone which is closely associated with the gryphite limestone is probably referable to this series.

4. It is generally believed that the sandstones of the lowlands are partly to be ascribed to the old red sandstone; viz. where they skirt the Grampian chain, and partly to the coal sandstones, the present series not occurring in that tract.

5. On the south of the southern or transition chain of Scotland, however, the sandstones of the present series certainly occur in the valley of the Tweed, and in the shores of the Firth of Solway, in Dumfriesshire, being in the latter district connected with the mass of the same formations stretching into the north of Cumberland.

6. In Ireland, the saliferous sandstones underlie the basalt of the Ulster district, but are confined to a narrow zone encompassing that area.

7. In England, the saliferous sandstone mantling round the south and south-west of the penine chain (see the account of the coal districts) occupies the central counties, sending a branch north-west to the point where the cumbrian chains inosculate with the former, and in the opposite direction, forming a band between the lias and the coal and transition series, through Warwickshire, Worcestershire, and Gloucestershire. In South Gloucester, Somerset, and the south of Monmouth and Glamorgan, these formations have a very irregular outline, since they form upfillings through which the elder rocks of the coal series protrude in all the loftier ranges; in Devonshire they are similarly disposed among the transition chains.

In all these places the lowest members appear to abound in conglomerates; those of Devonshire, which are associated with amygdaloid, have every feature of the German rothetodte, but the magnesian limestone is here wanting, and the variegated sandstone lies immediately on those conglomerates.

I have, in the preceding number, suggested the inquiry, whether the rock distinguished in Smith's Yorkshire as the Pontefract rock may not possess similar analogies.

The magnesian limestone forms a continuous band from the south of Durham through Yorkshire and Nottinghamshire.

Mr. Smith, in his geological map of Yorkshire, subdivides the magnesian limestone, or, as he calls it, red-land limestone, thus:

1. A hard bluish-white thin bedded stone which at Kinnerley, Knottingly, and Brotherton, makes the lime celebrated for agricultural purposes.

2. Red and blue clay and gypsum.

3. A soft yellowish calcareous freestone or magnesian limestone.

These beds are separated from the superior red marl by a thick conglomerate.

Mr. Buckland has observed in Yorkshire, beds closely resem-

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bling the rauchwacke or cellular limestone of the Continent,  
associated with magnesian limestone.

In the southern counties, these formations are represented by a calcareo-magnesian conglomerate, occasionally, but very rarely, passing into limestone beds of uniform texture. These conglomerates bear a great analogy, as Mr. Weaver has well remarked to the weissliegende of Germany.

These formations must be considered as parallel to, rather than identical with, the alpine limestones of the Continent.

The above deposits are comparatively very limited in England, and nine-tenths of the tract occupied by the great series we are now describing, is exclusively possessed by the superincumbent variegated sandstone and marl.

Salt or salt springs occur in Cheshire, Staffordshire, and Worcestershire; gypsum, passim, and sulphate of strontian, in Gloucester and Somersetshire.

#### (C.) *Western France.*

The new red sandstone which crosses the channel from Devonshire, is seen, though not extensively, skirting the transition rocks of Brittany; but the lias and oolite advance so near to them, as almost to overlie and conceal it; as also seems to be the case in the centre of that country, against the northern edge of that great group of primitive ridges branching off from the Cevennes; but we want information on this district.

According to the interesting communications of Dr. Boué, the variegated sandstone exists in patches (*par lambeaux*) in the south-west of France. It is there represented by marls with compact and fibrous gypsum (Cognar St. Froult, near Rochefort), and sometimes immediately covered by Jura limestone, &c. At the foot of the Pyrenees between St. Giron and Rimont, it is more extensively developed.

#### (D.) *Spain.*

Humboldt is of opinion that the limestone of Montperdur is that belonging to the present series. The specimens I have seen, and the fossils described as occurring in it, would certainly induce me to assign it to a later era (namely, that corresponding to our green sand), since it much resembles those rocks in the exterior chains of the Alps which are, by Messrs. Buckland and Brogniart, referred to that class; but I can hardly bring myself on such slight data even to suggest an inquiry militating against so high an authority. Be this as it may, after crossing the Pyrenees, the rock salt of this formation occurs associated as usual with gypsum at Cardona (a description of which will be found in the fourth volume of the Geological Transactions). The celebrated conglomerate mountain of Montserrat in the same quarter, is, perhaps, referable to the same era; and we find gypsum and rock salt abundantly distributed along the course of

the Ebro from below Saragossa to above Tudela. It is difficult to speak of a country whose geology has yet never received a strictly scientific examination; but enough is known to teach us that the central and western districts are principally occupied by primitive chains, while the east and south-east (with the exception of the transition chain of the Sierra Nevada) exhibit little but calcareous mountains, among which gypsum is plentifully interspersed. As we shall hereafter see that the limestone formation, answering in age to our magnesian limestone, swells into great importance on the Continent, and constitutes large mountain zones encircling the Alps, &c. which are in like manner characterised by the intermixture of gypsum, it is no improbable conjecture that these deposits belong to the same period.

#### (E.) *The Alps.*

We find these formations forming a zone on either side of the Alps; on the north, interposed between the older rocks and great Nagelfluë of Switzerland, which was once itself considered as belonging to them, but has been proved by subsequent researches to be of much more recent date, and contemporaneous with the sandstones of the basin of Paris. The red sandstone is here intimately associated with alpine limestone, which corresponds with the calcareous formations already described as coeval with our magnesian limestone; and gypsum and salt may be found interspersed through the whole series. A similar character applies to the zone on the south side of the Alps; here the red sandstone may be seen to the greatest advantage in the valley of the Adige, ten miles north of Trent, and in the valley of Avisio, which descends from the Val di Tassa into that of the Adige. In the same neighbourhood a porphyry occurs associated with these formations on the south of the Alps only.

The reader is referred for further particulars to the excellent memoir of Prof. Buckland, *Annals of Philosophy*, June, 1821.

It is probably the limestone of this formation belonging to the southern alpine zone, which extends into Carinthia, Istria, Dalmatia, &c.; the limestone of the Apennines, and much of that in Greece, may also, perhaps, be referred to the same era.

It is not to be understood that all the limestone chains bordering the Alps belong to the present series. Parts on the exterior are undoubtedly referable to the oolitic series; and others, as it should appear, parallel to the limestones associated with our green sand. The great disturbances which have affected these colossal chains, and the almost inaccessible nature of much of the ground, must long leave considerable obscurity on the exact demarcation of their constituent formations.

#### (F.) *Districts North of the Jura and on the Banks of the Rhine.*

The strata of the Jura chain cropping out to the north exhibit

beneath the lias along their northern and north-western escarpment the saliferous sandstone (Lons le saulnier, &c.).

Dr. Boué has traced the variegated sandstone, alpine limestone, and rotheliegende, skirting the Vosges on the left bank of the Rhine, where the continuation of this tract approaches the slate mountains of the Ardennes. It has been described by Omalius d'Halloy, who classes this series agreeably to its disposition in England as the oldest of the horizontal rocks, contrasting it with the inclined position of the coal and older formations.

Keferstein states, that the primitive range of the Schwartzwald and Odenwald on the right bank of the Rhine is succeeded on the east, first by rothetodte, then by alpine limestone, and thirdly by variegated sandstone; and according to his representation, the Wurtzburg calcareous platform is a portion of this band of alpine limestone. Boué and Humboldt, however, I believe, consider the same tract as muschel kalkstein. Having hastily traversed it myself, I felt also inclined to embrace the latter opinion.

Between this limestone and the cavernous (Jura) limestone of Bamberg, a tract of conglomerate and sandstone intervenes. Keferstein refers this to the variegated sandstone forming the third zone, consistently with his general ideas of the structure of this district; but if the Wurtzburg tract be muschel kalk, this sandstone district is, perhaps, equivalent with the sand of our inferior oolite. I felt much embarrassed by this district when I traversed it, and have not yet been able to procure information which fully satisfies me.\*

After passing the platform of Jura limestone between Bamberg and Bayreuth, however, the red marl of the variegated sandstone is unequivocally displayed resting against the Bohemian chains, the Fichtelgebirge, and the Thuringerwald.

#### (G.) *North of Germany.*

In the point to which we have now arrived, we may observe skirting the Thuringerwald all the formations noticed at the head of this article, viz. the red marl and gypsum; the calcareous beds associated with the cupriferous marl slate, and at the bottom the rothetodte: a shell limestone answering to our lias rests on these beds, and separates this from a similar district encircling the detached ancient group of the Hartz mountains: here, and in the continuation of this district towards Halle, the rothetodte is to be observed in many places in contact with the coal formation, and always above it. Rock salt is found in numerous points in this quarter, along the line between Osnaburg and

\* I have, therefore, followed the authority of Keferstein in colouring my map, preferring to copy the errors, if errors they be, of the best published document, rather than substitute an original representation from very imperfect observations, which would, therefore, have been quite as likely to prove erroneous.

Magdeburg throughout the south of Hanover. (See the works of Freisleben.)

The zone of these rocks surrounding the Thuringerwald continues to skirt the prolongation of the same great mountain band through Saxony, where it assumes the name of the *erzegebirge*, through Silesia, where it changes its appellation for that of the *riesengebirge*. It occurs on both sides this chain, extending on the south into the great basin of Bohemia, and covering the coal formation of that country and the adjoining parts of Silesia. This district has been fully described by Von Raumer,\* and in part also by Von Buch in his account of Glatz.

#### (H.) *Hungary.*

These formations appear to skirt in like manner both sides of the Carpathian chain, which is still only a continuation of this same great primitive band traversing central Europe. The most extensive salt mines which have ever been worked are to be found in the northern sandstone zone at Wielictzka† on the south of Cracow, and salt is also worked along the inner zone in several valleys descending to the west from the chain where it trends round Transylvania. The primitive ridge of the Carpathians, turning eastwards on the south of the Danube near its mouth; assumes the name of Mount Balkan, and proceeds to the coast of the Euxine, which cuts it off; but the transition rocks on the south of the peninsula of the Crimea, appear to form a portion of its northern exterior chain, and the Caucasus to form its prolongation; both these ranges are skirted by conglomerates, probably of this formation.

#### (I.) *Russia.*

The sand of this formation, containing gypsum, appears to be very abundant in the north and east of European Russia. Mr. Strangways has recently laid much important information on the mineralogical relations of this vast empire before the Geological Society, in which all the particulars hitherto collected are given. It will here suffice to observe, that if a line be drawn from Riga north of Moscow to the banks of the river Oural, this formation will be found plentifully distributed on the north and east of it, especially along the Volga and its branches on the north-east of Moscow: it appears indeed to extend to, and

\* Mr. Fichtel says, that on the north this zone extends from Wielictzka into Moldavia, in which interval he enumerates 58 places where salt is worked or salt springs found, and on the south from Eperics, 400 or 500 miles eastwards through Transylvania, affording 159 localities of salt.

† M. Beudant fancies the salt of the Wielictzka mines to be derived by infiltration from a superior sandstone which he assigns to the tertiary era, because it contains lignites. This hypothesis is highly improbable; lignites are by no means confined to the tertiary deposits. Mr. Buckland, who has visited the spot, felt convinced that the salt mines were in genuine red marl, and, I believe, observed, in the same vicinity, green sand overlying that formation. This is probably the lignite sandstone of M. Beudant.

invest the Oural mountains with, the intermixture of a cupriferous sand, probably allied to the cupriferous beds associated in this formation in Germany and the Tyrol, &c.

On the south of the Oural chain, it appears to stretch to the Caspian, and to spread very extensively in the adjoining regions of Asia.

(To be continued.)

## ARTICLE XII.

### *Observations on the Advantages of Oil Gas Establishments.*

By M. Ricardo, Esq.

(To the Editor of the *Annals of Philosophy*.)

DEAR SIR,

Brighton, Feb. 18, 1823.

It is now nearly two years since I first addressed you on the subject of oil gas, when you did me the favour to insert in the *Annals of Philosophy* a paper "On the Comparative Advantages of Oil and Coal Gas." These observations were founded upon a few experiments I was enabled myself to try on the small scale, the reports of those who possessed an oil gas apparatus, and upon what I conceived would be the results obtained at an oil gas establishment on an extensive plan. Since that period I have had ample opportunities afforded me of making a trial upon a larger scale at the Whitechapel-road Gas Works, situated in Oldford; and the result has been a full and satisfactory confirmation of what I had before advanced. The formation of a Company for carrying this work into effect was in contemplation at the time I first wrote. A capital was raised, an Act of Parliament was obtained, and to Messrs. Taylor and Martineau devolved the task of erecting the works, which were executed in a manner highly creditable to these gentlemen. About two or three weeks before Christmas, 1821, the public were supplied with gas, only five months having been occupied in executing the work and laying the mains. The first annual meeting took place on Thursday, the 6th of February last, when a most satisfactory report was presented by the Committee, and a dividend of two and a half per cent. declared on the capital advanced; not a dividend made for the occasion, but one arising from a clear profit over and above the expenditure, of which every proprietor had an opportunity of satisfying himself by a reference to the accounts which were laid before the meeting. This certainly may be considered as a strong proof in favour of oil gas, when it is known that the mains of this establishment run through, a district in which there is as little demand for

light as any in the vicinity of London. This Company originated with some few gentlemen in the neighbourhood who were anxious to have the road lighted with it. Many who subscribed did it without any view to profit, wishing only for the advantages of the light. Messrs. Taylor and Martineau held out no very flattering prospects, although they contemplated that it might not be unattended with profit; and such too was the opinion which I had formed when I assisted in promoting it.

Of the 10,000*l.* the capital subscribed for, only 8000*l.* have been advanced. Two sets of retorts, with the necessary apparatus, a gasometer containing 8000 cubic feet, and capacious reservoirs for oil, have been constructed; seven miles of main have been laid in various directions, the total cost having been about 7,300*l.*\* There are at present only 100 customers supplied from this establishment, but it is difficult to say, how many lights are in use, as the gas is supplied by meters, and paid for according to the quantity consumed; so that any person may have as many burners as he pleases. The average number in constant use may be reckoned at about 500 or 600; besides which there are about 180 public lights supplied at a price which yields no profit. One man as gas-maker, and a boy to assist him, are the only labourers required, and without any addition, these might supply double the number of lights. In this case, the current expences of labour, wear and tear, rent, and incidentals, would continue the same, the only additional charge being the cost of oil and coals. In my former statement, when I calculated the expences at 5*s.* 6*d.* per 1000 feet as a fixed sum, I was incorrect. This sum must be constantly diminishing with the increased demand for gas. On the present scale at this gas work, the expences may be estimated at 6*s.* per 1000 feet; if the consumption was doubled, it would be only 3*s.* per 1000 feet; and if the apparatus supplied to the full extent of what our capital would allow us, it would be considerably less; for with the addition of another gasometer and one labourer, during the winter months, I have no doubt from the observations I have made, that between 3000 or 4000 burners might be kept supplied. Two pairs of retorts working 8 or 10 hours a day, are found sufficient to provide all the gas for the present winter consumption, amounting to about 6000 feet per night. If the whole six pairs were at work, and kept employed night and day as they are in coal gas works, more than six times that quantity might be produced. If the Company had erected their own buildings, and furnished themselves with another gasometer, their capital subscribed would have been sufficient to have covered all these

\* The sum expended, as shown by the accounts, is about 7,700*l.* but of this nearly 400*l.* are for gas meters, and 760*l.* for an Act of Parliament. It should be observed that the building has been erected by the ground landlord, who receives an adequate rent.

expences. At the Brighton Coal Gas Works upwards of 30,000*l.* have been expended ; and, I believe, without a material increase to their works, they would find it difficult to supply gas for 3000 lights.

The report from the gas-maker of the quantity of gas produced from a given quantity of oil, is equally satisfactory. This depends in a great degree on the nature of an oil ; for an equal quantity of gas cannot be procured from the very inferior sorts as from those of a better quality. Whale oil is found to be the best for the purpose ; cod oil somewhat inferior ; yet from eight tons of the latter containing 2016 gallons, 201500 cubic feet of gas were obtained, which is within a trifle of 100 feet per gallon. This is a large production from that description of oil ; but I see no reason, why, with good management, the same quantity should not always be produced ; much certainly depends upon this, but it is soon acquired by practice and observation. The regulation of the heat of the retorts, and supplying them properly with oil, is of the greatest importance in gas making. The Oldford establishment has been fortunate in obtaining an operator who has equalled their most sanguine expectations. There are inferior oils which it may be the interest of a Company to purchase from lowness of price ; these of course are not expected to produce so much gas ; and lowness of price is not always a recommendation, as there are other circumstances besides variation of product which render some oils less eligible than others. It has been observed that the advocates for oil gas do not make any allowance for waste arising from leakage of pipes, &c. This, at the Oldford Works, is very small indeed. The valve leading from the mains has been opened during the day, and kept so for some time with the smallest perceptible fall in the gasometer. This loss in the coal gas works, if what is stated be correct, is so great that it is difficult to account for it, and it would seem almost impossible it should arise from escape only. It is well known that coal gas injures the pipes it passes through ; which, in no degree, occurs with oil gas : may not this fact account for the difference of loss ?

The foregoing statements, I think, clearly prove the advantages which an oil gas concern is likely to yield in point of profit to those who embark in such an undertaking. To the consumer of gas, the advantages are equally palpable. An individual in the vicinity of the Works, has his premises lighted up with oil gas, and the following is the result of one year's observation ; it is somewhat more than a year, but I will not consider the extra time. He has in his shop five large argand burners burning from sun-set till nine o'clock, and on Saturdays till eleven o'clock. In his counting-house, he has two burners, and in his warehouse one ; these last are not constantly lighted, and he estimates them at one and a half, so that the number of lights

may be considered as six and a half, the average number of hours of burning per night is about three hours. I estimated this before at two and a half; it was said to be three and a half: I have taken the medium; this, allowing the extra time on Saturday, would be 20 hours per week, or 1,040 per year. The annual consumption will be about 8,800 cubic feet, for which at 50s. per 1000 feet, deducting five per cent. will be about 21l. besides the hire of the gas meter; each burner will consume about 1,350 cubic feet per year, which very little exceeds one and a quarter foot per hour. The light of these burners is fully equal to, if not exceeding, that of coal gas consuming five feet per hour, thus realising upon a large scale all my experiments, which have tended to prove that one foot of oil gas is equal to four feet of coal gas. The illuminating power of these two burners has been tried by a gentleman whose accuracy in experiments of this kind may be relied on, and whose interest is as great in the success of coal gas as of oil gas: his trials have all been followed by the same results. The cost too will be equally favourable for oil gas. The use of the gas meter gives an advantage over the ordinary mode of charging per light, as in the latter case the consumer must pay for a stated number whether he uses them the whole time or not; but supposing he should pay for six and a half burners, the charge would be 26l. and if this were estimated, according to the quantity consumed at 15s. per 1000 feet, and considering one cubic foot of oil gas as only equal to three and a half cubic feet of coal gas, as I originally stated it, the cost then would be 23l. 8s. making a difference considerably in favour of the former, and likewise showing the decided advantage of the gas meter. The above statement will, I think, be conclusive to any but the most prejudiced, of the superiority of oil gas over coal gas in an economical point of view.

Of the benefits arising from its introduction into private houses, no one can speak more satisfactorily than myself, nor can any one more seriously regret the necessity I have been under of relinquishing the comfort and luxury arising from it in consequence of my retirement here on account of my health. The whole of my house was lighted with it; and all the advantages, great as they were, which I anticipated from it, were infinitely more than realised. There was not a single annoyance, or the slightest inconvenience arising from the use of it; while the brilliancy of the light, the cleanliness, the saving of trouble, and many other comforts attending it, were a constant theme of admiration, and now as constant a source of regret. Could the benefits of this mode of lighting be generally known, and felt, and duly appreciated, I have no doubt that oil gas companies would be universally established, and every house lighted with it. It is difficult to say what the economy of this mode of lighting is, that must depend upon the quantity of light; it is, however, satisfactory to know, that there is no waste; you may

be as profuse or as economical as you please, and you may have double or treble the light at the same cost as tallow candles; four or five times as much if compared with sperm oil, and above twelve times as much if the comparison be made with wax candles.

I know not that any thing can be more satisfactory in favour of any new improvement than a successful issue of a fair and impartial trial; and the results which I have now stated, of a year's trial at the Oldford Works, may be equally applicable to any other establishment of a similar description. It can hardly be erected on more disadvantageous ground; and if similar success should not ensue, it cannot arise from the planning and execution of the work, but from some other cause wholly unconnected with the nature of the establishment, either from want of consumption, or from improper management, and to which any other concern is equally liable. The great question in the formation of a gas company is, Which is most eligible, coal or oil? Which is likely to absorb the least capital? Which is likely to promise the fairest return? Which is likely to be attended with least loss in case of failure? Which is likely to afford the greatest satisfaction to consumers in general? To these queries, I do not hesitate for one single moment answering *oil gas*. There is not a single point in which it has not the most decided advantage; and it is only because these advantages are not generally known, or that they are disallowed owing to the gross misstatements of those interested, that oil gas is not universally adopted. All means are resorted to to cry down oil gas; and I understand that in some of the provincial papers a warm contest has been kept up, and that the opinions of scientific men have been brought forward to prove the incorrectness of the statements concerning the comparative illuminating powers of the two gases. It would be difficult to account for the discrepancy of opinion which exists on this subject (some estimating it as one to two; others as one to two and a half; some again as one to three), if we did not know that the goodness of oil gas depends upon the construction of the apparatus, and the mode of using it, and that oil gas of all the qualities just mentioned may be produced. The gas upon which I have experimented, and upon which the observations I have before made were founded, was produced from works upon a large scale, erected by Messrs. Taylor and Martineau; and the peculiar excellence of their arrangement is, that the gas produced from the action of their apparatus and retorts is always of a superior quality, which was most satisfactorily proved by Dr. Henry, in his paper read before the Royal Society. It is needless for me here to enter into a detail of experiments which I was trying, and which, for the reasons I have before given, I was reluctantly obliged to give up. I may, perhaps, take some future opportunity of entering more fully into that subject; for

the present, I need only say, that the quantity of light may be augmented or diminished by different modifications of burners, by lessening or increasing the pressure of gas, at the same time enlarging or diminishing the external hole through which it passes. Indeed in measuring the proportions of light produced from oil and coal gas, there are so many circumstances to be considered, that no two experimentalists would be satisfied with the results obtained by others. The most satisfactory proof which can be brought forward is the practical result of observations made by a disinterested and intelligent consumer, and which is shown by a computation of pounds, shillings, and pence, more intelligible to those usually interested in gas concerns than all the philosophical or chemical experiments that can be tried.

I hope, from what I have above stated, it will not be supposed that I mean to underrate the advantages of science; you, Sir, I am sure, know me well enough, and have been sufficiently acquainted with my pursuits, to exonerate me from such a charge; but the present question is not so much one of science as it is of economy and utility; and in such a question, practical experience would certainly take the lead of scientific experiments. We all know the results of the inquiry on the famous oil question, which will not readily be forgotten. It must have had the effect of teaching the public, that in similar points of dispute, they must look to other authorities than the mere dicta of scientific men; and no authority can be more satisfactory than that which results from the agreement of scientific experiments with practical results.

I would have transmitted to you a copy of the report of the Oldford Oil Gas Company, but it is not yet printed: it details the progress of the concern, and expresses the satisfaction of the Committee with the favourable account which they are enabled to lay before the Proprietors, and bears testimony of the correctness of all the statements which were made by Messrs. Taylor and Martineau at the commencement, in all of which they have been fully borne out after a fair and satisfactory trial.

I am, yours, &c.

M. RICARDO.

### ARTICLE XIII.

*Abstract of a Memoir entitled "An Attempt to ascertain the Chemical Composition of those Minerals which possess the same Crystalline Form as Pyroxene."* By H. ROSE.\*

THE author of this paper observes, that there are many minerals which, from whatever place obtained, always possess the

\* From Schweigger's Journal, vol. v. New Series.

224. *M. Rose on those Minerals which possess [Manganese] same form and constituent parts.* Of this agreement he adduces feldspar and emerald as examples; but there are other minerals, as pyroxene, amphibole, garnet, and mica, which, possessing similar forms, wherever they occur, differ greatly in their composition. This last mentioned circumstance has occasioned great difficulty in arranging minerals according to their composition.

It is observed, that M. Mitscherlich has removed much of this difficulty by showing that certain bodies, when containing the same number of atoms, may exchange their places in compounds without inducing any alteration of crystalline form; according to M. Mitscherlich, most oxides which contain two atoms of oxygen are isomorphous; and although his experiments were made with artificial salts, it is presumed that the same obtains with respect to crystalline minerals.

To illustrate this position, M. Rose has observed that the analyses of different specimens of pyroxene show, that all minerals which have the crystalline form of pyroxene are bisilicates of the four isomorphous bases, lime, magnesia, protoxide of iron, and protoxide of manganese; in all these, the oxygen of the base is to that of the silica as 1 to 2. If the reasoning of M. Mitscherlich be applied to the analysis of various substances classed by Häuy with pyroxene, it will be observed that they agree in composition with that substance; viz. augit, malacolite, sahlite, baikalite, coccolite, alakit, mussyite, diopside, and fassaite.

The following analyses were made by M. Rose in Prof. Berzelius's laboratory at Stockholm, and most of them were several times repeated; the minerals which possess the crystalline form of pyroxene are classed by M. Rose under the following heads:

1. Pyroxenes with lime and magnesia as bases.
2. Pyroxenes with lime and protoxide of iron as bases.
3. Pyroxenes with lime, magnesia, and protoxide of iron, as well as some protoxide of manganese as base.
4. Pyroxenes with lime and protoxide of manganese as bases.

#### *1. Pyroxenes with Lime and Magnesia as Bases.*

These pyroxenes are mostly white, and form the colourless and usually transparent malacolites, which are sometimes, by slight admixtures, yellowish and greenish. The proportion of lime and magnesia is in almost all which have been analysed very constant, both having the same quantity of oxygen, and together half as much as the silica.

#### *White Malacolite from Orrjervi, in Finland.*

Is white, only greyish when mixed with galena; occurs in large crystalline masses with distinct cleavage; semihard, and very translucent at the edges. Mr. Rose's analysis gives

Silica. ....	54.64	containing oxygen	27.48
Lime. ....	24.94		7.0
Magnesia. ....	18.0		6.97
Oxide of manganese ..	2.0		
Oxide of iron with magnesia. ....	1.08		
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	100.66		

*Yellow Malacolite from Longbanshyttan, in Wermeland.*

It occurs with red silicate of manganese in magnetical iron ore. Its colour is yellowish; its powder of a lighter colour; it is semihard; gives with difficulty sparks with steel; is translucent at the edges. M. Rose's analysis gave

Silica. ....	55.32	containing oxygen	27.82
Magnesia. ....	16.99		6.58
Lime. ....	23.01		6.46
Oxide of manganese ..	1.59		
Oxide of iron. ....	2.16		
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	99.07		

M. Hissinger has analysed another malacolite from Longbanshyttan; the composition is nearly the same.

Silica. ....	54.18	containing oxygen	27.25
Magnesia. ....	17.81		6.89
Lime. ....	22.72		6.38
Oxide of manganese. ..	2.18		
Oxide of iron. ....	1.45		
Loss by heating. ....	1.20		
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	99.54		

M. Bonsdorf, of Abo, has analysed a white malacolite from Tammare, in Finland, which gave the following results:

Silica. ....	54.83	containing oxygen	27.58
Lime. ....	24.76		6.95
Magnesia. ....	18.55		7.18
Alumina. ....	0.28		
Oxide of iron. ....	0.99		
Loss by heating. ....	0.32		
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	99.73		

The Count of Trolle Wachtmeister analysed a white malacolite from Tafel Tyotten, in Norway; its composition was,  
*New Series*, VOL. V. Q

Silica. ....	57.40	containing oxygen	28.87
Lime. ....	23.16		6.48
Magnesia. ....	16.74		6.48
Alumina. ....	0.43		
Protoxide of iron. ....	0.20		
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	97.87		

Pyroxene from Pargas gave Mr. N. Nordenskiöld the following composition :

Silica. ....	55.40	containing oxygen	27.7
Lime. ....	15.70		4.4
Magnesia. ....	22.57		9.3
Oxide of manganese ..	0.43		
Alumina. ....	2.83		
Oxide of iron. ....	2.50		
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	99.43		

## 2. *Pyroxenes with Lime and Protoxide of Iron as Bases.*

Hedenbergit from Tunaberg, in Sadermanland.\*

The colour is greenish-black ; it is semihard in a high degree. It occurs among the masses thrown out from a mine which is now deserted (Marmorsgufvan) not far from the cobalt mines at Tunaberg, together with quartz, partly pure and large foliated ; partly granular with magnetical iron ore. M. Rose's analysis gave the following result :

Silica. ....	49.01	containing oxygen	24.65
Lime. ....	20.87		5.86
Protoxide of iron. ....	26.08		5.93
Magnesia and oxide of manganese. ....	2.98		
	<hr/>		
	98.94		

## 3. *Pyroxenes with Lime, Magnesia, and Protoxide of Iron, as Bases, together with more or less Protoxide of Manganese.*

In these varieties of the pyroxene, a constant proportion of the bases is wanting ; and there is, therefore, a great variety in the composition. Among all varieties of this series of pyroxenes, there are not two which have their bases combined in the same proportion ; even pyroxenes from the same place vary in their composition : all, however, follow the law which M. Rose discovered ;

\* Hedenberg discovered this mineral, which he likewise first analysed. The result of his analysis is, however, very different from M. Rose's. Specimens which were obtained from Prof. Berzelius's collection were first analysed, and afterwards such as M. Rose brought with him from the place. Both analyses agreed completely in their results.

the oxygen of all the bases being equal to one half of that of the silica.

*Green Malacolite from Bjormyresoeden, in Dalecarlia.*

Its colour is leek-green; the powder greenish-yellow; it is semihard in a high degree; and on the edges scarcely translucent. It occurs in magnetical iron ore. The analysis gave the following result:

Silica. ....	54.08	containing oxygen	27.20
Lime. ....	23.47		6.59
Magnesia. ....	11.49		4.45
Protoxide of iron. ....	10.02		2.28
Protoxide of manganese	0.61		
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99.67			

*Green Malacolite from Bjormyresoeden, another Variety.*

It is scarcely different from the former in hardness, colour, transparency; in streak and lustre; it is in fact almost completely like the former. The analysis gave the following result:

Silica. ....	54.55	containing oxygen	27.45
Lime. ....	20.21		5.68
Magnesia. ....	15.25		5.90
Protoxide of iron. ....	8.14		1.85
Alumina. ....	0.14		
Oxide of manganese ..	0.73		
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99.02			

*Black Crystallized Pyroxene from Taberg, in Wermeland.*

Its colour is raven-black; the powder greyish-green; it is semihard; opaque; occurs on a bed of iron ore with epidote, asbestos, and tremolite. The analysis gave the following result:

Silica. ....	53.36	containing oxygen	26.84
Lime. ....	22.19		6.23
Protoxide of iron. ....	17.38		3.95
Magnesia. ....	4.99		1.93
Manganese. ....	0.09		
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98.01			

If the magnesia is not taken into consideration, this pyroxene belongs to the second division. Even in external appearance, it is like the hedenbergite, which belongs to that division.

*Green Sahlite from Sahla.*

The colour is light oil-green; the powder white. It gives

sparks with the steel, but with difficulty. It is in a high degree translucent at the edges. Occurs in calcareous spar. It was analysed by M. Rose, and the results obtained were:

Silica. ....	54.86	containing oxygen	27.59
Lime. ....	23.57		6.62
Magnesia. ....	16.49		6.38
Protoxide of iron. ....	4.44		1.00
Manganese. ....	0.42		
Alumina. ....	0.21		
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	99.99		

The composition of this malacolite is, if  $4\frac{1}{2}$  per cent. of protoxide of iron are not taken into consideration, the same as those of the first division. Before the blowpipe also, its properties are completely like them. There occur, however, at Sahla other kinds of sahlite, which, though similar in external appearance to this, are quite different in their chemical properties and composition; while the sahlite just now described, and all other malacolites of this composition, are easily fusible before the blowpipe, these are almost entirely infusible; and if reduced powder, it merely agglutinates a little. In a small glass tube, they blacken on the first action of the flame; in the open fire, they become white.

The colour of these sahlites is the same as that of the before mentioned, but their lustre is much less, and so soft that they are scratched by the nail; while the other sahlite gives sparks with steel. They occur in carbonate of lime, and are usually penetrated by small veins of galena. The analysis of these sahlites occasioned much trouble in ascertaining their true nature.

One of the purest specimens is in the collection of Prof. Berzelius, who was kind enough to supply as much of it as was sufficient for a chemical analysis. This sahlite, after having been deprived by diluted nitric acid of calcareous spar, lost on heating in different experiments 4.15, 4.92, 4.34, and 4.11 per cent. which distinguishes it sufficiently from the common malacolite, of which the loss in the fire never amounts to one per cent.\*

The powder, after being heated, had a brown-red colour, and gave the following results:

\* The hard sahlite lost in the fire 0.48 per cent.; the first variety of the malacolites from Bjornyresöeden only 0.12; the second 0.22; and the hedenbergite 0.7. This latter mineral when heated in a small glass tube by the lamp of the glass blower, gives out a sour liquid which seems to contain fluoric acid.

Silica. ....	63.21	containing oxygen	31.79
Lime. ....	5.18		1.45
Magnesia. ....	26.26		10.16
Protoxide of iron. ....	4.36		0.99
Oxide of manganese ..	0.82		
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	99.83		

It is clear that a great surplus of silica exists in this mineral, and that it is not a bisilicate. This exception to the common rule which existed in every other analysis was unexpected. M. Rose repeated the analysis twice, but always obtained the same result. Fragments of it, when distilled in a small apparatus, gave out water, which did not change any of the test papers. M. Rose endeavoured to find fluoric acid in this sahlite by melting it with carbonate of soda, dissolving the mass in water, evaporating the liquid to a small bulk, separating it from the silica, supersaturating it with muriatic acid, mixing it with excess of ammonia, and adding muriate of lime, after which the vessel was carefully closed. No precipitate of fluete of lime appeared, not even after several days; and only a trace of silica was deposited.\*

A considerable quantity of another sahlite was distilled in a porcelain retort; the loss amounted to 3.17 per cent. Water was distilled, and at last fumes passed through the aperture of the receiver, which smelt like a mixture of sulphurous and sulphuretted hydrogen. The water in the receiver was slightly sour; when saturated with ammonia, and slowly evaporated in a stove, it left only a small trace of salt of ammonia, which, when heated, evaporated like muriate of ammonia, and before the blowpipe with silica and soda, gave a brown button. By another distillation, when the receiver was kept very cold, a fluid was obtained, which, in the beginning, was turbid, smelt of sulphur, and on being saturated with ammonia, visible traces of sulphur were thrown down. When the sulphur had been separated, this solution was evaporated; the same salt remained as before, the brown colour which it communicated before the blowpipe to the glass of silica and soda, proved to be sulphate of ammonia. This sulphurous acid evidently derives its origin from the sulphuret of lead which occurs disseminated in the sahlite. The very insignificant quantity shows that it cannot be the produce of one of the constituent parts of the sahlite (the water of a perfectly pure sahlite, of which the analysis has been communicated above, did not contain any trace of it). Besides the sulphurous smell, an empyreumatic one was observed in the water, which is peculiar to the water

\* When the liquid which had been separated from the silica was evaporated to a small bulk, and a mixture of sulphuric acid and alcohol poured on it, it burned with a green flame, which, however, depended upon the muriatic ether, and not on boracic acid.

obtained from all magnesian minerals, as, for example, serpentine.

It was, however, necessary to ascertain, whether the whole loss in the fire consisted of water. A quantity of another specimen was, therefore, distilled, which, in different experiments, had lost 3·09, 2·99, and 3·25 per cent. on a red heat, in a porcelain retort with a receiver, to which was adapted a tube with muriate of lime. This experiment, which was made with the greatest care, was decisive, for no gas was given out, and the loss of weight of the retort answered completely to the increase of the receiver, and the tube with muriate of lime.\*

Fragments of the specimen which had served for this experiment, were employed for two different analyses. The fragments for the first analysis were taken from one end of the large piece; those which served for the second were from the opposite end. The result of these two analyses was :

Silica. ....	58·08	containing oxygen	29·21
Lime. ....	11·24		3·16
Magnesia with manganese . ....	22·28		8·62
Protoxide of iron . ....	5·30		1·20
Alumina . ....	0·47		
	<hr/>		
	97·37		

Silica. ....	58·30	containing oxygen	29·32
Lime . ....	9·89		2·78
Magnesia . ....	24·22		9·37
Manganese . ....	0·68		
Protoxide of iron . ....	4·24		0·96
Alumina . ....	0·11		
	<hr/>		
	97·44		

If 3·11 per cent. of water are added, which is the mean quantity of the loss that the mineral sustained by a red heat, a small increase is obtained in the analysis. Though the oxygen in the surplus of silica is equal to that of the water obtained, and it, therefore, might appear proper to consider the hydrate of silica as isomorphous with the form of the pyroxene; yet no true hydrate of silica being either found in nature, or produced by art, there exists no reason why it should occur here. It seems as if the distinguishing property of those sahlites, which, on being exposed to heat in a covered vessel, become black, give out water,

\* The quantity of purified sahlite which was used in this experiment weighed 35·916 grammes. The retort had lost 1·168 grammes; the receiver had gained 1·151,† and the tube with muriate of lime 1·009 gramme.

† The number is most probably 0·151.—*Edit.*

and are afterwards white, and which are not fusible, or whose fusibility diminishes in the same proportion as the water they contain increases, depends upon an infusible mineral, which contains water, and in the fire becomes first black, and afterwards white. Soap stone and noble serpentine are easily recognised by these properties, and these with common serpentine occurring in great quantity with the sahlite, it is clear that these varieties of sahlite are nothing but pyroxenes; that is to say, bisilicates of lime and magnesia combined with variable mixtures of soap stone or noble serpentine, of which the first is a trisilicate without water; the latter a trisilicate with water. Neither soap stone nor serpentine has the least tendency to crystallization, though both are chemical compounds; they are, therefore, no obstacle to the peculiar crystallization of the sahlite, it being already known from several examples, that both soap stone and serpentine possess a peculiar tendency to assume the crystalline form of other minerals.\*

*Reddish-brown Malacolite from Degaro, in Finland, analysed by Prof. Berzelius.*

Silica. ....	50.00	containing oxygen	25.15
Lime. ....	20.00		5.62
Magnesia. ....	4.50		1.74
Protoxide of iron. ....	18.85		4.29
Protoxide of manga- nese. ....	3.00		0.66
Loss by heating. ....	0.90		
			<hr/>
			97.25

(To be continued.)

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## ARTICLE XIV.

### *Proceedings of Philosophical Societies.*

#### ROYAL SOCIETY.

Jan. 23.—The reading of Mr. Macdonald's Observations on Magnetism was resumed and concluded. The principal subjects of them were the phenomena of the variation of the needle, to account for which, an hypothesis, in some respects new, was proposed by the author. It would appear, he observed, from Capt. Parry's discovery of the north-west magnetic pole, and

\* M. Rose distinctly found afterwards, on closer examination, in one specimen of these sahlites, serpentine mixed with it, which, from its exterior, hardly would have been distinguishable from the sahlite, had he not supposed it to exist in the mixture.

from other circumstances, that what we call the *variation* of the needle, is, in fact, no variation at all, so that the north-pole of the earth may not, in reality, possess any magnetic attraction. The situation of the pole discovered by Capt. Parry, by a rough computation from the amount of the dip at various spots in its vicinity, as given by him, is at the intersection of  $73^{\circ}$  north latitude and  $101^{\circ}$  west longitude. Mr. M. has ascertained, that the oscillations of the needle are isochronous; and also, that when the north pole of a magnet is presented to the south pole of the needle, the oscillations describe segments continually decreasing, but are still performed in equal times: now if the north pole of the earth have any attraction, the oscillations of the needle, when upon the line of no variation in the neighbourhood of the north-west pole, which was crossed by Capt. Parry, ought to be accelerated on approaching it; and by this means the fact may be ascertained.

Mr. Macdonald supposes that the north-west magnetic pole has a rotatory motion, producing the two lines of no variation in the northern hemisphere, and that during the 159 years, from 1667 to 1816, in which the needle was advancing to the west, it described one-fourth of its orbit. This theory of the motion of the pole, he remarked, required one objection to be obviated,—the supposed solidity of the earth: but of this opinion, he stated, there was no more physical proof than of the contrary one; the subject was one of the hidden secrets of God never to be discovered. The Mosaic records indicate the earth to be full of water, contained as in a shell; and many passages of Scripture might be adduced to confirm this indication; the mode in which the earth must have acquired its figure, and several astronomical facts, likewise tend to show that it does not consist of solid matter increasing in density to the centre.

Mr. M. supposes that there is also a south-east magnetic pole, by the rotation of which the two lines of no variation in the southern hemisphere are produced; Capt. Cook came to a spot where the dip amounted to rather more than  $70^{\circ}$ ; and this pole may be discovered or approximated to by sailing in the line of no variation at New Holland, as far as the ice will permit. The circumstance that the aurora borealis is never seen to rise, in Greenland, either in the north or north-west, but in the south-east or east, was cited from Crantz's History, in confirmation of this idea of a south-east magnetic pole.

In his former papers on magnetism, inserted in the Philosophical Transactions for 1796 and for 1797, the author had adopted Dr. Halley's theory of four magnetic poles; but two of these, it had since been found, did not exist where they were stated to be. In those papers, likewise, he had ascribed certain effects to the action of the sun's rays upon the earth, which, in consequence of Sir H. Davy's electro-magnetical discoveries, he was now disposed to attribute to galvanic agency; the diurnal

variation of the needle might still be connected with the influence of the sun; as, indeed, its increase from March to October seemed to prove. Mr. Macdonald further suggested, that a magnetical battery might be constructed, perhaps, by a proper arrangement of positive and negative poles; and concluded his paper with expressing his hopes, that what he had stated might lead some to pursue the subject, who were better qualified than himself for the investigation of it.

The Society then adjourned over the anniversary of the martyrdom of King Charles I.

*Feb. 6.*—A letter to the President from Sir Thomas Brisbane, Bart. FRS. dated Government House, Paramatta, New South Wales, Sept. 6, 1822. In this were communicated the results of the first observations made at the Observatory at Paramatta, by Mr. Charles Rumker; they related to the obliquity of the ecliptic at Paramatta, to the longitudes of Paramatta and Sydney, to the rediscovery of M. Eacker's triennial comet, and to the length of Kater's invariable pendulum vibrating seconds at Paramatta.

At the same meeting was read, An Account of some Caves discovered in the Limestone Quarries at Oreston; by J. Whidbey, Esq. in a letter to J. Barrow, Esq. FRS.

The two caves described in this paper were discovered, at the elevation of 93 feet above the sea at high water in spring-tides, in the quarries upon the Cat-water; from which the stones employed in the Breakwater is procured. One was thinly lined with stalactite, and the bones it contained were imbedded in clay and rubble; in the other, they adhered to the sides; these caves communicate with each other by a sort of gallery, which opens to the face of the quarry about the size of a man's body. They have been examined by Prof. Buckland and Mr. Warburton. Their form and position were illustrated by a drawing annexed to the paper.

Annexed to Mr. Whidbey's paper, was A Description of the Bones found in the Caves above-mentioned; by Mr. W. Clift, Conservator of the Museum of the Royal College of Surgeons; communicated by Sir E. Home, VPRS.

The contents of the caves discovered at Oreston in 1816 and 1820, which had been described in the Philosophical Transactions for 1817 and for 1821, were altogether different from those of the present, discovered in 1822. In the first instance, the bones all belonged to a species of rhinoceros; and in the second to a species of bear, and to an unknown antelope or deer: those now under consideration belonged to the known and existing genera of the ox, the deer, the horse, the hyæna, the wolf, and the fox. Some of them were thinly invested with stalactite, but the greater number were firmly imbedded in clay. None had been gnawed, except the radius of a young wolf, which presented traces of the canine teeth and incisors, of an animal apparently

about the size of a weasel. The bones of the various graminivorous animals were found together; but those of the carnivora at a distance from each other. All were very fragile and white; some were treated with muriatic acid, and found to have lost nearly all their animal matter; while others examined by Prof. Buckland retained about one-third less than those of Kirkdale. The proportion of animal matter retained by fossil bones, varies very considerably in different specimens. In the Museum of the Royal College of Surgeons, there are some teeth of the mastodon, from the banks of the Ohio, which have been deprived of their earthy matter by means of muriatic acid, but still showing their whole form. Mr. C. suggested, that the clay in which the Oreston bones were found, and which, in their immediate vicinity, was much blacker as well as more tenacious and solid than in any other part, might have abstracted the animal matter from them. They are so absorbent of moisture, that the largest adheres to the tongue with sufficient strength to support its whole weight. When immersed in water, much effervescence took place, and the bones became black; but resumed their usual appearance on being dried; this was particularly the case with those of the carnivorous animals. In consequence of their fragility, some of them were broken by the workmen while divesting them of the clay; while others fell to pieces on being exposed to the air. In respect to the latter circumstance, they resemble the tusks, &c. of the elephant, found in the sand above the blue clay at Brentford, Ilford, and other places near the river Thames, which divide into lozenge-shaped or into cubic fragments.

Fossil bones showing traces of disease, Mr. Clift observed, are extremely rare; and he has never seen any that exhibited fractures which had been healed during the life of the animals: there are, however, among these from Oreston, the metacarpal and metatarsal bones of an ox, which bear evidences of ossific inflammation; and the lower jaw of a young wolf, in which two abscesses, one on each side, have produced sinuses.

The fragments of shell found in one of the caves, retain their pearly matter, and appear to be those of an ostrea; but they are too small to present any satisfactory characters, not amounting to the bulk of a single valve.

A more particular description of the bones, illustrated with drawings, succeeded the above observations. There are bones of about twelve oxen, with short conical horns, standing upright; and larger than the medium size of the existing species of that genus. A few seem to belong to a deer, but this cannot be satisfactorily determined, as there is neither the head of the animal, its horns, nor its teeth. Some small bones of a young animal, apparently a calf or a fawn. The bones and teeth of about twelve horses, which must have been fourteen hands high. The bones of five or six hyenas, including two jaw-bones with

teeth. The bones of several wolves, of the same size as those of the existing species. The whole of these remains have been deposited in the Museum of the Royal College of Surgeons, by Sir E. Home.

Some further particulars of the quarry and caves, by Mr. Whidbey, were appended to Mr. Clift's paper. Mr. W. stated, that no more bones were likely to be discovered; for the rock containing the caves was very nearly worked out; and he expressed his opinion, that no communication had existed between the caves and the surface of the country since the Flood.

*Feb. 13.*—A Letter to the President from T. Young, MD. Foreign Secretary to the Society, was read: it related to Mr. Rumker's rediscovery of Prof. Encker's little triennial comet, near the place which the Professor had assigned to it by computation. Mr. Rumker first observed it on the 2d of June last, and it continued visible until the 23d, when it was lost in the light of the moon, and it could not afterwards be discovered.

At this meeting, also, part of a paper by Mr. Goldingham was read, relating to Experiments on the Velocity of Sound, made at Madras.

## ARTICLE XV.

### SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

#### I. *Splendid Meteor seen on Oct. 28, 1822.* By Mr. Davenport.

(To the Editor of the *Annals of Philosophy*.)

SIR,

London, Jan. 18, 1823.

Will you allow me room in your publication to inquire whether any of your correspondents witnessed an extraordinarily fine meteor that appeared on the 28th of last October?

I was travelling northward on the Hastings' road, and going slowly up Silver Hill, which is about 48 miles south-east of London, by road measurement, at about half-past five on the above-mentioned day, the sky being clear, the moon shining bright, and nearly full, the sun below the horizon, but the twilight still strong; I saw on a sudden in the sky about north-east, a luminous ball, of full one-third the apparent diameter of the full moon, giving a remarkably bright and white light. Its height above the horizon I consider to have been about  $22^{\circ}$ ; but being myself on a steep ascent, I could less easily judge of this. It passed towards the west in a horizontal direction, and the line of its motion, while I saw it, subtending an angle of above  $20^{\circ}$ , during about eight seconds of time. Unfortunately, it passed behind a loaded waggon; but as I pushed on to regain the view of it, I thought (but was not certain) that I saw a faint remainder of it for an instant.

If this meteor should have been observed from any distant place, so

that by comparison of the apparent motion, direction, and bearings, its height above the earth can be estimated, I will endeavour, when I revisit the spot, to ascertain its relative bearings and apparent altitude with greater accuracy; as I can fix on the spot on the road from which I viewed it, and also on the objects in the line over which I traced it.

I am, Sir, your obedient servant,

RICHARD DAVENPORT.

## II. *Analysis of the Ashes ejected from Vesuvius, during the late Eruption.* By Prof. V. P  p  , of Naples.

In ten ounces of these ashes, which correspond to 6000 grains, there are, according to this extraordinary analysis, 186 grains of saline substances, viz.

Sulphate of potash.....	9½
Sulphate of soda.....	44
Sulphate of lime.....	57
Sulphate of magnesia.....	16
Sulphate of alumina.....	14
Hydrochlorate of potash.....	9
Hydrochlorate of soda.....	31
	<hr/>
	180½
Loss.....	5½
	<hr/>
	186

Oxide of aluminium.....	1800
Oxide of calcium.....	300
Oxide of silicium.....	1200
Oxide of magnesium.....	300
Tritoxide of iron.....	1800
Antimony.....	360
Gold.....	13½
Silver.....	6½
	<hr/>
	5966
Loss.....	34
	<hr/>
	6000

However scrupulously the distinguished Professor has occupied himself in the search of other substances, he has not been able to find any more!—(*Giornale delle due Sicilie*, Nov. 16, 1822.)

## III. *Tutenag and White Copper of China.*

In the *Annals* for Sept. last, vol. iv. p. 236, we gave from No. 13 of the *Edinburgh Philosophical Journal*, Dr. Fyfe's analysis of the tutenag, or white copper of China. In the original paper, Dr. F. commences with some remarks on the very different statements which have been made respecting the composition and origin of tutenag; but it now appears that these may have arisen from the circumstance, that the two appellations have been applied promiscuously to two distinct substances. In No. 15 of the *Edinburgh Journal* are some observations

on the subject by Sir T. Dick Lauder, Bart. FRSE. in which he states, on the authority of a friend employed for many years in the trade between China and India, "that the substance analyzed by Dr. Fyfe is not tutenag, but white copper, the properties of which are totally different;" and he then proceeds as follows:

"The white copper is used by the Chinese themselves, who are so jealous of permitting other nations to have it, that its exportation is contraband. In defiance of this, however, considerable quantities of it are smuggled out of the country, and introduced into India, where it is considered as a great present to the Hindoos, &c. who make domestic vessels of it. The tutenag, on the contrary, is an article of very extensive commerce between China and India; and my friend informs me, that it is sent from China in slabs, of which he has had occasion to buy and sell many thousands. The slabs are about eight or nine inches long, by about five and a half wide, and about five-eighths thick. Its colour is greyish; and it is not malleable, but so brittle that it is even necessary to use considerable caution in putting it on ship-board, to prevent its being broken by one piece striking against another. The fracture has a glittering lustre, and somewhat resembles the appearance exhibited by that of bad iron; but the crystallization (if such a term may be employed) is larger. It does not ring, but emits a heavy clattering sound. It is employed by the natives of India as an alloy for copper, to make brass for their domestic utensils."

## ARTICLE XVI.

### NEW SCIENTIFIC BOOKS

#### PREPARING FOR PUBLICATION.

Dr. Baron, of Gloucester, has undertaken to write an Account of the Life of the late Dr. Jenner, and to arrange for publication the manuscripts of that distinguished character; for which purpose all the documents in possession of the family, have been committed to his care.

Mr. Thomas Clarke is preparing for publication a new System of Chemical Nomenclature, exhibiting not only the component Parts of Compound Substances, but also the precise Proportion of these Parts.

Sir W. Gell has in the press, a Narrative of a Tour through the Morea, giving an Account of the present State of that Peninsula, and its Inhabitants.

#### JUST PUBLISHED.

The Encyclopædia Metropolitana, Part 8. 4to. Price 1*l.* 1*s.*

An Inaugural Lecture on the Study of Chemistry, read at the Ashmolean Museum, Nov. 2, 1822. By Charles Daubeny, MD. FRS. MGS. Professor of Chemistry, and Fellow of Magdalen College, Oxford.

The British Flower Garden. By Robert Sweet. No. I. with Four coloured Plates, from Drawings by E. D. Smith. Price 3*s.*

## ARTICLE XVII.

## NEW PATENTS.

H. Ibbotson, Sheffield, fender-manufacturer, for a fender capable of being extended or contracted in length, so as to fit fire-places of different dimensions.—Nov. 28.

J. Dixon, Wolverhampton, Staffordshire, brass-founder, for improvements on cocks, such as are used for drawing off liquids.—Nov. 28.

J. Woollams, Wells, Somersetshire, land agent, for improvements in wheeled carriages.—Dec. 5.

W. Robson, St. Dunstan's-hill, Tower-street, printer and stationer, for a method to prevent or protect against fraudulent practices upon bankers' checks, bills of exchange, and various species of mercantile, commercial, and other correspondence.—Dec. 10.

J. Perkins, Fleet-street, late of Philadelphia, engineer, for improvements in steam-engines. Communicated to him by a foreigner.—Dec. 10.

S. Parker, Argyle-street, Westminster, bronzist, for improvements in the construction of lamps.—Dec. 10.

W. Bundy, Fulham, Middlesex, mathematical instrument maker, for a machine for breaking, cleaning, and preparing, flax, hemp, and other vegetable substances containing fibre.—Dec. 16.

T. B. W. Dudley, King-street, Westminster, mechanist, for a method of making or manufacturing malleable cast-metal shoes for draft and riding horses, and other animals, upon a new and improved plan or principle.—Dec. 16.

J. Nicholson, Brook-street, Lambeth, engineer, for apparatus for the more conveniently applying heat to certain instruments of domestic use.—Dec. 16.

J. Dumbell, Howley House, Warrington, Lancashire, merchant, for improvements relative to carriages.—Dec. 16.

J. Bainbridge, Bread-street, Cheapside, merchant, for improvements on rotatory steam-engines. Communicated to him by Amos Thayer, jun. of Albany, America, mechanist.—Dec. 16.

M. Wilks, Dartford, Kent, seed-crusher, for a new method of refining oil produced from seed.—Dec. 20.

T. Linley, Sheffield, Yorkshire, bellows-maker, for a method of increasing the force or power of bellows.—Dec. 20.

Sir J. Jelf, Oaklans, Gloucestershire, for a combination of machinery for working and ornamenting marble and other stone for jams, mantles, chimney-pieces, and other purposes.—Dec. 20.

J. I. Hawkins, Pentonville, civil engineer, and S. Mordan, Union-street, City-road, portable pen-maker, for improvements on pencil-holders, or port crayons, and on pens, for the purpose of facilitating writing and drawing.—Dec. 20.

W. Pass, Curtain-road, Shoreditch, dyer, for an improvement in calcining and smelting of various descriptions of ores.—Dec. 20.

## ARTICLE XVIII.

## METEOROLOGICAL TABLE.

1823.	Wind.	BAROMETER.		THERMOMETER.		Evap.	Rain.	Daniell's hyg. at noon.
		Max.	Min.	Max.	Min.			
1st Mon.								
Jan. 1	Var.	29.94	29.91	37	29	—	—	
2	S E	30.02	29.94	44	36	—	25	
3	S E	30.02	29.93	44	36	—	—	
4	S E	29.93	29.91	40	37	—	26	
5	S E	30.03	29.91	43	37	—	25	
6	S	30.25	30.03	45	35	—	—	
7	N E	30.30	30.25	41	32	—	—	
8	N E	30.25	30.16	40	20	—	—	
9	N E	30.16	30.05	32	21	—	—	
10	E	30.07	30.05	33	23	—	—	
11	N E	30.09	30.08	31	22	—	—	
12	N E	30.08	29.88	30	19	—	—	
13	N E	29.88	29.78	30	15	—	—	
14	E	29.78	29.48	31	14	—	—	
15	N E	29.55	29.48	31	20	—	—	
16	N W	29.58	29.55	34	26	—	—	
17	N W	29.58	29.57	34	26	—	—	
18	N W	29.69	29.57	34	6	—	—	
19	S W	29.89	29.69	19	4	—	—	
20	N W	30.05	29.89	32	19	—	—	
21	N E	30.13	30.05	33	22	—	—	
22	N E	30.13	30.02	28	20	—	—	
23	E	30.02	29.96	27	21	—	—	
24	N E	30.01	29.96	30	20	—	—	
25	E	30.01	29.98	28	22	—	—	
26	N E	29.98	29.89	31	25	—	—	
27	S E	29.89	29.68	40	29	—	—	
28	S W	29.68	29.33	46	42	—	1.05	
29	S E	29.56	29.33	50	39	—	04	
30	S W	29.56	29.34	46	40	—	—	
31	E	29.34	28.97	41	37	.42	20	
		30.30	28.97	50	4	.42	2.05	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

## REMARKS.

*First Month.*—1. Cloudy. 2. A gentle thaw with some rain. 3. Fine. 4. Cloudy. 5. Rainy. 6—8. Fine. 9. Hoar frost: fine. 10. Cloudy. 11. Fine: afternoon overcast. 12. Bleak. 13. Some snow this morning. 14. Fine. 15. In a heavy snow which commenced this morning, a flock of some hundreds of wild geese passed over us about 11, a. m. steering their course to the east. The snow fell to about four inches depth on the level. 16. Some snow at 10, p. m. 17. Cloudy. 18—20. Much rime on the trees; which, being rather loosely attached, a part fell as it collected, forming a regular snow shower under the trees: the latter nevertheless retained at length a sufficient quantity of the icy foliage to enable them to cast a full shadow on the ground as in summer, which had altogether a singular appearance. 21. Fine. 22—24. Cloudy. 25. It began to snow at three, p. m. and the fall continuing through all the night, covered the ground to five or six inches on the level. 26. Cloudy. 27. Some hail at nine, a. m. followed by rain, which continued till near four, p. m. 28. Rain. 29—31. Cloudy. During the intense cold of the month, much ice accumulated in the Thames, the navigation of which was for some considerable time suspended for the smaller vessels. The feathered tribes exhibited in various ways the appearance of distress usual with them on such occasions by changing their quarters, and seeking food nearer than usual to the houses and villages.

## RESULTS.

Winds: NE, 11; E, 5; SE, 6; S, 1; SW, 3; NW, 4; Var. 1.

## Barometer: Mean height

For the month.....	29.854 inches.
For the lunar period, ending the 4th .....	30.287
For 13 days, ending the 2d (moon north) .....	30.239
For 14 days, ending the 16th (moon south).....	29.946
For 13 days, ending the 29th (moon north) .....	29.888

## Thermometer: Mean height

For the month.....	30.629°
For the lunar period.....	33.166
For 30 days, the sun in Capricorn .....	30.383

Evaporation..... 0.42 in.

Rain..... 2.05

*Laboratory, Stratford, Second Month, 22, 1823.*

R. HOWARD.

# ANNALS

OF

## PHILOSOPHY.

APRIL, 1823.

### ARTICLE I.

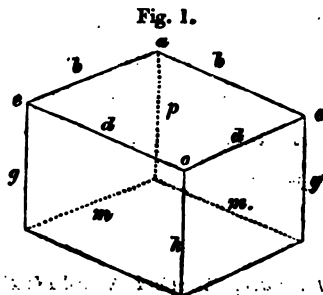
*Description of a new Mineral.* By Mr. A. Levy, MA. of the University of Paris.

(To the Editor of the *Annals of Philosophy*.)

SIR,

Great Russell-street, Bloomsbury, March 3, 1823.

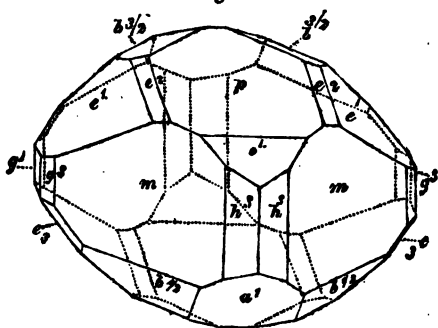
I SHALL again beg the favour of your inserting in the *Annals of Philosophy* the crystallographical description of a mineral which I cannot refer to any substance whose primary form has been determined hitherto. It occurs in small brilliant yellowish-brown crystals, with adularia and lamellary crichtonite, and comes from Dauphiny. On account of its colour, accompaniment, and locality, it has been classed with sphene; but as it will appear from what follows, it differs essentially from this last substance. Its hardness is much less than that of sphene; it cleaves very easily in one direction, and the face of cleavage is perfectly brilliant. The primary form is an oblique rhombic prism, fig. 1, in which the incidence of the two lateral planes  $m, m$ , is  $96^{\circ} 10'$ , that of the base  $p$  on one of the lateral planes  $99^{\circ} 40'$ , and the ratio between one side of the base and one of the lateral edges nearly that of 10 to 7. The primary form of sphene is also an oblique rhombic prism, but its dimensions are different. The incidence of the two lateral



planes is  $137^{\circ} 5'$ , and that of the base on one of the lateral planes  $94^{\circ} 3'$ . Spheue presents in the series of its secondary forms many oblique rhombic prisms, the lateral planes of which are produced by modifications either on the lateral edges, two of the sides of the base, or the lateral angles; and the base is, either the base of the primary or some secondary plane produced by a decrement on the angles  $a$  or  $o$ . None of these secondary oblique rhombic prisms that I have observed, nor any of those mentioned by Mr. Rose in the excellent paper on spheue, he has published at Berlin, have their planes inclined at the same angles as those of the primary of the substance I am describing. The secondary oblique prisms of this substance do not either in their measurements correspond with any of the oblique rhombic prisms of spheue. I have also tried whether I could not deduce by some law of decrement, the form of this new substance from the primary of spheue, and I have found it was not possible without assuming very complicated laws. I believe, therefore, I am entitled to consider it as a new species. Mr. Heuland has proposed to me to call it *Turnerite*, from the name of the gentleman in whose collection it was first noticed as a distinct species. This tribute is certainly well due to Mr. Turner, who has undertaken with so much readiness and liberality the publication of a detailed description of his most valuable collection. This new substance is very scarce, and besides the specimen where I have observed it, Mr. Heuland knows but one more in England. I could only detach a single crystal of it, and, therefore, could not attempt to determine its specific gravity, or to have it analysed. I hope the observations I have made may induce some others to examine the physical and chemical properties of this substance, should they be able to procure it, and it is partly for this motive I publish them now.

The form of the crystal I have examined is represented, fig. 2; its planes were sufficiently brilliant to measure the incidences of any two of them. The reasons for which I took the planes  $m, m$ , in preference to the planes marked  $b^{\frac{1}{2}}$ , for the lateral planes of the primary form, are obviously, that in the hypothesis I have made, the planes  $g^3, h^3$ , are the result of simple decrements on the lateral edges of the primary, and that in the other supposition, they would have

Fig. 2.



are the result of simple decrements on the lateral edges of the primary, and that in the other supposition, they would have

been the result of intermediary decrements. For a similar reason relative to the face  $e_s$ , I have determined the ratio between the lateral edge and one side of the base by assuming  $e^1$  to be the result of a decrement by one row on the lateral angles of the base. The cleavage I have mentioned before is parallel to a plane passing by the two small diagonals of the bases. The decrements which produce the other faces are indicated by the signs written upon them; and their incidences upon  $p$  or  $m$  are as follow :

	Incidence on $m$ .		Incidence on $p$ .	
$m$ .....	96°	10'	99°	40'
$b^{\frac{1}{2}}$ .....	140	50	119	30
$b^{\frac{3}{4}}$ .....	106	28	153	52
$e^1$ .....			137	22
$e^2$ .....			155	17
$e_s$ .....	144	51	110	11
$a^1$ .....			127	35
$o^1$ .....			142	29
$h^3$ .....	162	15		
$g^3$ .....	161	2		
$g^1$ .....	131	55	90	0

It will not be, perhaps, useless to remark, that in the oblique rhombic prism I have adopted for the primary form, the line joining the angle  $o$  with its opposite is not perpendicular to the edge  $h$ . A perpendicular drawn from  $o$  upon the edge opposed to  $h$  would cut that line at a distance from  $a$ , a little less than the two-thirds of the length I have assigned to it.

I am, yours, &c.

A. LEVY.

## ARTICLE II.

*Researches into the Mathematical Principles of Chemical Philosophy.* By the Rev. J. B. Emmett.

(Concluded from vol. iii. N. S. p. 433.)

Great Ouseburn, Feb. 22, 1823.

### Sect. II. On the Construction of Liquids.

*Lemma 1.*—When the repulsive force of heat in a solid exceeds the force of cohesion, its particles will be separated from each other.

*Lemma 2.*—If there be two curves having a common axis, and on the same side of it, whose ordinates are inversely as some given powers of their abscissæ; that curve whose ordi-

nates vary inversely as the highest power of the abscissa, will either lie wholly between the other curve and the common axis, or will intersect it in one point.

*Lemma 3.*—If the force of attraction vary inversely as any power of the distance, and there be two equal and parallel planes; when the distance between them is very small, their mutual attraction varies as the same power of the distance inversely, the force belonging to the surface only.

This follows from lib. 1, Prop. 90, Newt. Princip. by placing instead of a corpuscle at P, a plane parallel to D L, and making the distance A P very small.

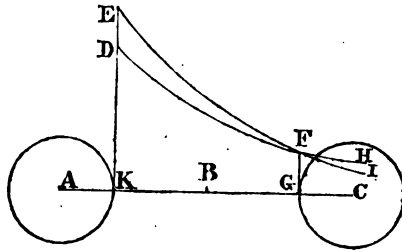
*Cor. 1.*—If the force of such planes be finite at any finite distance, it will be infinite in contact.

*Cor. 2.*—The same applies to spheres of indefinitely small magnitude, the force belonging to the surface only; in contact it will not be infinite.

#### PROP. I.

When the particles of a solid are separated by the force of heat, they remove to such a distance from each other, that the force of attraction at that distance balances that of repulsion, and the body becomes fluid.

Let A C be two equal and similar particles of matter; while they are in contact with each other, the force of cohesion exceeds that of repulsion; let the repulsive force of heat be increased until it overcomes the cohesion; the particles will separate



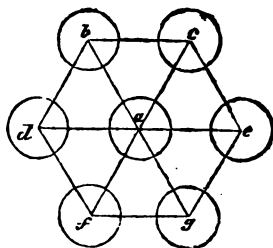
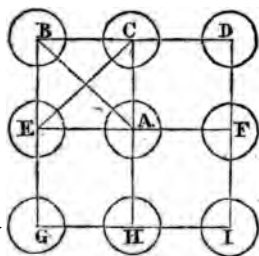
(Lemma 1), but the force of cohesion is indefinitely greater than the attraction of the particles at the least possible distance, and vanishes when contact of the particles ceases; therefore A and C become repulsive. Join A C, and bisect A C in B; describe the curve D F H such that its ordinates are as the forces of attraction at the distances represented by their respective abscissæ; at K erect the perpendicular E K, and D K is the entire force of attraction at K, except cohesion. Take E K to D K, as the force of repulsion in contact is to that of attraction, and describe the curve E F I such, that its ordinates F G are always as the force of repulsion; that is, as the density of the caloric atmospheres at B; this curve approaches A C more rapidly than the former, but E K is by the hypothesis greater than D K; therefore (Lemma 2), the curves will intersect each other, or the forces will be equal at some given point F; let fall the perpendicular F G, and the particles will be in equilibrio at

the distance  $KG$ ; for at  $G$  the ordinates of the two curves are equal; therefore, the opposite forces are equal to each other: also between  $G$  and  $K$  the force is repulsive; beyond  $K$ , it is centripetal; therefore, the temperature being uniform, the distance between the particles can be only  $KG$ , and they are there in equilibrio between two opposite forces; therefore, they have perfect freedom of motion round each other; and a number of such particles will constitute a liquid. Q. E. D.

*Cor.*—Under atmospherical pressure, the distance  $KG$  will be diminished, so that the excess of the force of repulsion above that of attraction shall equal the pressure.

### PROP. II.

To find the order of arrangement of the particles of a liquid.



Let there be a system of detached particles,  $B, C, A, F$ , &c.; join their centres  $BC, CA, AB$ , &c.; arrange them so that the lines  $BC, CA, AB$ , &c. which join the centres of contiguous particles may form squares  $BA, CF$ , &c. By the nature of the figure,  $A$  is surrounded by the greatest possible number of particles under the given arrangement. Suppose the forces acting between  $B$  and  $C$ ,  $C$  and  $A$ ,  $A$  and  $E$ , &c. mutually to balance each other, then  $A$  and  $B$ ,  $C$  and  $E$ , &c. attract each other (Prop. I.); and since  $AB$  is equal to  $CE$ , the arrangement of the system may remain; but if by any disturbing force,  $B$  be brought nearer to  $A$ , since the distances  $BC, CA$ , &c. must remain permanent,  $B$  will continually approach towards  $A$ ;  $E$  and  $C$  must recede from each other, until their mutual attracting forces balance their repulsion; i. e. when  $BA$  is equal to  $CA$ , when also  $CE$  is equal to  $2CA$ ; consequently when the triangles  $BCA, BEA$ , are equilateral. Similarly, since the position of  $C$  is changed, the equilibrium of  $CDFA$  is destroyed, and the particles  $D$  and  $F$  will assume the same order of arrangement; and the same change must take place throughout the whole system. Again: by the same reasoning, if  $B$  and  $A$ ,  $C$  and  $E$ , are in equilibrio,  $B$  and  $C$ ,  $C$  and  $A$ , &c. will be mutually repellent; and finally, if disturbed, assume the same order. Therefore, their arrangement  $a, b, c, d$ , &c. becomes such that the straight lines  $ab, bc, ca$ , &c. joining their centres, form equila-

teral triangles, in which equilibrium is preserved; for since the particles are equal and similar, and  $a, b$ , mutually balance each other,  $b$  and  $c$ ,  $c$  and  $a$ , are in equilibrio; and if  $c$  be brought nearer to  $b$  or  $a$ , it is repelled (Prop. I.); if removed to a greater distance, it is attracted; therefore the triangles  $abc, ace$ , &c. will be equilateral. Q. E. D.

### PROP. III.

Liquids will be expanded by heat and contracted by cold.

It has been demonstrated in a former paper that by increasing the heat, the repulsive force of every calorific atmosphere is increased; and upon the surface of a particle of a liquid, it exceeds the force of attraction. Hence (Lemma 2), the distance of the point in which the forces are in equilibrio is increased; therefore the particles separate. Conversely, diminish the heat, and the particles approach, or, in other words, the volume contracts. Q. E. D.

### PROP. IV.

Solids absorb heat during fusion.

By Prop. 1, the particles separate during fusion; hence the calorific atmospheres are enlarged, and particularly where they are most dense; therefore, caloric will be absorbed. Q. E. D.

Cor. 1.—Hence, during fusion, some bodies will absorb more heat than others.

Cor. 2.—The proportionate quantities absorbed during the fusion of different solids are measured not by equal weights, but by weights which are proportional to the atomic weights.

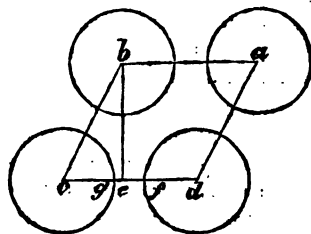
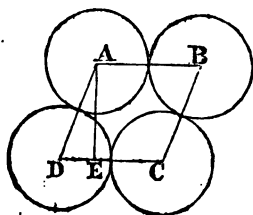
Cor. 3.—Hence solids fuse gradually, and the temperature remains constant during fusion.

Cor. 4.—Hence the temperature of a solid cannot be elevated above a certain point.

Cor. 5.—Hence the reason of the operation of that class of frigorific mixtures which depends upon the solution of crystals of saline matter.

### PROP. V.

Some bodies expand, others contract, during fusion.



Let  $A, B, C, D$ , be four particles of a solid; join their centres,

and from A, let fall the perpendicular A E; let the angle A D C be that which the parallelogram has, when the particles are on the point of separating from each other. When they have separated, as *a, b, c, d*, the angles are 60° and 120° respectively (Prop. II.); let *f g* be the distance to which they separate (the liquid having the same temperature as the fusing solid); from *b* let fall the perpendicular *b e*, which bisects *g f* and *c d*.

The solid A B C D : solid *a b c d* :: D C × A E<sup>3</sup> : d c × b e<sup>3</sup>, or as A D × A E<sup>3</sup> : {A D + *f g*} × b e<sup>3</sup>; but *b e* : b c in a constant ratio; let *b e* =  $\frac{b c}{n}$ ; that is =  $\frac{A D + f g}{n}$ ; therefore, so-

lid A B C D : solid *a b c d* :: A D × A E<sup>3</sup> :  $\frac{\{A D + f g\}^3}{n^3}$ . Now

A D × A E<sup>3</sup> may have any value between A D<sup>3</sup> and  $\frac{3 \cdot A D^2}{4}$ ;

while  $\frac{\{A D + f g\}^3}{n^3}$  is always  $\frac{3 \{A D + f g\}^2}{4}$ , which may be greater

or less than the former according to the value of *f g*; therefore, since these quantities admit of indefinite variation, some bodies will expand, and others contract. Q. E. D.

#### PROP. VI.

Liquids will be attracted by solids.

It has been proved (Prop. I), that beyond a certain distance, the entire force exerted by the particles of a liquid is centripetal; at all distances the centripetal force of the particles of a solid exceeds the repulsive; therefore, the two will attract each other. Q. E. D.

Cor. 1.—If the mutual attraction of the particles of a liquid exceeds their tendency to a solid, a small drop will retain nearly a spherical figure upon the surface of that solid.

Cor. 2.—If the mutual attraction of the particles of the liquid be less than their attraction to the solid, the drop will spread upon its surface.

Cor. 3.—Hence in Cor. 1, a small mass of a solid specifically heavier than a liquid may be made to float upon its surface.

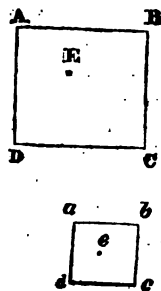
Cor. 4.—If two drops of a liquid, constituted as in Cor. 1, be placed upon a horizontal polished surface, and very near together, they will attract each other and unite.

Cor. 5.—If two liquids be mixed, of which the similar particles attract each other more powerfully than the dissimilar, and which differ in specific gravity, they will separate again.

#### PROP. VII.

If a porous solid be moistened with a liquid, the liquid will cause expansion of the solid; and if equal solids be taken, having unequal pores, the entire expansive forces upon equal surfaces will be inversely as the diameters of the pores; the solid being insoluble.

Let  $A B C D$  be a capillary pore in any solid ; take  $E$  any point within it ; immerse the solid in the liquid ; it attracts those parts of the liquid which are within a certain minute distance from the surface ; these parts tend to enter at  $E$  ; and since the force of attraction is inversely as the square of the distance, and all liquids are incompressible, or indefinitely nearly so, the tendency will be inversely as the distance of  $E$ , and the whole force upon one side of that pore will be as that side directly and its distance from  $E$  inversely. Take  $a b c d$  similar to  $A B C D$ , and  $e$  similarly situated to  $E$  ; the tendency of the liquid to enter at  $E$  : that at  $e$  ::  $a b$  :  $A B$  ; but the area of one side  $A B$  : area of one  $a b$  ::  $A B^2$  :  $a b^2$  ; therefore the whole force upon the side  $A B$  : that upon  $a b$  ::  $A B$  :  $a b$  : now if equal solids be taken, the number of such pores contained under equal sections will be inversely as the square of the homologous lines of those pores ; hence the whole expansive force acting upon equal surfaces will be inversely as the diameters of the pores. Q. E. D.



*Cor. 1.*—In different substances, the force will be as the actual attraction existing between them and the liquid directly, and the magnitude of the pores inversely.

*Cor. 2.*—When an insoluble solid is immersed in a fluid, if the tendency of the liquid to enter its pores be greater than its cohesive force, its parts will be separated.

#### *Scholium.*

By Lemma 2, we may see the reason why corpuscular forces are so great as they are found to be, and why the forces are corpuscular. The demonstration of this and several other parts is omitted for the sake of brevity, and they will subsequently appear as a separate work in an enlarged form. From this the reason is evident why an elastic fluid of so amazingly great rarity as caloric can produce the observed powerful effects ; for distances from the surface of a particle being taken in harmonical progression, the repulsive force of the caloric atmosphere decreases in geometrical progression, and therefore distances may be found, such, that this may equal any finite force. Upon the same principle depends the immense force of water when admitted into the capillary apertures of porous solids.

Other curious phenomena admit of easy solution. From the great tenacity of melted glass, it appears that the utmost limit of expansion is produced before the glass is melted ; that is, when the angles  $A, B, C, D$  (Prop. V.) have become right angles, the particles are yet preserved in contact by a powerful cohesive force. On account of this tenacity and the great rapidity with which it increases as the temperature is reduced, the particles *cannot readily yield to any force which may be impressed upon*

them; if the glass be unannealed, i. e. allowed to cool rapidly, it hardens rapidly, and the particles cannot return to that position in which they would be placed were the cooling very gradual: hence, from a former paper, the particles may have the same position when cold which they had in a fluid state; but if the place of one particle be disturbed, the equilibrium of the whole system will be destroyed, and the mass, however large, will be broken to pieces, by the action of a small piece of any hard matter: hence the peculiar properties of bottles of unannealed glass, Prince Rupert's drops, &c. Also, owing to the great tenacity of glass at the annealing temperature, by that process, the particles can never assume that position in which their attracting forces tend to place them; hence then they will always have a tendency to change, when the situation of a part is altered; the cutting diamond may depend upon this principle.

The mechanical properties of liquids prove that the repulsive force of heat decreases more rapidly than the centripetal force. The parts of a liquid have perfect freedom of motion, therefore the particles cannot touch each other; since they are at a certain distance from each other, which is permanent, the temperature being constant, they must be in equilibrio between two opposite forces, which are equal at that distance. If the force of repulsion vary less rapidly than the other, there may be a distance at which the two forces are equal; but if two particles under such circumstances be disturbed, the equilibrium is destroyed, and, if brought nearer, they will come into contact; if removed to a greater distance, they will separate indefinitely, which is contrary to the fact: if the force of repulsion vary most rapidly, both forces, Prop. I. will tend to that point, so that the particles can be only at that distance from each other. Again: if the repulsive force vary less rapidly than the other, increase of heat would cause contraction, and *vice versa*, which is absurd.

Cor. 2, Prop. VII. shows why dry unbaked clay, white lead, prussian blue, and many other solids, insoluble in water, are reduced to a paste when immersed in it; these substances are well known to be sensibly porous, and it is evident that the force with which the water tends to enter into and expand the pores, exceeds the cohesive force of those solids.

The tenacity of moist clay results from a similar cause. Clay also will contract permanently by the application of heat. By the first application of heat, the water contained in the clay will be expelled: it is then filled with minute pores; if the temperature be increased, the force of cohesion is considerably diminished, and the sides of the pores will have a tendency to approach each other; consequently the volume will contract, and the contraction will be permanent.

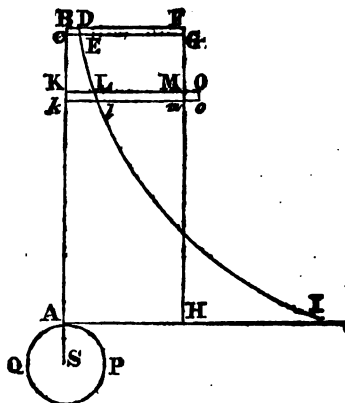
## Sect. III. On Gaseous Bodies.

## Lemma.

In distances from the particles of bodies, such, that the increase of density of the surrounding caloric is very small compared with its entire density. If the force of attraction vary inversely as the  $n$ th power of the distance, and the elastic force of caloric be as its density, the repulsive force of the caloric atmosphere will be inversely as the  $n - 1$ th power of the distance.

Let  $APQ$  be a particle of matter; produce the radius  $AS$  indefinitely; describe the curve  $DLI$ , such that its ordinates  $BD, KL, \&c.$  perpendicular to  $BS$ , shall be proportional to the force of attraction at  $B, K, \&c.$  Let this particle be immersed in an uniformly diffused elastic medium, whose density is  $BF$ ; at remote distances  $A, K$ , the pressure on  $K$ : pressure on  $k$ :  $S k$ :  $S K$ .

Through the point  $F$  draw  $FH$  parallel to  $BS$ , and if the medium were not attracted by  $APQ$ , its density at all points  $A, K, B, \&c.$  would be equal to  $BF$ . Take  $C$  indefinitely near to  $B$ ; the specific gravity of the elastic medium at  $B$  is as  $BD \times BF$ ; therefore its pressure at  $C$  is as  $BD \times BF \times BC$ ; if this pressure be indefinitely less than the whole elastic force of the fluid, its density will be increased by this pressure, by a quantity which is indefinitely small, compared with the whole density, or the fluid may be considered as incompressible by the force acting upon it. At the point  $K$ , let the density  $KM$  be increased by the superincumbent pressure, by the quantity  $MO$ , and let  $MO$  be very small compared with  $KM$ ; through  $k$  draw  $ko$  parallel to  $KO$ , and  $Oo$  parallel to  $Kk$ ; the evanescent area  $Km$  will be less than the evanescent area  $Ko$  by the indefinitely small area  $Mo$ ; therefore the pressure  $KL \times KM \times Kk$  is less than the pressure  $KL \times KO \times Kk$  by the quantity  $KL \times MO \times Kk$ , which is indefinitely small when compared with either; therefore the pressure at  $k$  will be indefinitely nearly equal to the sum of all the several pressures  $KL \times Kk \times BF, CE \times BC \times BF, \&c.$ ; that is, as the area  $kBDl \times BF$ . Let the force of attraction be as  $\frac{1}{AB^n}$ , and the



lines LD, SB, be produced infinitely; the area  $kBDl$  is as  $\frac{1}{A K^2 - 1}$ . Q. E. D.

*Cor. 1.*—If the force of attraction be inversely as the square of the distance, the force is as  $\frac{BF}{AK}$ .

*Cor. 2.*—The greater AK is taken, the nearer will this approximate to the truth.

*Cor. 3.*—If at equal distances the forces of different particles be different, the areas BDKL at those distances will be as those forces.

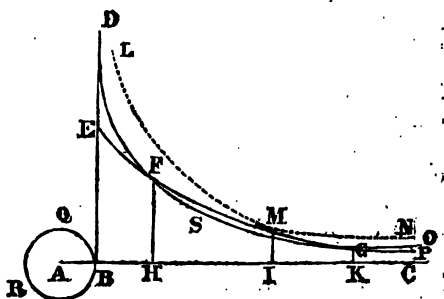
*Cor. 4.*—If the density of the medium vary, the area BDKL will be as that density.

*Cor. 5.*—If different particles be placed in similarly constituted media of different densities, the areas BDKL at equal distances will be as their forces of attraction and the densities of the media.

### PROP. I.

When a liquid has attained a certain temperature, its particles become mutually repulsive, and it becomes gaseous.

Let QRB be a particle of matter, A its centre; draw the right line AC; and at B draw the tangent BD. Describe the curve E M P, such, that its ordinates FH, MI, &c. may be proportional to the force of attraction at the distances BH, BI, &c. In liquids, the force of



repulsion exceeds that of attraction on the surface (Sect. 2, Prop. I). Take BD to represent the force of repulsion, and let the curve D S G O be such that its ordinates are as the forces of repulsion at the half distance of their abscissæ. Within a certain distance of B, the curve D S G O will approach the axis more rapidly than E F M, and will, therefore, intersect it in some point F (Lemma 3, Sect. 2); therefore at H, the forces will balance each other. Beyond a certain distance, the curve D S G O will approach the axis less rapidly than D F M (Sect 3, Lemma), and will, therefore, intersect it in some other point G. Increase the heat, and the points H and K will continually approach each other, until, at a certain temperature, they meet in some point I; draw the perpendicular IM, and the curve D S G O, i. e. at a higher temperature LMN is a tangent to the curve E F M P at M; hence between B and I the force

is wholly repulsive; at I evanescent; beyond I repulsive *in infinitum*. If the heat be increased by the smallest possible quantity, the particles must separate until the force of repulsion is balanced by some external force. Q. E. D.

**Cor. 1.**—Under the pressure of the atmosphere or any other external compression, a liquid will become gaseous at a higher temperature than in vacuo.

**Cor. 2.**—The greater this pressure is, the higher will be the temperature required.

**Cor. 3.**—Some bodies may become gaseous, without previously entering into a state of fusion; for H and K may coincide before cohesion is destroyed.

**Cor. 4.**—Such bodies may be fused under a strong pressure.

**Cor. 5.**—A liquid boils in vacuo, when H and K coincide.

**Cor. 6.**—Under pressure, at the point M, the boiling point is attained when the repulsive force exceeds the centripetal by a quantity equal to the compressing force.

**Cor. 7.**—The temperature of a boiling liquid is the same as that of the disengaged vapour.

**PROP. II.**

**Caloric is absorbed during evaporation.**

From the ratio between the specific gravities of the same body in a solid or liquid, and in a gaseous state, it is evident that in the latter, the particles are separated to a great distance from each other; therefore their calorific atmospheres are enlarged, and heat is absorbed. Q. E. D.

**Cor. 1.**—The specific heat of any body is greater when it is elastic, than when in a solid or liquid state.

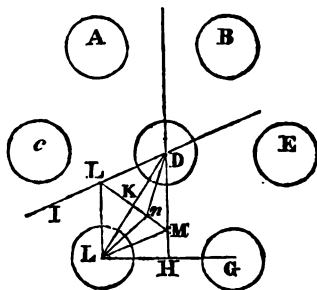
**Cor. 2.**—Hence the action of frigorific processes which depend upon evaporation.

**Cor. 3.**—Heat will be evolved when a gas is resolved into a liquid or solid state.

### PROP. III.

**At a given temperature, the elastic force of a gas will be nearly inversely as its volume.**

By Lemma 1, Sect. 3, the elastic force of a particle of a gas is inversely as the distance from its surface; therefore, neglecting the force with which the particles attract each other, the elastic force of a gas will be inversely as its volume; for let A, B, C, D, &c. be a number of particles of a gas, they are equal and at all equal distances, their forces are equal, by the hypothesis. Bisect the



Since  $F G$  in  $H$ , and join  $D H$ ; then, since  $F H$  is equal to

H G, the elastic force of the atmosphere of F is equal to that of G in the point H. Bisect D F in K, and through K draw L M at right angles to it, which will cut D I, D H, in L and M; join L F, F M; then, since D K, K F, are equal, the atmospheres of D and F are equally elastic at that point; because D L is equal to F L, they are equally dense at L; also at M; or if any other point *n* be taken in L M, and D *n*, F *n* be joined, the forces will be equal at that point; and if the solid figure be taken, the elastic forces of the atmospheres of D and F will be equal in every point of the hexagonal plane passing through K, to which D F is perpendicular; therefore, the entire repulsion between those particles takes place in that plane; and the same may be proved of all the other particles; therefore, since the angles L F K, K F M, are constant, as the density varies, all the lines L F, F K, L D, &c. have to each other a constant ratio; and by Sect. 3, Lemma, density at K : that at M in a constant ratio, and since the elastic force at K is inversely as K F, that at L is inversely as K F, and the same may be proved of the entire repulsive force; therefore, the entire repulsive force between the two particles D and F will be inversely as D F; therefore, by Newton's Princip. lib. 2, Prop. 23, the elastic force of the gas will be inversely as the volume. Q. E. D.

*Cor. 1.*—If the compression be such that the force of attraction between the particles produce any sensible effect, having given the ratio of the forces at any given distance, the force will be as  $\frac{1}{D} - \frac{1}{D^2}$ , each term being multiplied by the force at that given distance.

*Cor. 2.*—If the gas be highly compressed, its elastic force will increase in a higher inverse ratio than that of its volume.

#### *Scholium.*

If the force of attraction vary inversely as the cube of the distance, the cube of the compressing force will be as the fourth power of the density. If the force of attraction be as the fourth power of the distance inversely, the cube of the compressing force will be as the fifth power of the density.

#### PROP. IV.

Gases will be expanded by heat, and contracted by cold, their elastic force being constant.

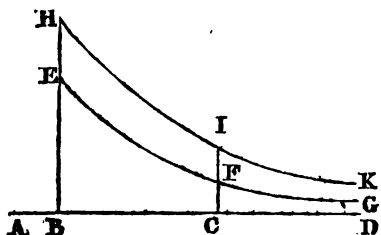
Let the elastic force of a confined portion of a gas be balanced by the pressure of the air; heat it, and the elastic force of the caloric atmospheres of its particles will be increased; therefore, the particles will have a tendency to separate from each other; the gas will expand; but the force of each particle is inversely as the distance; therefore, a distance will be found, such, that the gas will again be in equilibrio with the external air, and con-

sequently the expansion will proceed until this distance is attained. The converse will take place by cooling. Q. E. D.

#### PROP. V.

The elastic force being constant, the change, produced in equal volumes of different gases by the same variation of temperature, will be equal.

Let  $AD$  be the radius of a particle of a gas produced; let  $EFG$  be the curve whose ordinates represent the force of attraction at the distances of their respective abscissæ; these multiplied into the density of the surrounding caloric give the density of the caloric at those points; therefore the pressure at  $B$  is as the area  $EFGD$ . Let the density of the surrounding caloric be increased, and be to its former density as  $n : 1$ ; represent this by the curve  $HIK$ ; then the area  $BEGD : \text{area } BHKD :: 1 : n$  and area  $BHKD : \text{area } CIKD :: AC : AB$ ; therefore, the area  $BEGD : \text{area } CIKD :: AC : nAB$ .



Let the area  $BEGD$  be equal to the area  $CIKD$ ; then  $AC = n \cdot AB$  or  $AC : AB :: n : 1$ ; or the distance between the particles will be as the change of temperature, force of attraction being neglected. Q. E. D.

*Cor.*—If  $D$  be the density of caloric, or true temperature from zero, the volume of a gas will be as  $D^3$ , when the particles are so distant that the compression of the caloric bears an insensible ratio to its whole density.

#### Scholium.

Hence may be established a remarkable difference between gases and vapours; in the former great elastic force remains at a temperature at which the latter become liquid or solid; therefore in gases the deviation from the laws of expansion will be much less than in vapours; in the latter, there will be variations, hereafter to be investigated, which will be the greatest in the least volatile matters.

#### PROP. VI.

Heat will be evolved by the sudden compression of a gas or vapour; for by compression the caloric atmospheres of the particles are diminished; therefore, part of the heat which was latent becomes sensible. Q. E. D.

*Cor.*—Cold results from the sudden rarefaction of gaseous matter.

## PROP. VII.

Gases are attracted by solids or liquids, whether they do or do not exert on them any chemical action.

From the nature of attraction, the particles of a gas when indefinitely near the surface of solid or liquid matter, will tend to it; therefore upon the surface of every substance which is surrounded by any gas or vapour, there will be an atmosphere of that gas or vapour, of very small extent, whose density is greater than that of the surrounding medium, whose increased elastic force is balanced by that of the surrounding medium, and its tendency to the body. Q. E. D.

*Cor. 1.*—Hence bodies which are filled with minute pores will absorb large quantities of gaseous matter.

*Cor. 2.*—Hence liquids will contain air for which they may have no chemical attraction.

*Cor. 3.*—The rays of light will be refracted and dispersed by passing near the surface of any solid; this will be distinct from inflection.

*Cor. 4.*—When a dry porous body, freed from air, is brought into any gas or vapour, its temperature will be increased.

*Cor. 5.*—All bodies may be more or less perfectly freed from air by being heated, or placed under an exhausted receiver.

*Scholium.*

Prop. 6 and 7 explain some curious phenomena. Charcoal and other very dry porous substances absorb a large quantity of any gas or vapour in which they are immersed, the volume of gas absorbed being many times greater than that of the solid; hence very dry impalpable powders cannot be readily mixed with liquids. Hence also during the absorption of a gas by a porous solid, heat will be evolved, and upon this principle, the common pyrophorus seems to act.

*General Remarks upon Chemical Attraction.*

Chemical attraction is distinguished from that which produces mere mechanical effects, by many remarkable differences in the properties of the resulting compounds.

Introduce an inflamed stream of hydrogen into a vessel of oxygen gas; both gases will rapidly be condensed, and the only product will be water. In this experiment, the gases enter into chemical union, i. e. into that state of combination in which the attraction subsisting between the particles of the oxygen and hydrogen gases, overcomes their elastic force, and is greater than the force exerted by like particles upon each other; hence the water may be congealed, reduced to a state of vapour, or enter into many combinations, without any decomposition taking place. The sensible qualities of the gases are totally

changed; oxygen gas is a powerful supporter of combustion; hydrogen gas is highly inflammable; their compound is equally destitute of either of these properties; water may be congealed, but at the lowest temperature hitherto produced, each element retains its gaseous state.

Into a vessel of carbonic acid gas introduce a stream of ammoniacal gas; the gases will ultimately disappear, and a white crystallized solid will result, which retains the properties of the ammonia in a mild form, but none of those which distinguish the acid.

If dry nitrous gas and oxygen gas be mixed in proper proportions, the volume is reduced to one half; the gases, which were invisible, assume a deep-orange colour; each was destitute of acid properties; the compound is a powerful acid.

By combination, solids sometimes become liquid or gaseous. Mix together a solid amalgam of zinc with one of bismuth; the mixture becomes fluid. Burn a piece of charcoal in a confined portion of oxygen gas; the charcoal disappears, and carbonic acid gas is produced: the charcoal, therefore, by combination assumes a gaseous state. Similarly, mix dry muriate of lime and snow at a very low temperature; they rapidly become liquid.

The colours of bodies are often changed; dissolve peroxide of iron in sulphuric acid; into one portion drop solution of prussiate of potash, an intense blue precipitate is formed; into another portion pour solution of hydrosulphuret of potash, the precipitate is black; into a third pour subcarbonate of potash, and a yellowish-red precipitate is obtained; with arsenite of potash, a dull yellow.

The forces which produce chemical changes often act with extreme violence; place a thin slice of phosphorus upon a grain or two of hyperoxymuriate of potash; give it a gentle blow with a hammer, and a most violent detonation will follow.

During combination, a change of temperature takes place. Mix four parts of dry lime with one of water; the water loses its fluidity, for the lime remains dry, and much heat is excited. Introduce phosphorus, pulverized antimony, or bismuth into chlorine gas; the gas will be absorbed, and brilliant combustion ensues; and in all cases, if the volume be reduced, heat is excited; if increased, or if solids become liquid, or liquids, gaseous, cold is produced. Again, bodies which are susceptible of chemical union, combine only in certain definite proportions; thus in water, the weight of oxygen has to that of the hydrogen a constant ratio. If bodies can combine in two, three, or more different proportions, the ratio of the quantities of one of the elements to a given weight of the other will be as the numbers, 1, 2, 3, &c. The force of chemical attraction is not the same in all bodies of the same class, as is manifest from the various cases of simple and compound decomposition. These, and all other chemical changes

are the result of the operation of the two great powers, attraction and the repulsive force of caloric. In the present state of chemical science, from the want of some important data, we cannot estimate the effect of these forces, nor compare the relative forces of chemical attraction, which is very different to the order of decomposition; for it is affected and even inverted by the following causes, the results of the operation of the primary forces upon similar particles.

1. *Temperature*.—Heat mercury to nearly its boiling point during several days, giving it access of air; it will be converted into a red oxide; heat this nearly to ignition, the mercury reassumes its metallic state, and oxygen gas is evolved. At the usual temperature of the air, the attraction of the particles of mercury for each other, and the elasticity of the oxygen gas, prevent their union: at a certain temperature, the former force is so far diminished, that they combine: at a temperature approaching to ignition, the mercury becomes elastic, and the elasticity of the oxygen is amazingly increased, and they are again separated: similarly, at the ordinary temperature of the air, sulphuric or muriatic acid will separate the boracic from its combination with alkaline or earthy bases; the contrary takes place at a red heat, evidently because the sulphuric and muriatic acids, from their tendency to become elastic, are united to the bases by a less force than at a lower temperature. Many other cases might be advanced to prove that the apparent attractions of bodies for each other are changed by variations of temperature, which acts in two ways; first, when it favours chemical union, it diminishes the cohesive force of one or more substances; secondly, when it effects decomposition, it is by giving to one element at least a tendency to become elastic. Hence tables of chemical attraction serve for only one temperature.

2. *Elasticity*.—This might have been considered solely as a result of the last cause, but it acts in various peculiar ways at the ordinary temperature, and on this account deserves a separate place. Carbonic acid is elastic at all temperatures, and probably has a greater tendency to a gaseous state than any other. Its power of saturation is very great, and the force with which it adheres to many bases is peculiarly powerful, yet it is separated from its combinations by every other acid, doubtless, in part at least, from its great tendency to the elastic state at all temperatures. And, in general, bodies which are volatile are separated from their compounds more easily than those which are fixed. Hence carbonic acid will not unite with dry lime, but with its hydrate; and phosphorous will not shine in pure oxygen gas.

3. *Cohesion* promotes combination when the body to be formed has a great cohesive force, and those to be separated are elastic, fluid or soluble, in the menstruum employed. It opposes it when the substance used for decomposition has a cohesive

force greater than the other assisting forces. For example, in solution, potash and soda separate all the metallic oxides from most acids, because, by reason of cohesion, the oxides are insoluble. Barytes separates potash and soda from most of their salts, and nearly all the resulting compounds of barytes are insoluble. Lime separates them from those acids with which it forms insoluble compounds, but in no other cases. Although the force does not become actual cohesion, until the compound is formed, we here see the operation of the force which ultimately produces cohesion, while the particles are yet separated. These remarks need not be further extended, since they are obvious to every reader.

Electricity appears to be most intimately connected with this subject. In all cases of electrical excitation, the opposite states are always exhibited in such proportion as will mutually neutralize each other, whether excited in the ordinary manner, or by chemical agency, as in the galvanic series. If compound bodies, as neutral salts, be exposed to the operation of the galvanic battery, the principles are separated from each other; oxygen, chlorine, and acids appear at the positive pole, while hydrogen, azote, and inflammable matter, together with earths, alkalis, and metallic oxides, tend to the negative; and those bodies which unite with the greatest force differ most in their natural electric energy. Electricity is excited by the contact of dissimilar bodies, when the same relation is observed, with this peculiar difference, that bodies of the same class have different degrees of electric energy, whereby, after contact, the two electricities are exhibited. Whether electricity be the cause of chemical combination or a counteracting force, cannot yet be determined; and for the purposes of research, it is not material that this should be decided. However, until the several electric energies of bodies be determined, as well as the relation of the electric energy to caloric, to the atomic weight, specific gravity, &c. nothing can be determined with certainty. I therefore withhold further researches into the nature, &c. of chemical combination for the present, except that all tables of capacities of bodies for heat, expansion, &c. should be computed for weights proportional to the atomic weights, for then they represent the capacities, &c. of single atoms; this is most conspicuous in cases of combustion. Since heat is evolved during that process, there must be a diminution of capacity; if, therefore, we add the atomic capacities of the bodies, and from the sum subtract the atomic capacity of the product (that is, the capacity of a weight of the product, equal to the sum of the weights of the ingredients consumed), the remainder will be the proportion of the diminution of capacity, or of the heat evolved; for example, the atomic capacity of hydrogen gas is 2.82; that of oxygen is 4.75; the sum is 7.57; the capacity of an atom of aqueous vapour is 1.75; therefore, 5.82 is the heat evolved.

Similarly, the atomic capacity of carbon is 1.95; twice the atomic capacity of oxygen is 9.5; the sum 9.695; the atomic capacity of carbonic acid is 2.887; i. e. 6.808 is the quantity of caloric evolved. Until we are furnished with the above data, and particularly with the precise relation of electricity to chemical attraction and caloric, it will be vain to attempt to institute any researches into the principles of chemical philosophy; and, in all computations, tables must be constructed for the atomic weights of bodies in every case. For the table of atomic capacities, the argument is, atomic weight  $\times$  capacities of equal weights.

Heat evolved by combustion or combination: argument, sum of the atomic capacities of the bodies combined — atomic capacity of the compound.

Atomic expansion in volume: argument,  $\frac{\text{atomic weight}}{\text{specific gravity}}$ ; in length, the cube root of the above quantity.

Conducting powers: argument,  $\sqrt{\frac{\text{atomic weight}}{\text{specific gravity}}}$  for the lengths of the metallic rods on which the experiment is to be made, in order to obtain the true ratio of the conducting powers.

### ARTICLE III.

*Abstract of a Memoir entitled "An Attempt to ascertain the Chemical Composition of those Minerals which possess the same Crystalline Form as Pyroxene."* By H. Rose.

(Concluded from p. 231.)

#### 4. *Pyroxenes, with Lime and Protoxide of Manganese as Bases.*

The red silicate of manganese from Langbanshyttan, in Wermeland, Sweden.

This mineral (manganèse oxidé silicifère rouge de M. Haüy) occurs generally only massive on the Hartz, Siberia, Transylvania, and near Langbanshyttan, with a distinct foliated texture identical with pyroxene, which is further proved by an analysis of Prof. Berzelius, according to whom it is a bisilicate of protoxide of manganese combined with some bisilicate of lime.

The result of his analysis was:

Silica. ....	48.00	containing oxygen	23.80
Protoxide of manganese	49.04		10.76
Lime. ....	3.12		0.87
Magnesia. ....	0.22		
Oxide of iron. ....	Trace		

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100.38

It is, therefore, a bisilicate of protoxide of manganese combined with a small quantity of bisilicate of lime.

Besides the pyroxenes of these four divisions, there exist in nature also some which contain alumina as a real constituent part. They are rare, and the quantity of alumina which occurs in them never amounts to more than seven per cent. The alumina, which contains three atoms of oxygen, is not isomorphous with the bases containing two atoms of oxygen, and its presence in the pyroxenes is still enigmatical. It seems that in proportion as the quantity of alumina increases, that of the silica decreases, so that it is not improbable that it does not exist as a silicate in the pyroxenes, but as an aluminate, of which the electronegative constituent part may be isomorphous with silica. Such pyroxenes which contain alumina are the black pyroxene from Frascati analysed by Klaproth;\* the pyroxene from *Ætna* analysed by Vauquelin, and the black pyroxene from *Pargas* analysed by Nordenskiöld.

The greater part of the pyroxenes analysed by M. Rose were fragments taken from massive pieces, with a very distinct foliated fracture. In this state, the minerals contain less foreign admixtures than when they are crystallized. In this case, the crystals are generally large, and have distinct faces; but between the laminae very distinct admixtures may be discovered, and so many traces of the matrix that they may be seen with the unassisted eye. In the beautifully crystallized pyroxene from Frascati, the naked eye discovers many such foreign admixtures, while even with a glass none can be found in the massive white malacolite from Orriervi.

The pyroxenes analysed by M. Rose were all measured by Prof. Mitscherlich, and found to be real pyroxenes. The inclinations of the two sides, and of the terminal face, approached to the numbers given by Haiiy, and agreed completely with the measurement of Phillips. These cleavages occurred on the malacolite from Orriervi, the yellow variety from Langbanshyttan, though they were not so clear in the hedenbergite, in both malacolites from Bjornyreodden, or the hard sahlite.

The pyroxene from Taberg is the only one of those analysed by M. Rose, which is well crystallized. The red silicate of manganese from Langbanshyttan shows only the cleavage parallel to the sides, and the truncations of the acute and obtuse solid angles. The inclination of the sides is the same as that mentioned by Haiiy as belonging to

\* Klaproth found alumina in all pyroxenes, of which he has published an account in the collection of his analyses; however, among them all, the pyroxene from Frascati only is a real pyroxene; the augit from the Sanalpe is hornblende; the slaggy augit from Sicilia which is not crystallized is, according to Prof. Weiss, an obsidian; the common augits from the Rhon mountains are not crystallized, and no trace of foliated fracture can be discerned in them.

pyroxene. The cleavage is very indistinct in the soft sahlites which contain serpentine. Their external similarity only with the hard sahlite, from Sahla, was the reason which induced M. Rose to analyse them. It was necessary to repeat this analysis often as it gave questionable results, until at last no doubt could remain about their true chemical nature. The cleavages parallel to the sides of the prisms and its truncations only were observable, and very indistinct; the terminal face could not at all be discerned. This imperfect crystalline structure depended evidently upon the foreign admixture of serpentine.

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#### ARTICLE IV.

*Some Experiments and Researches on the Saline Contents of Seawater, undertaken with a View to correct and improve its Chemical Analysis.* By Alexander Marcet, MD. FRS. Honorary Professor of Chemistry at Geneva.\*

IN a paper on the temperature and saltiness of various seas, which the Royal Society did me the honour to publish in their Transactions for the year 1819, I threw out a conjecture, that the sea might contain minute quantities of every substance in nature, which is soluble in water. For the ocean having communication with every part of the earth through the rivers, all of which ultimately pour their waters into it; and soluble substances, even such as are theoretically incompatible with each other, being almost in every instance capable of co-existing in solution, provided the quantities be very minute, I could see no reason why the ocean should not be a general receptacle of all bodies which can be held in solution. And although it will appear from the following account, that I have been unsuccessful in some of my attempts to prove the truth of this conjecture, it may fairly be ascribed either to a want of sufficient accuracy in our present methods of chemical analysis, or of the requisite degree of skill in the operator.

Some time after the communication to which I have just referred, an extraordinary statement was pointed out to me, upon the authority of Rouelle, a French chemist of the last century, from which it appeared that mercury was contained in sea salt;† and I saw soon after in the *Annales du Musée*, vol. vii. a paper by the celebrated chemist Proust, who, in a great measure,

\* From the Philosophical Transactions for 1822. Part II.

† See *Journal de Médecine*, vol. xlviii. 1777, page 327.

confirmed that statement, by announcing that he had found traces of mercury in all the specimens of marine acid which he had examined.

Improbable as the fact appeared, I thought it worth while to repeat the experiment, and to take that opportunity of making some collateral researches upon other substances, much more likely than mercury to be discovered in sea water.

For this purpose I availed myself of the kindness of my friend Mr. John Barry,\* who happened to be in the vicinity of Portsmouth, to supply me with specimens of sea-water, carefully concentrated upon the spot, in vessels of Wedgwood ware, and with scrupulous attention to cleanliness in the process. Accordingly he was so obliging, as not only to send me a quantity of brine evaporated under his own eye, in the manner just mentioned, but he also collected for me a valuable series of specimens from the salt works near Portsmouth, from all the stages of the process, so as to afford me an opportunity of investigating with accuracy all the chemical circumstances of this interesting branch of national economy. Finding myself, however, much pressed by time at this late period of the session, I shall, after briefly adverting to Rouelle's supposed discovery, confine myself in this communication to a few observations which I have made on sea-water itself; keeping out of view, for the present, the topic of salt-making, which, however, I intend to resume at some future period, in a more complete and satisfactory manner.

I first attempted to detect mercury in a specimen of *bay-salt*, such as is obtained in the salt-works near Portsmouth, by spontaneous evaporation. This variety of salt forms large crystals, but is always more or less contaminated by earthy matter, which gives it a dirty appearance. It has, probably, a general resemblance to the French *Sel de Gabelle*, which is more impure still, though, I believe, obtained in a similar manner.†

Eight ounces of this salt were put into a coated retort connected with a receiver, and about four ounces of nitrous acid were poured upon it. A pretty brisk action took place, which was further increased by the application of heat; fumes of chlorine were immediately disengaged, and a reddish fluid condensed in the receiver; the heat was continued, and gradually raised in a charcoal fire till no acid or moisture any longer came over; at which time a new emission of red fumes indicated that the nitrate formed in the retort was beginning to part with its acid: minute drops of fused salt soon bedewed the upper part and neck of the retort, so as to be mistaken at first for a sublimate. This, however, proved to be almost solely muriate of soda; and

\* Mr. John Barry, of Plough Court, inventor of a new and valuable process for preparing extracts in vacuo, &c.

† The name of *bay-salt* is often applied to foreign as well as British salt, and in general it simply denotes that the salt has been obtained by spontaneous evaporation.

on careful examination, it did not appear to contain the smallest atom of corrosive sublimate.

I next dissolved five or six pounds of bay-salt in water, and collected in a filter the insoluble earthy sediment, in which Rouelle stated that the quicksilver was usually found. This sediment being carefully dried, and heated to redness in a coated retort, a white sublimate arose, and condensed on the neck of the retort; but this sublimate proved to be muriate of ammonia, and did not contain the smallest portion of corrosive sublimate, or other mercurial salt. This sal-ammoniac, though evidently formed during the distillation from the vegetable and animal matter contained in the sediment, suggested to me the idea of looking for ammonia amongst the contents of sea-water.

I now submitted some Sel de Gabelle, which I had procured from Calais for the purpose, to similar experiments, and the sediment, also, was carefully examined. The result was essentially the same as with the bay-salt. After adding nitric acid to the salt, the heat was gradually pushed to redness; and when all the moisture was evaporated, a white sublimate appeared, as in the former case, which, in this instance, proved to consist almost entirely of nitrate of soda; but always without the least particle of mercurial salt, and without any muriate of ammonia.\*

I therefore think myself justified in concluding that the mercury, which other chemists have detected in sea-salt or its products, must have been introduced there from some local or accidental circumstances.

In experiments upon sea-salt, or in general upon the saline contents of the sea, it is obvious that, in order to exclude sources of error, it is necessary to operate upon pure sea-water, and not upon salts obtained from it by the usual processes in the large way, these being always more or less contaminated by the clay pits in which the evaporation is carried on, by the metallic boilers, or other adventitious causes. I, therefore, now turned my attention to the sea-water itself, and in particular, the perfectly pure and transparent specimen of concentrated brine from the Channel, which I have above mentioned. Mr. Barry procured this water near Bembridge floating light, about two miles north-east of the eastern extremity of the Isle of Wight, and the evaporation which it had undergone at Portsmouth, had only separated from it a quantity of calcareous matter, principally selenite.†

\* In the former experiment the sublimate was principally muriate of soda, owing, no doubt, to the decomposition having been less complete, and the operation less gradually conducted than in the latter experiment.

† The water, immediately on being raised from the sea, had been allowed to stand a sufficient time to deposit the earthy particles suspended in it, by which means it had become beautifully transparent: 100 pounds of the water produced only three grains of earthy sediment, in which I could discover nothing but carbonate of lime and oxide of iron. It is in this sediment, according to Rouelle, that mercury is to be found. I need hardly say that I could not detect in it the least particle of that metal.

A few pounds of this water were evaporated nearly to dryness, at a gentle heat, so as to reduce the mother liquor to the smallest possible quantity. This liquor was suffered to drain off, and reserved for experiments, as it is in this fluid that any new ingredients are most likely to be detected.

I had suspected that some nitric salt might be found in sea-water, but in this I was disappointed. The discrimination by the shape of the crystals being in this instance scarcely practicable, the mode which I employed for detecting it, consisted in concentrating the bittern in a glass tube or retort, till it began to deposit solid matter, then adding sulphuric acid and gold-leaf, and boiling the mixture; the gold-leaf was not in the least acted upon, nor was any smell of nitric acid perceived; but on adding the smallest quantity of nitre to the same mixture, the gold was dissolved, and the smell of aqua regia was instantly perceived.\*

A portion of the said bittern was next examined by appropriate reagents, with a view to detect any minute quantity of earths or metals, especially alumina, silica, iron, and copper, which former inquirers might have overlooked; but I could find no other earth except magnesia: and to my surprise, I did not find in the bittern the least particle of lime; which proves that sea-water contains no muriate of lime, as had been generally supposed. I was equally unsuccessful in my attempts to detect iron or copper, by the most delicate tests. In fact, neither alkalies nor alkaline carbonates precipitated any other substance from the bittern of sea-water, except magnesia.

The deposit obtained at Portsmouth during the early period of the concentration of the water, being analyzed, I found it to consist of selenite, mixed with a little muriate of soda, and a portion of carbonate of lime. The presence of this last substance in sea-water, in a state of perfect solution, being, I believe, a new fact, I neglected no means of establishing it with certainty, an object which was accomplished without difficulty.†

Carbonate of magnesia having been supposed by some chemists to exist in sea-water, I looked for it in the same deposit; but I could not detect the least portion of it by the most delicate tests.‡

I next turned my attention to the alkaline salts of sea-water; and here I was more fortunate; as I succeeded in ascertaining beyond a doubt, that sea-water contains ammonia, as it yielded sal ammoniac by evaporation and sublimation. This result was easily obtained. Some of the bittern being evaporated to dryness in a retort, and a low red heat applied, a white sublimate

\* For this easy and elegant process for detecting nitric acid, a point attended with difficulty, I am indebted to Dr. Wollaston.

† The deposit was treated with acetic acid, which occasioned an effervescence. The clear liquor being then decanted off, and evaporated to dryness, and alcohol added, acetate of lime was found in the filtered alcoholic liquor.

‡ Namely, solution of the mass in dilute muriatic acid; precipitation of the lime, and addition of phosphate of ammonia to the filtered liquor.

appeared in the neck of the retort, which proved to be muriate of ammonia. The mode in which this substance was identified was as follows :

1. The sublimate was re-dissolved in water, re-evaporated to dryness, and again sublimed by the heat of a spirit lamp.

2. This new sublimate being again dissolved, and solution of magnesia and phosphoric acid added, a triple phosphate was formed.

3. On adding caustic potash to the solution, and bringing the mouth of a phial containing muriatic acid close to the vessel, abundant white fumes appeared.

4. The sublimate gave precipitates both with muriate of platina and nitrate of silver.\*

Sulphate of soda having been mentioned by many chemists as one of the constituents of sea-water, I endeavoured to ascertain, whether or not it existed in it. But all attempts to detect this salt in the bittern by crystallization were fruitless, though great pains were taken for that purpose; and I feel the more confident that there is no sulphate of soda in sea-water, as the presence of this salt in any but the most minute quantities would be quite incompatible with our knowledge of chemical affinities. For since there are co-existing in sea-water muriate of soda and sulphate of magnesia, it is evident that sulphate of soda would decompose muriate of magnesia, which salt is known to be in sea-water. And again we know, that sea-water contains sulphate of lime and muriate of soda; therefore it cannot contain sulphate of soda; for in that case we should have muriate of lime, which I have shown to be contrary to fact.

The last circumstance which I shall at present notice relates to the state in which potash exists in sea-water.† Potash is found, by its appropriate re-agents, principally in the bittern, but it is found also among the salts which are separated from it, especially in the latter period of crystallization. By further and repeated evaporation of the bittern, and successive separation of the mother water remaining after the removal of the crystals formed, various distinct crystals were obtained possessing their characteristic form; namely, prismatic sulphate of magnesia, cubic and star-shaped muriate of soda, and rhombic crystals, quite different from either of the other salts. These crystals, which were evidently portions of an oblique rhombic prism, being carefully separated and washed with water and alcohol,

\* As it did not enter into my plan, on this occasion, to turn my attention to the estimation of proportions or precise quantities, I have not attempted to estimate exactly the proportion which ammonia bears to the other ingredients of sea-water; but as its presence can easily be shown in 100 grains of the bitter salts, its quantity cannot be extremely minute.

† It will be recollected that the presence of potash in sea-water, though announced by myself in the paper on sea-water to which I have before alluded, was Dr. Wollaston's discovery. I have likewise to mention, that the above experiments respecting the state in which it exists, were either made by him or at his suggestion.



ARTICLE VI.

*Meteorological Results deduced from diurnal Observations kept at the Apartments of the Royal Geological Society of Cornwall, Penzance, for 1822. By E. C. Giddy, Curator. (Communicated by Davies Gilbert, Esq. MP. FRS. President.)*

BAROMETER.													
1822.	Maximum.	Minimum.	Mean of the maximum.	Mean of the minimum.	Mean of the month.	Monthly range.	Mean daily range.	Greatest daily range.	True maximum.	True minimum.	Mean of true maximum.	Mean of true minimum.	True mean of the month.
January ...	30.26	29.10	30.05	29.96	30.005	01.16	00.10	00.50	30.212	29.053	29.999	29.903	29.951
February ...	30.48	29.10	29.94	29.84	29.890	01.38	00.10	00.57	30.414	29.034	29.880	29.774	29.827
March. ....	30.30	29.52	30.00	29.86	29.930	00.78	00.10	00.34	30.234	29.454	29.931	29.707	29.864
April. ....	30.34	29.00	29.77	29.70	29.735	01.34	00.07	00.30	30.186	28.916	29.707	29.631	29.669
May. ....	30.12	29.24	29.81	29.73	29.770	00.88	00.06	00.28	30.024	29.180	29.726	29.646	29.686
June. ....	30.04	29.52	29.80	29.85	29.875	00.52	00.04	00.14	29.958	29.412	29.795	29.732	29.768
July. ....	30.08	29.20	29.66	29.59	29.625	00.88	00.06	00.26	29.978	29.104	29.594	29.521	29.527
August. ....	30.06	29.30	29.77	29.73	29.750	00.76	00.04	00.20	29.952	29.304	29.658	29.628	29.643
September. .	30.02	29.14	29.77	29.73	29.750	00.58	00.04	00.26	29.945	29.056	29.680	29.640	29.660
October. ....	29.90	29.94	29.49	29.37	29.430	00.96	00.11	00.40	29.828	28.862	29.415	29.395	29.355
November. .	30.10	29.08	29.61	29.52	29.565	01.02	00.09	00.36	30.028	29.020	29.544	29.454	29.499
December. .	30.22	29.88	29.87	29.80	29.835	01.34	00.07	00.30	30.172	28.926	29.828	29.758	29.793
Annual													
Means, &c.	30.48	29.88	29.80	29.72	29.760	11.80	00.07	00.57	30.414	28.826	29.739	29.649	29.694
Ratio 1821	30.50	27.85	29.72	29.64	29.680	13.14	00.08	00.59	30.434	27.806	29.654	29.569	29.611

Barometer, 1822.—Highest, Feb. 27, Wind NE, 30.48; Lowest, Dec. 2, Wind W, 28.86.

1892.	DAY AND NIGHT THERMOMETER.										COMMON THERMOMETER.									
	Maximum.	Minimum.	Mean of the maximum.	Mean of the minimum.	Mean of the month.	Monthly range.	Mean daily range.	Greatest daily range.	Minimum at 8 a.m.	Maximum at 2 p.m.	Minimum at 8 p.m.	Mean at 8 a.m.	Mean at 2 p.m.	Mean at 8 p.m.	Mean of the month from the three preceding columns.	Monthly range.	Mean daily range.	Greatest daily range.		
January...	51	34	47	42	445	17	6	9	37	50	36	44	46	44	442	14	5	7		
February...	54	36	49	42	455	18	7	12	38	53	39	44	49	45	460	15	5	10		
March...	56	39	54	45	495	19	9	14	44	56	43	48	53	48	491	13	5	8		
April...	63	36	53	43	480	20	11	17	40	64	40	48	53	47	490	24	6	10		
May...	69	42	62	49	555	27	13	19	43	69	46	55	61	55	570	26	7	12		
June...	78	52	70	58	640	26	12	21	53	76	55	63	69	63	630	21	7	11		
July...	71	52	67	56	615	19	11	15	56	71	56	61	65	61	621	15	5	11		
August...	77	48	67	55	610	29	13	18	54	76	54	60	66	60	620	22	6	12		
September...	68	45	63	55	590	23	10	15	47	68	50	56	62	57	581	21	5	10		
October...	63	43	57	50	535	20	8	13	47	62	48	53	56	54	541	15	4	8		
November...	58	39	54	46	500	19	7	14	42	58	40	48	53	51	502	18	4	9		
December...	54	28	44	37	405	26	7	16	28	53	32	40	44	40	411	25	6	12		
Annual Means, &c.	78	28	57	48	525	228	9	21	28	76	32	51	56	52	536	191	5	12		
Ditto 1891	73	26	56	48	520	223	9	18	26	71	29	52	56	52	535	186	5	12		

Day and Night Thermometer.—Highest, June 6, Wind SE, 79°; Lowest, Dec. 21, Wind E, 28°.  
Common Thermometer.—Highest, June 6 and Aug. 22, Calm, 76°; Lowest, Dec. 21, Wind E, 28°.

1822.	WIND AND WEATHER.										RAIN IN INCHES.								
	North.	North-east.	East.	South-east.	South.	South-west.	West.	North-west.	Calm.	Brisk.	Boisterous.	Prevailing.	Rainy and showery days.	Dry days.	On the top of the Geological Museum, No. 1.			Total.	Ground level, No. 2.
															S. E. H.	S. P. H.			
January ..	3	5	1	0	2	12	5	12	0	3	0	NW	16	15	0.095	0.460	1.455	2.425	
February...	2	1	0	2	4	12	4	3	0	10	0	SW	11	17	0.835	0.615	1.450	2.440	
March.....	2	1	1	4	5	10	6	2	0	13	0	SW	14	17	0.555	0.920	1.475	2.800	
April.....	4	3	4	4	3	6	2	4	0	5	2	SW	10	20	1.120	0.205	1.925	2.980	
May .....	3	3	5	7	6	2	2	0	3	1	0	SE	9	23	1.055	0.040	1.095	1.340	
June .....	3	0	3	6	5	5	3	3	3	2	0	SE	10	20	0.955	0.955	1.910	2.240	
July .....	2	0	1	4	3	6	6	0	0	9	0	NW	15	16	4.335	3.055	7.390	9.070	
August.....	3	2	0	7	1	6	9	2	1	2	0	W	9	22	0.910	0.690	1.600	2.200	
September...	2	2	6	8	1	7	2	2	0	7	3	SE	10	20	0.815	0.675	1.490	1.865	
October .....	3	0	0	8	6	5	7	1	1	15	7	SE	24	7	1.845	1.560	3.405	5.315	
November...	1	1	0	1	6	11	9	1	0	14	2	SW	22	8	1.500	2.230	3.730	7.255	
December...	1	6	8	5	1	4	3	3	0	6	0	E	8	23	0.635	0.535	1.170	2.545	
Annual Results.	29	24	28	56	43	77	58	42	8	87	14	SW	158	207	15.555	11.940	27.495	41.675	
Ratio 1821	20	16	21	59	42	75	65	56	5	130	28	SW	185	180	21.160	11.350	32.510	43.500	

No. 1, 45 feet above the surface of the ground, and 143 above the level of the sea. No. 2, 90 feet above the level of the sea.

The result of very attentive observations on the rain guage throughout this year tends to confirm the conjectures offered in the number of the *Annals of Philosophy* for July last, p. 18.

The excess of rain received by the guage on the ground level is, as before, very great at particular times, and, on the whole, nearly corresponding with that of the last year, viz.

1821 was as 1 to 1·512 }  
1822 was as 1 to 1·523 } or as 2 to 3 nearly.

Among the daily differences, we found as much as 1·5 excess; and, on the other hand, in quantities very nearly equally. The least monthly excess was that of June, being ·172 only, and the greatest monthly excess that of December, which rose to 1·175. In every instance, the excess of the lower above the upper guage has been proportioned to the more or less velocity of the wind; and nothing has occurred to weaken, but much to corroborate, the idea, that it is principally, if not altogether, occasioned by the whirl or eddy produced by the recoil of the current of air from the sides of the building. This is a question of fact, which we continue to hope some of your meteorological correspondents, better provided with anemometers, will try by the test of actual experiments.

*Penzance, Feb. 14, 1823.*

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## ARTICLE VII.

*On the unconformable Position of the Pontefract Rock of Sandstone, with respect to the subjacent Coal-measures, as shown in the new Geological Map of Yorkshire; and regarding some Errors therein, as to Parts of the Ranges of certain Rocks in the Coal Series. By Mr. John Farey.*

(To the Editor of the *Annals of Philosophy*.)

SIR,

*Howland-street, Feb. 19, 1823.*

I AM one of those who have felt a strong interest in the perusal of several long and very important geological papers, chiefly from the pens of Messrs. Weaver, Conybeare, Buckland, Phillips, Sedgwick, and Winch, which have appeared in your last four volumes; and observing that Mr. Conybeare, in p. 137 of your last number, considers the continental Rock, which has of late been much the subject of discussion, under the name of *rotheliegende*, to be probably the same with the English rock, which my friend Mr. Smith has named the "Pontefract Rock," and lately depicted its course in his four-sheet Geological Map of Yorkshire, sold by Cary: I beg permission to state in your *Annals*, that I fully concur with Mr. Conybeare in opinion, that

the rock in question belongs to, and is the lowest of, the unconformable, magnesian, and gypseous series,\* which locally covers and forms the eastern boundary of the Yorkshire coal-field, and that this Pontefract rock has been improperly associated and linked with the six other coal-measure grit rocks, named by Mr. Smith on the margin of his map. I agree also with Mr. C. in thinking, that this Pontefract rock deserves a very careful examination and description. Towards the accomplishment of which object, I beg to mention as the results of much labour and thought bestowed on the investigation of particular rocks, two general maxims, which I submit, should always be kept in view by the practical geologist, viz.

1. That an individual rock or stratum *is never uniform throughout* its whole extent; that is, none of them display *at every point* the same thickness, composition, or state: nor do their beds or fissures contain *everywhere* the same organic or other extraneous matters.

2. That *no unique parts occur* locally, in any rock or stratum, except, perhaps, in places where an alluvial covering may have infiltrated or otherwise communicated its accidental properties; that is, whatever qualities or states, the beds of any individual rock or stratum may *in one place* exhibit, or whatever imbedded or venigenous matters it may *in one place* contain; each and every of such qualities, states, and matters (with the exception, seldom difficult to discriminate, which has been mentioned above), and the same beds and thicknesses, nearly, may be sought for, and with some confidence expected to occur, *in numerous other places*, in the whole course and extent of such individual rock or stratum.

Which maxims suggest forcibly, that much remains yet to be done, which the concurrence of numerous observers can only accomplish, before *all the properties* of any individual rock or stratum, and their relative frequency of occurrence, being known, the most *essential characters* can be selected (among which the precise *species* or varieties of *organic remains*, will mostly stand foremost, I believe), whereby to identify detached and far distant parts, of such individual rock or stratum.

In the pages referred to in the note below, I have shown, that the Pontefract rock of sandstone, often in so incoherent or loose a state as to be dug for *sand*, shows itself almost uninterruptedly at the western edge of the yellow or magnesian limestone, across the counties of Nottingham, Derby, York, Durham, and part of

\* On this point, viz. as to the *immediate contact* always of the Pontefract rock with the lowest magnesian limestone rock, my falling into error in 1807, and (thanks to Mr. William James as to the Bedworth case, and to Mr. James Porter as to the Garforth case) after seven years setting myself right again, respecting the *unconformableness* of the series overlying our coal-measures, had with me no effect; as will appear in pages 157, and 462 to 465 of my first volume of "Derbyshire Report;" and pages 168, 409, 410, &c. of the third volume; also in the Philosophical Magazine, vol. xiv. pages 167, 168, 174, 342, &c.; and vol. xvi. p. 283, &c. wherein I have correctly represented this important fact.

Northumberland; besides which, I have there mentioned the probability, that this same rock underlies the magnesian limestone in Salop and in Cumberland counties; and, perhaps, it exists also under that of Abberley, in Worcestershire; but these last very contorted rocks, and others such on the north of Dudley, at and southward of Breedon, in Leicestershire, and at Wild Park, in Derbyshire,\* require, I am of opinion, a great deal more of careful investigation than has yet been bestowed upon them, before any thing satisfactory can be concluded concerning them.

Mr. Conybeare's observation in p. 137 is perfectly just, as to *the highest seams of coal*, and the strata of the regular series accompanying and overlying them, not being, perhaps, displayed on the surface, in any part of the British Islands; and it is equally true, that the parts or places are, in very few of our extensive coal-fields, yet ascertained, where *the series is the most complete*, or where the highest of their conformable carboniferous strata are visible.

In the year 1816, the late Duke of Buccleugh employed me professionally, among other matters, to ascertain this point, as to the Midlothian coal-field; the full details of which investigation remain in the hands of his Grace's intelligent mineral agent, Mr. James M'Laren,† of Dalkeith; showing, that the town of Fisher-row, near to Musselburgh, stands on *the highest stratum*, of above 5000 feet thick of coal-measures, including 84 seams of coal!‡ as has been inferred, from a systematic comparison of 2374 spots or places of observation, in the pits, boreings, levels, and quarries, or visible bassets of the strata of that district; whence, the extremes and *the average thicknesses* of the greater part of 337 alternating strata are ascertained, amounting together to the total thickness above named.

I have frequently pointed out to my mineral friends resident

\* If the Breedon and Wild Park limestones (Derb. Rep. i. 158) belong to the *lower magnesian rock*, may not the gypsum hummocks of Chellaston, Aston, and Rallington (Rep. i. 149, &c.) intermediately situated, in a strata trough, belong to the gypsaceous red marl, resting immediately upon that rock, answering to the gypsum near Fairburn, in Yorkshire (Phil. Mag. vol. xxxix. p. 102), which, in Bollum Farm Well, and many others thereabouts, has proved pure and crystallized, several yards thick?—and may not the absence in Derbyshire and Yorkshire, of salt springs and rock salt, which last in Cheshire, underlies gypsum, arise from the latter belonging (as locally imbedded masses) to the upper or great red marl, overlying *the very irregularly coarse gritstone*, which, on Sherwood Forest and in other situations, I mistakenly represented as “*gravel rock*,” in most of my early writings? The Birchwood Park, and the Newbold Astbury limestones, occurring in small denudations, from whence *no basset edges could be traced*, were, in 1811, hesitatingly referred by me to the yellow limestone series (Derb. Rep. i. 409 and 461), when, but for a complete error, as to “*an upper series of coals above the yellow limestone*” (see my Index), of which I had often been told, I should have associated them, either with the shale *limestone*, or with the first limestone rock.

† And other copies in my possession. I am happy in this opportunity to mention, that Mr. M'Laren is now carrying into effect the instructions which he received from me, by making similar surveys of the districts in which his Grace's other mineral properties are situated.

‡ See Mr. Westgarth Forster's Treatise on a Section of Strata, p. 89, 2d. Edit.

in Yorkshire, that near to the edge of the yellow limestone, about south-east from Wakefield, the highest visible rock or stratum of all the great coal-field of Yorkshire, Derbyshire, and Nottinghamshire, might be found, and ought to be carefully sought for and ascertained; but which interesting service to science has not yet been performed I believe.

The late Mr. Edward Martin, whose excellent brief account of the South Wales coal-field is printed in the Philosophical Transactions for 1806, has described its highest valuable seam of coal to be found, only in a local patch or hummock, between Neath and Llanelly, but has not mentioned the place; nor has he traced the bottom line of the trough, which he merely says, "may be drawn in an east and west direction through the middle of the basin." In May, 1806, Mr. Martin favoured me, by tracing the trough-line on my large map of England and Wales, by Cary, from which I beg here to record its range. Commencing westward, it enters the southern part of Kidwelly Marsh, passes Penbre, Llanelly, Courty Carney, north of Pentalassa, by Cadoxton, Glyn-Carwg, Llandeudwy, Tonna-yodwr, Eglwysilan, Energh, Bedwas, Mynyddyslwyn, and thence curves up the Ebwith Valley to near Lanhiddel, and there ends, according to Mr. M. the dips of the measures from the north-west, north, north-east, and east, centering there, as the end of the deep line of Trough.

On comparing Mr. Smith's planning of the Pontefract rock with the interrupted and detached observations which I have had opportunities of making in Yorkshire, I beg to point out (truth being my object on all occasions) the following errors, which have been committed in the Yorkshire map, beginning south. The north part of the village of Todwick, which probably stands on the Pontefract rock, has been represented as standing on a hummock of the limestone, but whose edge does not proceed so far westward by near a mile, I believe; and the limestone is here mistakenly represented as detached by the Pontefract rock appearing round it on the east; but the large quarry of limestone (for export by the Chesterfield Canal, see *Derb. Rep.* i. 411 and 434, and iii. 318), called Pecks Mill, or Dog-kennel; and another quarry in South Anston town, east of the church (i. 411), clearly prove this to be an error.

My unfortunate ignorance of the *unconformable character* of the Pontefract rock, when surveying this border of Derbyshire in 1809, prevented my considering, as now I think probably may be the fact, that from near South Anston town to the town of Rotherham (see *Derb. Rep.* i. 169), this same rock unconformably stretches out upon the coal-measures, having its western edge in Todwick, Conduit Hill, Aston, Aughton, Treton, Catline, Brinkworth, and Eccleshall, and extends near a mile north-west from Rotherham, along the Doncaster road; and I consider my friend Smith as wrong, in representing the Rother-

ham "red rock" (g), and the "Ackworth rock" (c) as entering Derbyshire, west of Pebley Lane; believing that these rocks in proceeding southward pass under the edge of the Pontefract rock and disappear before they quit Yorkshire.

The thirteenth grit rock of the Derbyshire series, on which Killamarsh church stands, seemed proved, as well by its basset edge as by the ironstone rakes worked on both sides of it, to range northward by Belk Lane, east of Wales Wood, west of Swallownest colliery, by Park Hill and Folken (and having crossed the Rother), by Orgrave, east of Highhazles colliery, through the western part of Tinsley village (and having crossed the Don), east of Massborough, &c.

On the west and south-west of the village of Kippax, Mr. Smith has omitted to notice a point of the Pontefract rock, which spreads out beyond the village of Great Preston, and has upon it two detached hummocks of yellow limestone, which last he has placed too far north. From Berwick in-helmet, the Pontefract rock has been coloured  $1\frac{1}{2}$  mile too far down the Aberford Vale. At the same time that this remarkable unconformable projection, wants extending  $1\frac{1}{4}$  mile further west, so as to surround and underlie four detached hummocks of the yellow limestone, which, most unfortunately, my friend has wholly omitted to notice; because, I think, the sandy rock supporting these four hummocks, and resting upon the edges of five of the gritstone rocks, and six of the coal shales of my Derbyshire series, presents, when taken in connection with the facts of the Garforth colliery, and the "shell ironstone" rake (in my ninth coal shale, *Derb. Rep.* i. 217 and 414), near at hand, the completest case of the *unconformableness* of the Pontefract rock, and those above it in the new series, which can any where be found in the same space.

The largest and most western of these limestone hummocks lies on the north of Seacroft village, and east of *Rounday Farm*, which last gives name to the large old shallow quarries worked in this hummock, which is a very flat one, containing apparently only the lower yellow beds (below the "blue beds" mentioned in *Derb. Rep.* i. 157 and 408, and ii. 409), covered in great part by an alluvial clay patch, which stretches thence north-east for  $1\frac{1}{2}$  mile.

The second of these hummocks is around Barnbow houses; Berwick town stands on the next, which hummock is about a mile long north-east and south-west, and half as wide; and the last and smallest of these hummocks lies north of the last, and north and west of the Brooks, meeting on the north of Berwick: it is in part covered by alluvial clay.

The next projection of the Pontefract rock which has been omitted, extends west-south-west from the continuous edge of the limestone, and incloses the conspicuous limestone hummock of *Elstoft*, where there are quarries; and which limestone is

surrounded by dots for colouring on the map of my friend. In the next place, a hummock of the Pontefract rock has been omitted on the north-west side of Bardsea village (Derb. Rep. ii. 410, note). Another is omitted under East Keswick village; and a point of the same rock, north of the Wharfe River, extending from near Wood Hall, under Carlston Hill House, where there are gritstone quarries in it.

In addition to which, my friend has omitted to show a remarkable *denudation*, through which the Wharfe River passes in Collingham parish; wherein for  $1\frac{1}{4}$  mile in a north-west and south-east direction, and a mile in some places, transversely measured, the second coal shale is laid bare of its unconformable coverings. Also the southernmost of two singular hummocks of the limestone on the neck of the Pontefract rock separating this denudated tract from the coal-field, is omitted.

I must now return to the border of Derbyshire, and speak of the rock which Mr. Smith marks (n), and denominates "flagstone:"\* it is my fourth grit rock (Derb. Rep. i. 164, 423, and 427, &c.), and my third coal shale lies under and west of it, producing, as Mr. S. truly states, "ganister stone," to which I will add, excellent fire clay, which are its chief characteristics. From near Sheffield, past Penistone, Huddersfield, and Halifax, and so on north-westward to near Swill Hill, the range of this fourth rock is shown with sufficient accuracy, except as to the double commencement and western of the two courses coloured for this rock, from Whirlow to Stock Bridge; but the remainder of its course on the map I consider to be completely erroneous.

Near to Swill Hill, this fourth rock has arrived at its most western extension in the bottom of the *general trough in the Yorkshire coal-field*, which *trough-line* ranges from somewhere near South-Elmsal, already hinted at in p. 273, as a place south-east of Wakefield, at the edge of the Pontefract rock, and probably passes Wakefield, Woodchurch, Birstal, and High Farnley, on the north-east in each instance, and thence near Swill Hill, and so on, passing south of Pendle Hill, into the Burnley and Blackburn coal-field, in Lancashire.

From the bottom line of this important trough, near to Swill Hill, the basest edge of this celebrated fourth rock, has a general course of about east-north-east, passing, I believe, along the northern verge of Bradford Dale to the town of Bradford; from whence I have ascertained its course to be along, or not far from, the Toll Road towards Leeds, until within half a mile of Kirkstal Bridge, when this rock descends obliquely under Armley Hall, and crosses the Air River about a mile below the Bridge; from whence its course is for the next mile about north-north-west, between Kirkstal and Burley. From hence an east-north-

\* Which is an ill-chosen name; because half a dozen of the lower coal-measure rocks, produce, in particular quarries, excellent flagstones, difficultly distinguishable from those of the fourth rock.

east course is resumed by its basset, passing south of Hedingly village, and north of Woodhouse village, where there are large flagstone quarries in it; and about half a mile north-west from these, on Hedingly Common, fire-clay and ganister are dug in the third coal shale, and coarse third grit, north of this, near Mean Wood (see *Phil. Mag.* vol. xxxix. p. 102).

From the Woodhouse quarries, this fourth rock passes on the south of Potter Newton and Chapel Allerton, north of Allerton Gledhow, past Rounday flagstone quarries, north of Redhall Houses, by Berkby Farm, past Thorner flagstone quarries; and soon after, this rock, with an easy dip about south-east, disappears\* under the edge of the unconformable Pontefract rock, on the south of Thorner village.

The northern edge of the third grit rock, from the excellent and well-known quarries at Bramley Fall, by the Leeds and Liverpool Canal, declines obliquely, and crosses the Air River, about a mile above Kirkstal Bridge: thence its course is up Hascar Wood, and proceeds thence mostly with a very bold and decided northern edge, until, on the south side of Bardsey village, this rock also disappears under the Pontefract rock, in the same place and manner, as Mr. Smith has mistakenly shown, with regard to the lower or northern edge of the rock (*i*), which he calls the "Bradgate rock;" but why it is so named I do not know.

The existence of ganister (galliard, or crowstone), and the prevalence of clays adapted to the uses of the potter (like those of Newton, anciently), and the makers of furnace bricks, lying as floors to their coal seams, has been ascertained at short distances, throughout the course between the third and fourth grit rocks, indicated above, as regularly here as in Derbyshire (almost to the neighbourhood of its county town), wherein, these important features of the coal series were first ascertained, and such details published in its county report, as can leave no kind of doubt on the subject.

In the manner above described, the strata northward, continue to have a south-eastern dip (which would have been a south-south-west dip, but for the deepening of the trough line in each stratum, to the east-south-east, or nearly) into the great trough of coal-measures herein described; in consequence of which, my second grit rock approaches and passes under the Pontefract

\* There can be little reason for doubt, that after the basset edge of this fourth rock has passed some distance under the yellow limestone, it turns northward, and has an uninterrupted course under its unconformable cover, into Durham, and that there, somewhere near to Morton, Bolam, or Brusselton, this important rock emerges again from under the Pontefract rock, and has doubtless a long course across that county and Northumberland, which course I could ascertain and depict as certainly, I think, as several years ago I did, as to 94 miles of its course across Derbyshire and Yorkshire (see *Phil. Mag.* vol. lxxxix. p. 102), and as to a great many miles of its course in Flintshire (see *Phil. Mag.* vol. xlv. p. 166). Why has this matter slept, and no one attempted to map *gritstone rocks of the Wear and the Tyne coal-field*?

unconformable rock, somewhere about the junction of the sheets of Mr. Smith's map, perhaps, leaving then my first coal shale on the surface, in contact with and passing under the Pontefract rock; and it is this coal shale which, in such case, produces the Harrowgate medicinal spaws: unless indeed both the second and the first grit rocks have passed under the Pontefract unconformable rock, before we arrive so far north as these spaws?; under which last circumstances, these celebrated springs will be referable to my "limestone shale."

But whichever of these may, on a rigid investigation, prove to be the fact, it will, I think, certainly result, that a *strata ridge* (which may not be easily definable by a line on the map, or without careful levellings, owing to this ridge in the upper measures, having an irregular rounding summit), will be found emerging from under the edge of the Pontefract rock, somewhere not far from Thornton Chapel; and, proceeding westward by Pateley Bridge, there, either my shale *limestone* or the first limestone rock emerges on the surface of this ridge in a local denudation, extending to near Raven's Cross: thence this strata ridge probably declines southward, and ranges through the smaller denudated tract of limestone, north-east of Barden; then bending again northward, it probably, near Burnsall, enters a more extensive denudated tract, proceeds, perhaps, by Kirkhamallam Dale, and then bends so much northward as to pass Settle, and proceeds on the north of Clapham and Ingleton; passing thus north-east of, and occasioning the dip south-westward into the Black Burton coal-field.

Generally speaking, the strata ridge last described should be considered as the boundary northward, of the great Yorkshire coal-field; and that to the northward of this strata ridge, a district of undulating strata commences, and the same are disposed in several strata troughs, ranging eastward and westward, and are separated by corresponding strata ridges, through the remainder of the county; and, perhaps, it is the same in the south-western parts of Durham. On this principle I should, if entering on a survey of the local coal-fields of this northern part of Yorkshire, search northward of Thornton Chapel, before conjectured to stand on the great strata ridge, for the emerging of the first grit rock\* (dipping north-eastward, and covered more northerly by the first coal shale†) from under the unconformable

\* My first grit rock may probably be comprised in Nos. 104 to 95 in Mr. W. Forster's "Section."

† Part of my first coal shale probably appears in his Nos. 94 to 91, and the remainder of this shale is omitted, I think; especially as Mr. F. himself, in p. 93, suspects some strata to be here omitted; but the number and thickness of these (including, probably, I think, my second and third grit rocks, and the intervening second coal shale) are, perhaps, much more considerable than he is aware of. Where is the *ganister* found? or its peculiar species of leafy *reefs*?, or the two species of *marinus* shells? enumerated in p. 243, vol. i. of Mr. Sowerby's "Mineral Conchology," in the counties of Durham or Northumberland?; and is this stone there any where applied to the making of roads?

Pontefract rock; and this first grit rock, I doubt not it would appear to be which my friend has represented as surrounding westwardly the trough of lower coal measures on the west of Masham; but which rock mistakenly has been coloured as a continuation from Swill Hill, mentioned p. 275, of his "flag-stone rock," answering to my fourth grit.

Similar remarks apply to the smaller coal trough on the west of Patrick Brompton, although its strata are in part covered and superficially bounded on the east, by unconformable upper red marl, overlapping previously the yellow limestones and their intervening gypseous marl, and then the edge of the Pontefract rock, in these parts.

I will now only further remark, that the cause of the disappointments seems now apparent, from the boreings made some years ago in search of coals on the west of East Ripton village, south of the Wharfe River, and westward of Sicklin Hall and Spofforth, on the north of the river, as well as those older trials doubtless made, but, perhaps, not recorded, in the Collingham denudation, mentioned p. 275, viz. that *no thick or valuable seams of coal occur, in so low a part of the carboniferous series*; but which spots, even yet, it would seem, my friend considers to have strata higher in the series than the fourth grit rock, the usual limit downwards, in these parts, of marketable seams.

I am, Sir, yours, &c.

JOHN FAREY.

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### ARTICLE VIII.

*Memoir illustrative of a general Geological Map of the principal Mountain Chains of Europe.* By the Rev. W. D. Conybeare, FRS. &c.

(Continued from p. 218.)

\* \* \* In citing the authority of Boué as confirming the opinions of Prof. Buckland and myself, with regard to the distinction between the todte liegende and old red sandstone of England, I was not aware that he had formerly entertained different views; this I have since learned from his memoir on Germany, inserted in the volume of the Wernerian Transactions recently published. That memoir, however, was read in the beginning of 1822, before the appearance of the arguments on this subject brought forward in the "Outlines." Since that period, M. Boué has enjoyed opportunities of studying many other localities of this formation, and has, with a candour that does him honour, made the concession which I have quoted; the letter I have cited bears date Vienna, Jan. 14, 1823. It may be considered, there-

fore, as conveying his matured opinion, and as I have, through his correspondent, his sanction in extracting any observations that appeared to me important, my citation may be regarded as an authentic declaration of his present views. I have thought this explanation necessary to remove any apparent contradiction between it and the memoir alluded to. I need not add that his testimony on my side derives additional weight from the circumstance, that his judgment was not hastily formed, but the result of a slow conviction.

*The Series of Lias and Oolites, Muschelkalk, and Jura Kalkstein.*

This system viewed generally consists of a series of alternating deposits of clay, and of limestones of a texture considerably more earthy and less compact than those of the preceding epochs, and often oolitic. These are strongly contrasted also with the earlier formations in the very important circumstance of their zoological character, I mean the nature of the organic remains which they contain, as they appear to preserve the traces of a very different and more advanced state of animal population in the waters beneath which they were accumulated. In speaking, however, of the earlier formations, I would be understood to mean those earlier than the alpine limestone; for the alpine limestone is clearly in this respect connected more nearly with the subsequent than antecedent order of things; and these observations should, therefore, in strict propriety, have stood at the head of that formation; but since the organic remains in that series are rather of local than common occurrence; whereas in the present they form a striking and general character, this deviation from the rigour of method may be excused. The vertebral class, of which rare and faint traces alone occur in the older rocks, presents in these a rich and most interesting addition to the lists of animals now existing; these principally belong to the order of oviparous and amphibious animals; but exhibit in some instances an organisation which would fit them rather for a life entirely aquatic than one divided between both elements (e. g. the ichthyosaurus), and appear to bear the same relation to the actual genera that the cetacea do to other mammalia: others, however, are like their recent antitypes fitted for moving both in the land and water. In this zoology of a former world, we count several species of the crocodile (England and France afford four at least which may be distinguished): these do not agree specifically with any of the varieties now known to exist; but the differences are very slight, often indeed less marked than between the recent species; there is no room, therefore, to doubt that their general habits were similar. Were these species capable of enduring the cold of high latitudes, and there hatching their eggs? or did they migrate? or was the temperature of our planet then higher than at present? Some varieties

of recent crocodiles have been found out at sea a considerable distance from the land. It becomes the inquisitive spirit of science to propose these problems ;—it becomes the modesty of that spirit to hesitate in attempting (without more ample data than we at present possess) their solution. Many other genera referable to the great order of the lizard family, but evidently partially or entirely aquatic, which have either entirely vanished from, or (and this is from their bulk and striking appearance very improbable) remain undetected in the actual state of animated nature, are preserved in these beds to exercise the researches of the comparative anatomist. We already reckon, and the subject is yet far from exhausted, four distinct genera of this kind (1. The fossil saurian, of Maestricht; 2. That of Stonesfield, *megalosaurus*; 3. The *plesiosaurus*; 4. The *ichthyosaurus*); many of these, the latter especially (which unites to the head of a lizard the vertebral column of a fish, and combines the modes of progression peculiar to each), present links as important as striking in the great chain of animal being: the lines of *Lucretius* are, therefore, in one sense\* philosophically true:

Multaque tum tellus etiam portenta creare  
Conata 'st mirâ facie membrisque coorta,  
Multaque tum interiisse animantum sæcla necesse est.

Turtles more or less approaching to the recent are likewise found. Here again we have some presumption, though certainly no proof, of a warmer climate.

Mingled with these remains (though in such small quantity as to show that they have been casually brought from a distance, while the former are found under circumstances which indicate their having lived and died in their present sites), are the disjecta membra of land animals (the *didelphys* of Stonesfield), of birds (also Stonesfield), and of coleopterous insects (also Stonesfield), sufficiently proving that the earth and air were peopled with life as well as the waters.

The vegetables of this series are dicotyledonous as well as monocotyledonous. The shells are characterized by the disappearance of many of the genera, and most of the species, belonging to the transition and carboniferous series, and the introduction of many new genera, almost all the species being also new; scarcely indeed a single species can be identified. The same remark applies equally to the zoophytes.

In one instance, that of the encrinites, Mr. Miller has made an observation as important and beautiful as it is original. This very interesting order of animals considered merely in the relations of

\* Not certainly in that which bears an aspect favourable to materialism, for it is impossible to point out instances of design more striking or beautiful than are often exhibited in the structure of these fossil animals. Thus much even in the present obscurity of the subject we can perceive; and did we fully know the various revolutions of our planet, we should doubtless find fresh reason to admire the adaptation of its living occupants to its existing state at every successive period.

this structure, divide themselves into two well marked and distinct series. Now of these one only is found in the older rocks; the other is confined to those more recent than the coal formation. It is very probable that ulterior researches may extend similar conclusions to many other classes; it is impossible that the subject should be in better hands than those of the author just alluded to, whose unwearied patience in the investigation of those minute details which afford the only sure grounds for such an induction, is joined to that philosophical spirit of generalisation which can alone combine them into a luminous system.

The series of this system in England where it has been most fully studied, consists of three great deposits of a calcareo-argillaceous character, alternating with three others of calcareo-siliceous matter and of oolitic limestones.

- |                           |  |
|---------------------------|--|
| 1. Upper Oolitic system.  | { <ul style="list-style-type: none"> <li>a. Argillo-calcareous Purbeck strata, separating the iron sand and oolitic series.</li> <li>b. Oolitic strata of Portland, Tisbury, and Aylesbury.</li> <li>c. Calcareous sand and concretions (Shot-over hill and Thame.)</li> <li>d. Argillo-calcareous formation of Kimmeridge and the vale of Berks, separating the oolites of this and the next system—Oaktree clay of Smith (generally).</li> </ul>                         |
| 2. Middle Oolitic system. | { <ul style="list-style-type: none"> <li>a. Oolitic strata associated with the Coral rag (Pisolite of Smith).</li> <li>b. Calcareous sand and grit.</li> <li>c. Great Oxford clay (Clunch clay of Smith) separating the oolites of this and the next system.</li> </ul>  |
| 3. Lower Oolitic system.  | { <ul style="list-style-type: none"> <li>a. Numerous oolitic strata, occasionally subdivided by thin argillaceous beds; including the Cornbrash, Forest marble, schistose oolite and sand of Stonesfield and Hinton, great oolite, and inferior oolite.</li> <li>b. Calcareo-siliceous sand, supporting and passing into the inferior oolite.</li> <li>c. Great argillo-calcareous formation of lias and lias marle, constituting the base of the whole series.</li> </ul> |

N. B. This list is in a descending order.

The most probable analogies of these with the continental formations appear to be the following, in which I have inverted the preceding order, and followed the ascending series.

1. *Lias* (3. c), *Muschelkalk*, and *inferior Beds of the Jura Limestone*.—There is still some doubt whether the term muschel-

kalk may not be applied to more than one formation;\* first, it is sometimes applied to the Wurtzbourg limestone, which Keferstein classes with the alpine, and which has, therefore, been so coloured in the accompanying map on his authority, though I have already expressed my own inclination to consider it as representing our lias. Secondly, there is less doubt that the rocks to which the name muschelkalkstein have been applied in the north of Germany (where, I believe, it was originally adopted) are equivalent to our lias. Thirdly, it is certain that most of the beds commonly included under this designation on the north-west and north of the Jura chain and the Rauhe Alp, extending into Wurtemberg, &c. are identical with our lias, as also the beds underlying the oolites of the *côte d'or* in the north-east of France, extending towards Metz, &c. &c.

2. *Sandstone of the inferior Oolite* (3 b).—Arenaceous and iron shot beds are interposed between the Jura lias and oolites near Lons le Saulnier, and through the whole northern escarpment of that chain, and its prolongation the Rauhe Alp (*Eisen sandstein of Keferstein*). According to Haussman and Keferstein, the quadersandstein in the north of Germany is similarly placed; but I suspect that more than one formation may be confounded under this denomination, and the local circumstances under which it occurs are such as to render the determination of the question difficult.

3. The other divisions of the series, including all the beds which, properly speaking, can be termed oolites, resemble one another so much that their individual identification with those of the Continent can hardly be expected in the present state of science; if indeed, which seems very questionable, a close agreement in these minor features of arrangement really exists, through very extensive tracts, and in very distant sites. Such a

\* M. Boué appears to be of opinion that the muschelkalkstein of Central Germany is not our lias, but a formation wanting in England, and intermediate between the new red sandstone and lias being separated from the latter by the quadersandstein; so that the ascending series is: 1. New red sandstone; [2. Muschelkalkstein; 3. Quadersandstein] 4. Lias and oolites, the formations between brackets being absent in England. He cites a section near Amberg in confirmation of this position. This debateable ground refers principally to the Wurtzbourg district, and the country extending thence towards the frontiers of Bohemia. I feel that it is more important at present to state difficulties of this kind than attempt an hasty solution. I shall return to this subject in tracing the course of these formations; on the whole I certainly incline to the view which I have indicated in the text. Those who distinguish the muschelkalk as a peculiar formation refer to two principal localities; first, the platform of Wurtzbourg (which has been just mentioned); and, secondly, in the North of Germany, at Goettingen, &c. they consider it as characterized from the lias by the absence of the alternating argillaceous strata so common in that formation. It is an earthy limestone, generally of pale colours (greyish or yellowish), has some subordinate marly, arenaceous, and oolitic beds, with occasional layers and nodules of chert; its peculiar petrifications are: *Chamites striatus*, *Belemnites paxillosus*, *Ammonites amaltheus*, *A. nodosus*, *A. angulatus*, *A. papyraceus*, *Nautilites binodatus*, *Buccinites gregarius*, *Trochilites lavis*, *Turbinites cerithius*, *Myacites ventricosus*, *Pectinites reticulatus*, *Ostracites spondylioides*, *Terebratulites fragilis*, *T. vulgaris*, *Gryphites cymbium*, *G. suillus*, *Mytilites socialis*, *Pentacrinites vulgaris*, *Encrinites liliformis*. See also the note of M. Boué on the north of Germany.

correspondence, however, may certainly be traced on the opposite shores of the British channel, and there seem to be strong indications of the same kind in the Jura chain.

#### DISTRIBUTION OF THIS SERIES.

##### (A.) *England.*

A line drawn from the north-east of Yorkshire to the south-west of Dorsetshire will nearly indicate the western and inferior limit of this series. The lias, however, extends westwards from this boundary on both sides the British Channel in Somersetshire and Glamorganshire. The eastern and superior limit follows a line nearly parallel to the former, and between 20 and 41 miles to the south-east of it. The three systems of oolites form as many parallel ridges of hills separated by intermediate valleys, following the course of the intermediate clays. The great or third oolitic system forms the most extensive and continuous of these ranges. The chains presented by the other two are more partial, and interrupted through considerable intervals.

##### (B.) *Other British Islands.*

The British Islands present no traces of these formations beyond the limits above assigned, with the exception of the lias, which occurs in Ireland in the county of Antrim, near the south-east border of the basaltic district; in the Isle of Sky, and some other of the Hebrides.

##### (C.) *France.*

1. *In the Denudation of Boulogne.*—The escarpment of the chalk sweeps round a semicircular tract, in the centre of which this town is placed, with a radius of about 12 miles distance. The green sand follows the foot of this escarpment, and may also be traced on the north-east of Uissant, and the hills close to Boulogne, on which Buonaparte's tower is built, are capped with strata of ferruginous sand, possibly our own iron sand. But instead of these formations occupying, as in the corresponding tract in Kent and Sussex on the English side, the whole of the denuded area, we find the coast, through the greater part of its breadth, lined with a calcareo-argillaceous formation which underlies the sand last mentioned; this may be studied in the cliffs on either side of Boulogne, which, however, are, from the nature of the material, in a very crumbling state.

One of the upper beds of this formation consists of a remarkable calcareo-siliceous grit. Boulogne is principally built of this rock. It often forms the upper stratum of the cliffs on the north-east of that town, and is particularly abundant at a place called le Creche, between it and Uissant. This rests on some beds of argillaceous limestone, separated by clay; and along the

bottom of this cliff is found a coarse limestone of a brown colour, full of cornua ammonis, turbinated univalves, &c.

These alternations of argillaceous and calcareous beds continue to extend into the interior towards the east of Boulogne. In a deep pit sunk at Souverain Moulin, about five miles from the town in that direction, in a fruitless attempt to procure coal, the workmen passed through 12 such alternations, and then pierced a solid calcareous rock 100 feet in thickness, containing ammonites. Below this, occurred a thin seam of wood coal, and then 20 feet of a shelly limestone full of turbinated univalves, small oysters, serpulæ, &c. together with impressions of ferns and other vegetables; and, lastly, another thin seam of carbonized wood, resting on coarse limestone.

The coral rag is exhibited with well-marked characters about nine miles south-east of Boulogne, near Samers, and the great oolite may be seen in the country round Marquise at the same distance on the north-east. Both these points approach very closely to the chalk escarpment; so that if the green sand, iron sand, &c. exist at all in these directions, they must be greatly reduced in extent.

Near Marquise, the oolite comes in contact even with the older rocks of the coal formation, which show themselves in this corner of the denudation almost immediately beneath the chalk. This coal district presents a band of mountain limestone accompanied by another of regular coal-measures. The principal marble quarries are at Ferques, and the principal coal mines at Hardingen. The stratification is extremely confused and contorted.

2. Within the interior area of the basin of Paris, there is also a small denudation exposing the oolites, in a district called the Pays de Bray, a little north-west of Beauvais.

3. *Surrounding the Chalk and Green Sand of the Paris Basin.*—This series also may be seen emerging from beneath the chalk in the coast westwards from the mouth of the Seine, where they have been traced by M. de la Beche.

Along the mouth of the Seine, on both sides, the chalk and green sand repose on a blue marl and marl-stone. At Trouville sur Mer, the oolites of the upper and middle formation, i. e. the Portland stone and coral rag, emerge from beneath this marl. Between Villers sur Mer and Dives, the clay separating the second and third system (the Oxford clay) forms the base of the cliffs, which are capped by the lower beds of the coral rag, and an overlying mass of green sand. Still further west by St. Comme, Arromarche, St. Honorine, Virreville, and Grandcamp, the cliffs present the inferior oolite resting on lias.

Hence, a zone of these formations extends circling round the chalky and arenaceous border of the basin of Paris, by Caen, Alençon, Poitiers, Bourges, Auxerre, Bar le Due, and Mezieres, the oolites stretching beyond all these places, succeeded at a

wider distance by the subjacent lias; and still further by the red marl, which, with a few occasional interventions of coal-measures, reposes on the transition and primitive chains of Bretagne and la Vendée on the west; of Limousin, Auvergne, Forez, Beaujolais, and Morvan on the south, and of the Vosges on the east.

The oolites of this range form the lofty hills known by the name of cote d'or. Here the oolites approach closely to the primitive chain of Morvan. M. Boué says, that the muschelkalkstein may be traced completely round the Vosges.

4. *South of France surrounding the Basin of the Garonne.*—The tertiary basin of this district is described by M. Boué as limited by Bayonne, Carcassouc, Montaubon, and Talmont. This is succeeded on the north by Jura limestone, covered by iron sand, &c.

In approaching the Pyrenees, he has observed a band of Jura limestone succeeded by quadersandstein with lignite, reposing in some places (as near Dax) on muschelkalkstein.

#### (D.) *Northern Germany.*

The muschelkalkstein is here found among the formations skirting the Hartz, both on the north and on the south, ranging in the former direction on the south of Hanover and Brunswick,\* and on the latter forming a platform in the country between the Hartz and the Thuringerwald, on the escarpment of which it is

\* M. Boué in one of the letters to which I have so often referred gives the following description of the muschelkalkstein of Brunswick, which is the best I have yet seen.

"This formation lies above the variegated sandstone. Its organic remains are encrinites and pentacrinites, some species of terebratula, especially the subrotunda and carnea, pectines, venericardie? a bivalve badly preserved and unascertained, but generally and loosely classed as musculites, an univalve approximating to turbo, an ammonite not carinated, a small indistinct univalve, uncertain whether nautilus or serpulula, and many singular markings in the form of elongated and serpentine cylinders which might be, perhaps, considered as merely accidental configurations of the calcareous paste, but yet from their ramification, &c. assume an appearance resembling that of the Isis tribe. The masses of lead glance, occasionally found in this formation, at once distinguish it from the Jura limestone, yet in the lowest members of this muschelkalk are portions of an oolitic texture closely associated with the subjacent variegated sandstone.

"Above it lies an argillaceous, calcareous, and arenaceous formation, in which slate clay, more or less bituminous, alternates. In the under part, it alternates with a few beds of a limestone like the muschelkalk, but without shells; in other parts, and especially in the upper, with a compact sandstone, with a calcareous or argillaceous cement.

"The whole is covered with the quadersandstein, a formation often with a siliceous cement, and very compact, but at other times of very slight coherence. Associated with and beneath this formation, I observed two beds of a compact calcareous marl, with fragments of pinna, retaining their shelly texture, and not petrified.

"The argillaceous formation contains in the bituminous portions mineral charcoal, fragments of bituminous wood, and a kind of pitch coal used in some places as fuel, and impressions of monocotyledenous vegetables.

"Ammonites (of the subdivision planulites), of small size, occur abundantly; but are confined to particular beds; in others a large common ammonite with sound back and not carinated is found in ironstone nodules. I observed also a species of terebratula? or, perhaps, gryphites; one or two species of donax; three or four unascertained bivalves; an univalve somewhat resembling clasilis; and more rarely remains of very fine pentacrinites."

286 *Rev. W. D. Conybeare on a Geological Map of* [APRIL,  
well exposed in the descent from the memorable planes of Jena.  
It is covered by quadersanderstein, on which, according to  
Haussman, Keferstein, Bouè, &c. repose in the north of Germany  
marls, oolites, ferruginous oolites, and chalk.

Some observations of Prof. Buckland indicate considerable derangement in the Brunswick district; a ridge of chalk extends on the north of the Hartz towards Goslar, often in highly inclined beds, and forming a hog's back; yet on the north of this, where the more recent deposits might be expected, the variegated sandstone again protrudes.

South of the Thuringerwald, the same muschelkalkstein extends by Heldburghausen and Cobourg.

#### (E.) *Central Germany.*

According to Bouè and Humboldt, the platform of Wurtzbourg considered by Keferstein as zechstein, or alpine limestone, is muschelkalkstein. I have already alluded to its position in describing the alpine limestone. If this view be correct, the sandstone between Wurtzbourg and Bamberg is not, as represented by Keferstein, buntersandstein, but quadersandstein.

#### (F.) *Great Range of Franconia, the Swabian and Bauhe Alp, and the Jura Chain.*

This is an immense and continuous band principally occupied by the oolitic series, crowned in some places by still more recent deposits of the same age with the chalk marl, &c. of England.

In the most northerly part of its course through Franconia, it forms the platform between Bamberg and Bayreuth. We have here a compact limestone (celebrated for its caverns containing the remains of fossil bears), &c. a sandstone, and some marly beds much resembling in fossils and character our chalk marl: this system rests on the sandstone extending between Bamberg and Wurtzbourg. It is to be regretted that the obscurity which still hangs over the relations of this tract affects equally the superincumbent limestone, &c. Bouè considers this sandstone as quadersandstone and the limestone as representing the whole oolitic series.

Further south, he gives the following as the section presented near Amberg: 1. Quadersandstein. 2. Ditto with hydrate of iron. 3. Lias, with gryphœa arcuata, gryphœa gigas, belemnites, &c. 4. Argillaceous rocks with hydrate of iron, ammonites, mya, cerithia, &c. 5. Several varieties of Jura limestone, magnesian compact, and oolitic. 6. Marl, with hydrate of iron. If the gryphœa gigas be the same with the gryphœa dilatata. This series is in the ascending order.

I should prefer considering the deposit No. 2 as our Oxford clay. I should then identify the deposit here called quadersandstein

with the sand of the inferior oolite, and reckon the subjacent Wurtzbourg muschelkalk as the equivalent of our lias. Nos. 3, 4, 5, would then represent the oolites of our middle or upper system; but these are points which cannot be fully settled until resident geologists shall arise on the spot, and publish detailed accounts of its structure and fossils.

Near Ratisbon the Jura limestone is covered, according to Buckland and Boué, by deposits of green sand and chloritose chalk; the solenhofen lithographic strata belong, I believe, to one of the beds in the upper part of the series of Jura limestones, perhaps corresponding to our chalk marl.

Here the beds decline to the south, plunging beneath the trough of tertiary formations which intervenes between this chain and the Alps, and rising north, present their outcrop in the escarpment on that side.

In its passage through Wirtemberg, the chain presents, along the base of its northern escarpment, decided lias at Boll, &c. south of Esslingen; a ferruginous sand (that of our inferior oolite) succeeds; and then the oolitic series forming the great mass of the hills.

By tracing this escarpment from this point to Bamberg, and examining the relations of the Wurtzbourg platform with reference to this chain in the vicinity of Stutgard, much of the obscurity I have alluded to would probably be removed; because we here stand on certain ground as to the identity of the Boll rocks and the English lias, which would afford a secure basis to the other terms of the comparison.

Merian is said to have published a good account of this chain in the vicinity of Basle,\* but I regret not having been able to consult it.

There is a very interesting description of the part of this chain near Lons de Saulnier by Mr. Charbant, in which it appears that its base is composed of red and variegated marl containing gypsum; that on this reposes an extensive formation of beds of gryphite limestone (lias), alternating with marls containing fossils exactly similar to those which characterise this formation in England; that these are covered by a series of oolitic beds, the lowest of which (like our inferior oolite) abounds in ferruginous particles, and is surmounted by a fine granular oolite and freestone, forming the escarpment of the first terrace of these hills; and lastly that, above this first terrace, are others each presenting an oolitic series based on argillaceous beds which separate it from that beneath.

We have here, therefore, the enumeration of the lower forma-

\* The denudation of the Rhein, that near Basle, exhibits a patch of primitive and transition rocks, over which the calcareous beds of the Jura extend. This patch is probably connected with the primitive rocks which further north emerge in the chain of the Black Forest.

tions of the Jura chain, where they crop out along the base of its escarpment on the north-west; and an account by Von Buch of the environs of Neufchatel completes the account by adding the beds in the central and south-eastern regions, where they finally dip beneath the tertiary formations. He divides these formations into four series which he enumerates in a descending order. I have ventured to assign their probable English equivalent, but these can only be considered as mere conjectures at present.

*First Series.*

29 beds of oolites contain madre-	
pores, &c. ....	Coral rag.
Upper marl .....	Oxford clay.
32 oolitic beds. ....	Cornbrash.
Second marl. ....	Clay beneath the cornbrash.

*Second Series.*

486 more compact beds, some of	
which contain strombites. ....	Forest marble.

*Third Series.*

350 oolitic beds .....	Great oolite.
60 beds hard black marl. ....	Clay of the fuller's earth.

*Fourth Series.*

100 beds not described, probably	
the same with those above Lons	
le saulnier .....	Inferior oolite and lias.

The Jura chain exhibits arched and contorted stratification through its central ridges; but on the north-west all the beds crop out, and on the south-east dip rapidly beneath the tertiary formations which occupy the great valley of Switzerland separating these ridges from the Alps. The exterior chains of the Alps again exhibit in what have been called the younger varieties of alpine limestone, beds of the same age with the above.

On the south of Geneva, the continuation of the Jura chain unites with these external calcareous chains of the Alps; and in this vicinity, therefore, the true relations of the several formations ought to be particularly examined. Hence they are continued south through Dauphiny and Provence, but I am not acquainted with any good account of their structure in this quarter.

(G.) *The Alps.*

For an account of these formations in the external chains of the Alps, I refer entirely to the excellent paper of Prof. Buckland, which has already appeared in the *Annals*.

(H.) *Hungary.*

According to Beudant, rocks referable to the Jurassic series occur principally in two localities; first, on the south-west, extending northwards from Lake Balaton or Platten; and secondly, on the south-east towards the borders of Transylvania above the sources of the river Korosh.

He gives the following list of these formations:

1. An encrinal limestone (most probably the muschelkalk): this is most abundant in the second district, but occurs also in the first; it is covered by

2. A magnesian limestone: not the same with the English magnesian lime or zechstein, but associated with, and constituting a part of the true Jurassic limestone which covers it: it exhibits three varieties: *a.* Saccaroidal; *b.* Compact; *c.* Oolitic. This formation abounds in the first district between Bude and Lake Balaton.

3. Jurassic limestone: *a.* Containing hippurites; *b.* With great oysters; *c.* With mumulites; *d.* Abounding in shells; *e.* Bituminiferous. This exists in many patches throughout the first district between Gran and Lake Balaton.

(I.) *Russia, and other imperfectly ascertained Localities.*

In Russia, the lias certainly occurs near Moscow, but we have no distinct particulars concerning the secondary formations of that vast country.

The tracts of these formations in England, France, Germany, and Russia, may be considered as parts of a continuous series of deposits occupying the great central basin of Europe. That on the Italian side of the Alps, although necessarily mentioned in connexion with that chain, belongs to a distinct and southern basin, of which a great part of Spain also probably forms a portion. Much of the limestone of that country may perhaps prove to be lias, and the oolites are distinctly described by Mr. Townshend as extending round Anchuela del Campo, about half way between Saragossa and Madrid.

(To be continued.)

## ARTICLE IX.

*An Account of some Results obtained by the combined Action of Heat and Compression upon certain Fluids, such as Water, Alcohol, Sulphuric Ether, and the rectified Oil of Petroleum.*  
By M. le Baron Cagniard de la Tour.\*

It is well known that by means of a Papin's digester, the temperature of fluids may be raised much above their usual boiling point; and we are led to suppose that the internal pressure which increases with the temperature would be an obstacle to the total evaporation of the fluid, especially if the space left above the fluid be not considerable.

In reflecting on this subject, it occurred to me that the expansion of a volatile fluid had necessarily some limit, beyond which the liquid, notwithstanding the pressure, must be converted into vapour, little as the capacity of the apparatus allows the fluid to extend beyond its maximum of dilatation.

In order to verify this opinion, I put some alcohol of specific gravity 0.837, and a ball or sphere of quartz, into a small Papin's digester made of the end of a very thick gun barrel; the fluid occupied nearly one of the apparatus. Having noticed the kind of noise which the ball occasioned while rolling in the cold gun barrel, and afterwards when it was slightly heated, I arrived at a point in which the ball seemed to rebound at each percussion, as if it was no longer surrounded by a fluid in the gun barrel. This effect was best observed by applying the ear to the end of the handle, which served to support the machine; it ceased upon cooling, and was reproduced when the necessary degree of heat was again applied.

The same experiment was repeated with water, but with imperfect success; for on account of the high temperature which it was necessary to employ, the apparatus could not be perfectly closed. With sulphuric ether and oil of petroleum, the case was different; they presented the same phenomena as alcohol.

In order to observe these effects of heat and compression with greater facility, I put the same liquids into small glass tubes closed at one end, and afterwards at the other, by means of the blowpipe. A small piece of glass was fastened to each tube to serve as a handle.

One of the tubes into which alcohol was introduced so as to occupy nearly two-fifths of it, was heated with the precautions requisite to prevent its being broken; in proportion as the fluid expanded, its mobility became greater; the fluid after having attained

\* From the *Annales de Chimie et de Physique*, tom. xxi. p. 127.

nearly double its original volume, disappeared completely, and was converted into so transparent a vapour that the tube seemed suddenly empty; but on suffering it to cool for a moment, a very thick cloud was formed, after which the fluid reappeared in its original state. A second tube, nearly half full of the same fluid, gave a similar result; but a third, of which the fluid occupied more than half, was broken.

Similar experiments made with oil of petroleum, of specific gravity about 0.807, and with ether, presented analogous results, excepting that the ether appeared to require less space than the oil of petroleum to be converted into vapour without breaking the tubes, and the latter less than alcohol, which seems to indicate that the more a fluid is naturally dilated, the less volume it takes to attain its maximum of expansion.

All the tubes in these trials were exhausted of air before they were closed; the experiments when repeated with tubes in which the air was left, gave similar results; the progressive expansion of the fluid was even more easily estimated in the latter case, there being no inconvenient ebullition as in the former.

The last experiment was made with a glass tube about one-third full of water; this tube lost its transparency, and broke a few seconds afterwards. It appears that at a high temperature water is capable of decomposing glass by combining with its alkali; this suggests the idea that some other result interesting to chemistry may, perhaps, be obtained by increasing the applications of this process of decomposition.

By carefully observing the experimental tubes in which the air had been left, it was remarked that those in which the fluid matter had not quite space enough to acquire the dilatation preceding its conversion to vapour, did not always break immediately after the fluid appeared to have completely filled this space, and the explosion was slower as the excess of fluid was less apparent.

May it not be concluded that fluids which are usually but slightly compressible at a low temperature, become more so at a higher temperature? and still more strongly in the present case, in which the liquid is ready to become an elastic fluid under a pressure, which, according to theoretical calculations, would appear to be equal to several hundred atmospheres?

With respect to this, there will probably be some difficulty in admitting, that a small glass tube scarcely three millimetres in diameter, and scarcely one millimetre thick, should resist so considerable an expansive force; it will, perhaps, be thought preferable to suppose that the molecules of an elastic fluid, and particularly of a fluid vapour, are susceptible at a certain degree of compression and heat, of assuming a change of state similar to semifusion, and capable of facilitating a greater reduction of volume than that derived from the absolute pressure.

Until these doubts are removed by new experiments, it appears that we may recapitulate what has been stated in the following conclusions :

1. That alcohol of specific gravity 0·817, oil of petroleum of specific gravity 0·807, and sulphuric ether, submitted to the action of heat and compression, are susceptible of being completely reduced to vapour under a volume rather exceeding twice that of each fluid.

2. That an increase of pressure occasioned by the presence of air in several of the experiments which have been described, occasioned no obstacle to the evaporation of the fluid in the same space, that it merely rendered its expansion more quiet and more easy of observation until the moment in which the fluid suddenly disappeared.

3. That water, although undoubtedly susceptible of being reduced to very compressed vapour, could not be subjected to complete experiments for want of sufficient means to close the compressing instrument perfectly, as well as that it alters the transparency of glass tubes by combining with the alkali which enters into their composition.

I have presumed that this notice would particularly interest those who are concerned in the use of steam-engines, and also probably furnish some slight indications for the solution of the question relating to the compressibility of fluids, lately proposed as a prize subject by the Institute ; it is this which determined me to present it to the class, my chief ambition being to prove, that I desire to render myself more and more worthy of the favourable reception which it has bestowed upon my former labours.

#### *Supplement to the preceding Memoir.*

I have attempted to determine the pressure which ether and alcohol exert at the moment in which these fluids are suddenly reduced to vapour. The method I adopted was the following :

*Exper. 1.*—I took a tube, *a b c* (see fig. next page), of the most even bore I could obtain, the interior diameter of which was one millimetre ; I united it to the tube *d e f* ; the internal diameter of which was about  $4\frac{1}{4}$  millimetres. The apparatus then resembled a syphon barometer. The two ends, *a* and *f*, remaining open, mercury was first introduced, and afterwards sulphuric ether. The mercury occupied the space *b c d e*, and the ether the space *e f* ; by inclining the apparatus, it was easy to alter the level of the mercury so as to fill the space *b a* ; by these means it was ascertained that a variation of one millimetre in the large tube, caused a variation of 20 millimetres in the smaller one ; a proportion which was judged sufficient for the graduation required. The space *b a* is that which the mercury may occupy, when its level *e* in the large tube is sunk to the point *d*, the length *a b* is

528 millimetres; the space  $d f$  double  $e f$ , is that supposed to be occupied by the ether when it is entirely reduced to vapour.

The 528 millimetres were graduated on a separate scale, which was applied when required to the tube, as it was graduated towards the upper part.

The apparatus, prepared as above described, being closed at the extremities  $a, f$ , was heated with the requisite precautions. At the moment in which the ether was completely reduced to vapour, the level  $b$  of the mercury rose to the point  $g$ , the distance of which from the point  $a$  is 14 millimetres; thus the column of air, which was 528 millimetres long, was reduced to 14 millimetres; this indicates a pressure of 37 or 38 atmospheres. This experiment three times repeated gave each time the same result.

Ether is, therefore, susceptible of being reduced to vapour in a space less than double that of its original volume, and in this state of vapour, it exerts a pressure of 37 or 38 atmospheres in the tube which contains it.

*Exper. 2.*—Alcohol of specific gravity 0.817 was substituted for ether in the apparatus above described by opening the ends  $f$  and  $a$ ; the alcohol occupied the space  $f e$ , that is to say, one-third of that supposed to be necessary for the total conversion of the alcohol into vapour. The mercury occupied the space  $b' b c d e'$ , and filled the small tube, when, by inclining the apparatus, the level  $e'$  was sunk to  $d$ . The length of the column of air  $a b'$  was 476 millimetres. After the extremities  $a$  and  $f$  were closed by the blowpipe, the apparatus was exposed to heat with the same precautions as observed with respect to the ether. At the moment in which the alcohol was totally reduced to vapour, the level  $b'$  of the mercury rose to the point  $g'$ , that is to say, to four millimetres from the point  $a$ . Thus the column of air, of 476 millimetres in length, was reduced to four millimetres, which indicates a pressure of 119 atmospheres.

Alcohol may, therefore, be totally reduced to vapour in a space rather less than three times that of its original volume; and at this degree of expansion, it exerts a pressure of 119 atmospheres on the tube which contains it.

The extremities  $a$  and  $f$  were a little drawn out, in order that they might be more readily closed by the blowpipe; and the capacity of these parts of the tube was ascertained by introduc-



ing a little mercury, which was afterwards passed into the cylindrical part of the tube as a method of measuring it. By this precaution, it was determined, for example, that the length  $a$   $a'$  of 10 millimetres ought only to be reckoned as two, &c.; the results which have been stated were obtained in this mode.

When the apparatus was cold, a small bubble of gas was observed to have been formed above the alcohol, but it produced a difference of only two millimetres in the level of the mercury in the small tube above  $b'$ .

In order to determine the degree of heat at which the ether and alcohol are reduced to vapour in these experiments, the tubes containing these fluids were heated in oil, in which a thermometer was placed. A cylindrical glass vessel was employed to contain the oil; by these means it was easy to determine the moment at which the liquids in the tubes were reduced to vapour; it was found that ether required  $160^{\circ}$ , and the alcohol  $207^{\circ}$  of Reaumur.

The apparatus above described for determining the pressure exerted by ether and by alcohol, was also subjected to the heat of the oil bath; but a refrigerator was previously adjusted to the upper part of the small tube containing the column of air, by which the temperature of the column was constantly kept at  $18^{\circ}$ . The results, as to the degree of vaporization and to the pressure exerted, agreed with those which have been described.

When my memoir was read to the Academy, I announced that water heated in glass tubes altered their transparency, so as to prevent any observation of what took place; since that period I have found that by adding a small quantity of carbonate of soda to the water, the transparency of the glass was much less injured. By this method, I ascertained, although with some difficulty, on account of the frequent breaking of the tubes, that at about the temperature of melting zinc, water may be completely reduced to vapour in a space equal to nearly four times that of its original volume.

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## ARTICLE X.

### ANALYSES OF BOOKS.

*Transactions of the Royal Geological Society of Cornwall,*  
*Vol. II. 1822.*

WE are happy to introduce to our readers' attention this second volume of the labours of our scientific brethren in Cornwall; the papers it contains are replete with facts that are of much importance, not only in their applicability to practical pur-

poses, but as tending to elucidate some of those more difficult and interesting subjects of geological research, respecting the true nature of which there is such a diversity of opinion. A considerable portion of the volume is occupied by four papers on the temperature of mines, the contents of which we shall not advert to in the present article, as a memoir on the subject will shortly appear in the *Annals*, containing a full account of the facts which are detailed in them. Before we proceed to a brief analysis of the other papers, it may be well to state, from the preface, that the construction of a geological map of Cornwall, intended, according to the wishes of the Society, "to show not only the varieties of rocks, but also the locality and position of the principal metalliferous veins, and the cross-courses which intersect them, has already occupied much of the time and attention of some members;" but this, it is observed, "is an undertaking of immense extent and labour; and the map of the lodes in one parish (St. Just) given in the present volume, will at once show the nature and importance of the plan, as well as the time requisite for the completion of such an undertaking."

I. *On some Advantages which Cornwall possesses for the Study of Geology, and on the Use which may be made of them.* By John Hawkins, Esq. FRS. &c. Hon. Mem. GSC.

The advantages of Cornwall for the study of geology, are stated in this short paper to arise from its being a primitive country, having greater facilities for observation than any inland country can possess; and from the immense extent and importance of its mining concerns. In illustration of some remarks on the vague use of language in this science, the following statement is given.

"It is long since the attention of geologists has been drawn to the disposition observable in every distinct rock mass, to separate in a form which is in some measure characteristic of its composition; although to this day, as far as I know, no general name has been given to this phenomenon. By some who have noticed a degree of uniformity in the angles resulting from the inclination of so many plane surfaces, it has been confounded with crystallization, and by others, on rather better grounds of reasoning, with stratification; although from the first it differs in the very material point of the structure of its mass, which is not one crystal, but an aggregation of crystals; and from the other, in that of continuity. It seems to be the effect of the contraction of matter at the period of its consolidation, and is common both to the volcanic and the Neptunian formations; and this theory derives a further confirmation from the influence which the size and form of the aggregated parts of the mass have in the mode of its division.

"I know of no better generic name for this distinctive character, than that of articulation, while most of its specific forms might be expressed by the terms prismatic, cuneiform, rhom-

boidal, and tabular, of all which we are at no loss for examples in this county."

II. and III. *On the Temperature of Mines.* By Robert W. Fox, Esq. MGSC.

IV. *On the Stratified Deposits of Tin-stone, called Tin-floors, and on the Diffusion of Tin-stone through the Mass of some primitive Rocks.* By J. Hawkins, Esq. FRS. &c. Hon. Mem. GSC.

In Cornwall, Mr. Hawkins observes, the veins of tin are so rich and so numerous, that, with the exception of the alluvial beds of that metal, in the stream-works, every other mode of its deposition in the earth seems to have been disregarded. He then expresses his opinion that the same interest of capital which is obtained by working the tin-lodes, or even a greater, "might be obtained, with infinitely less fluctuation in its amount, from situations where nature has scattered her favours with a more sparing hand, but in a more equal manner:" alluding "to those mineral deposits, which our miners call tin-floors, and more particularly to those important objects of mining industry, which the Saxons call stock-works, should they be found to exist here."

These remarks are succeeded by some information on both these subjects, partly extracted from the author's own journals, and partly from the printed reports of very accurate observers. Tin-floors "are said to occur at Bal-an-uûn, in the parish of Lelant, and at Huel-grouan in the parish of Breage; and, if I am rightly informed," continues the author, "Curclaze mine, near St. Austle, belongs to this class, and merits a very particular examination." The tin-floors on the sea-cliffs of the parish of St. Just are next described; but we shall extract a more particular account of these from a subsequent paper, by Mr. Carne. Such beds are not unfrequent, it is stated, in the highest ridge of the mountains which constitute the boundary line between Saxony and Bohemia. Those at Zinnwald are on one of the highest points on the Bohemian side; "the strata here consist of a fine grained, half decomposed granite, which alternates with the tin-floors. These again consist of quartz, mica, and gneiss," in the two latter of which the tin is found interspersed together with fluor spar and wolfram. "Similar floors, composed of magnetical ironstone, tinstone, and pyrites, occur in other parts of the same range of mountains:" the most considerable of these is situated at Breitenbrunn.

What are called tin-floors at Trewidden Bal mine, in the parish of Madron, in Cornwall, are, correctly speaking, "small, very short fissures or veins, which run partially through the elvan, varying in breadth from half an inch to eight or nine inches, and so irregular and interrupted as to render it difficult to ascertain either their direction or their underlie. These small capricious veins appear frequently to diverge from a central body, and then bear some resemblance in form to the spreading roots of a tree." It is afterwards stated, from another part of the author's journal,

that these floors underlie four feet in six to the north-east, and that they occur in a white porphyritical rock. "The famous Stock-work at Altenberg (a stumbling block in the way of geologists), is nothing else than a large mass of the mountain impregnated with tinstone; occasioned by the meeting of a number of lodes and veins at this point, some of these crossing each other nearly at right angles, some bearing each other along in their crossing, and others stretching along in a parallel or nearly parallel direction. The same natural cause seems to have produced, at the same period, an infinite number of smaller veins and fissures, which run in all directions through the rock, so that many parts of the Stock-work bear a striking resemblance; on a larger scale, to the veined varieties of common marble. The veins here are chiefly of quartz, and are united to the rock in such a manner as to indicate their coeval formation. It is remarkable, however, that the tin lies for the most part dispersed through the latter, and this circumstance has given occasion to the very singular mode of working the mine; for as no profit attended the driving upon regular lodes, and the mass of rock was unequally impregnated with tin, the greatest degree of irregularity appears to have marked the progress of the excavations from their commencement. . . . The rock itself, which composes the Stock-work, is a variety of porphyry, the mass being a fine mixture of chlorite earth and clay, in which the grains of felspar and quartz are scarcely perceptible."

To this follows some particulars of the Stock-work at Geyer, in Saxony; and, after adverting to the probable existence of similar veiniferous masses of rock in Cornwall, Mr. Hawkins concludes his paper with the following advice as to the proper mode of working them.

"I would suggest the expediency of removing the whole mass of impregnated rock from the surface downwards, in successive terraces or levels; most earnestly recommending the immediate adoption of the Saxon process of stamping and dressing. No man who has witnessed this process, can forbear reprobating our own, as rude, wasteful, and inefficient; for if, with any truth, it may be said, that in the art of mining we are inferior to our continental neighbours, we are so in a most shameful degree in the department which I have just mentioned."

V. *On the relative Age of the Veins of Cornwall.* By Joseph Carne, Esq. FRS. MRSA. Hon. MGS. MGSC.

In the commencement of this extended and elaborate communication, after some remarks on the number and variety of the mineral veins of Cornwall, and on the diversity of sentiment respecting the formation of veins, Mr. Carne observes, "with respect to their comparative or relative age, I apprehend all parties (except those who hold that all veins are contemporaneous, and were formed at the same time as the containing rocks), are agreed on this important principle; that a vein which is inter-

sected, or traversed, by another vein, is older than the vein by which it is traversed." On this principle, he proceeds, "I would make a humble attempt to ascertain the relative age of the veins of Cornwall."

By a *true vein*, Mr. C. understands "*the mineral contents of a vertical, or inclined fissure, nearly straight, and of indefinite length and depth.*" To the distinctive characters of *contemporaneous veins*, he adds the following: "When these veins meet each other in a cross direction, they do not exhibit the heaves, or interruptions, of true veins, but usually unite. In a multitude of contemporaneous veins, some may appear to be heaved; but the apparent heave seldom affects more than one vein, and it is, in general, easy to perceive that what appear to be separate parts of the same vein, are different veins which terminate at or near the cross vein. When they meet with true veins, they are always traversed by them."

"With all these descriptive particulars, however, it is frequently very difficult to distinguish true from contemporaneous veins;" and, for this reason, Mr. Carne arranges the veins of Cornwall into three orders, of Contemporaneous, Doubtful, and True veins.

The first order comprises veins of the following substances: *Granite in Granite, Felspar, Mica, Shorl, Shorl-rock, Quartz, Actynolite and Thallite, Arinite, Garnet-rock, Prehnite, Chlorite, Ironstone, or Irestone* (compact hornblende, with chlorite and quartz), *Serpentine, Greenstone, Asbestos, Agate, Calcareous Spar in Limestone, Jasper, Opal, and Fluor Spar.*

The second order commences with "*granite veins in slate,*" of which the following account is given: "These have been discovered in no less than fourteen places on the coast of the western part of Cornwall, between Ponthleven and St. Ives Head, viz. (beginning at the most eastern point). 1. About half a mile eastward of Trewavas Head, in the parish of Breage. 2. On the eastern side of Portcove Cove, in the same parish. 3. On the western side of the same Cove. 4. At St. Michael's Mount. 5. At the village of Mousehole. 6. At Rosemodris, three miles south-west of Mousehole. 7. At Carnsilver, about half a mile west of Rosemodris. 8. Near Whitesand Bay, between the Land's End and St. Just. 9. At Porth Just, joining Cape Cornwall in the south. 10. At Polladan Cove, joining the same Cape on the north. 11. At Chycornish Carn, near Botallack. 12. At Pendeen Cove, in St. Just. 13. At Polmear Cove in Zennor. 14. At the Cove north of Zennor church."

"The relative ages of those veins has been a subject of as much discussion as almost any point in geology; especially as it appears to involve several other points, which are deemed, by different parties, of essential consequence to their systems. I believe the more they are examined, the more difficult it will be found to form any consistent theory respecting them: in order,

however, to be convinced of this, it is necessary to examine them at every place where they have been discovered; for so different are their appearance, and attendant circumstances, in different parts, that a very plausible theory made with reference to the veins of one spot only, will be found quite inconsistent with those of another. From the whole, however, the following facts may be collected:

"1. They occur only at, or near, the junction of the granite and slate.

"2. They are not metalliferous.

"3. They have no general direction or position. At Mousehole and Polladan Cove, they run east and west: those at St. Michael's Mount, and some at Porth Just, run west-north-west and east-south-east; those at Polmear Cove, north and south. Some are quite vertical, as at Portcue, Rosemodris, and Polmear Cove: those at St. Michael's Mount and Mousehole are nearly so: others are inclined at different angles, as at Trewavas Head, Porth Just, and Chycornish Carn; and others quite horizontal, as at Polmear Cove: at the latter place, indeed, they may be seen in almost all positions.

"4. Their direction is usually as straight, and their size as regular, as those of true veins; but in some cases, they become smaller as their distance from the granite mass increases.

"5. Their greatest length has never been ascertained: some at Rosemodris may be traced in the slate nearly 200 feet, and are then lost in the sea.

"6. The granite of the veins generally appears different from that of the main body; it is of much smaller grain: it contains a much larger proportion of quartz, and very little mica; sometimes, indeed, no mica at all.

"7. The slate which is contiguous to the veins becomes almost imperceptibly changed from clay-slate to mica-slate, and sometimes has even the appearance of gneiss.

"8. The slate which is close to the veins is frequently much harder than that which is more distant from them, and its texture is, in general, not so slaty.

"9. At St. Michael's Mount, and Polmear Cove, the veins may be traced to the granite mass, with which they appear to be in complete union, and to form one body, losing entirely their character as veins. Whether the other veins unite with the granite mass or not, has not been ascertained, as the point of junction is seldom accessible, or even visible.

"10. At Carn Silver, one of the veins may be traced from the slate *into* the granite mass. This is the only instance which I have discovered of a granite vein penetrating both the slate and the granite.

"11. Some veins (as at Carn Silver, Chycornish Carn, and Pendeen Cove) are closely connected with the slate, and the two bodies appear intimately united, and inseparable: in fact,

they appear contemporaneous. Others (as at Trewavas Head, the western side of Portcue Cove, Rosemodris, and Polmear Cove), are so easily separable from the slate, and have walls so distinct, that, under any other circumstances, they would be taken without hesitation for true veins.

"12. Fragments of slate are visible in several of the veins, as at Trewavas Head, St. Michael's Mount, Mousehole, Porth Just, &c. I have not observed them in the main body of granite.

"13. At Mousehole, and at St. Michael's Mount, the slate is intersected by numerous small quartz veins, some of which are traversed by the granite veins; others, on the contrary, traverse and leave both the granite veins, and the other quartz veins.

"14. At Mousehole, and Whitesand Bay, where a junction of the main bodies of granite and slate takes place, they appear, at some points, so completely intermixed, as almost to exclude any other idea than that of contemporaneous formation; although, at other points, the junction is distinct and regular.

"15. In most other places where the junction occurs, the slate reposes on the granite, without any appearance of a dislocation, or disturbance of the strata, particularly at Portcue Cove, Carn Silver, and Polmear Cove."

After having thus described these curious objects of geological inquiry, Mr. Carne briefly examines the various opinions which have been entertained respecting their origin and formation. He then proceeds to describe the remaining veins of the doubtful order; viz. *veins of steatite, veins of calcareous spar, elvan courtet, and veins of oxide of tin.*

(To be continued.)

## ARTICLE XI.

### *Proceedings of Philosophical Societies.*

#### ROYAL SOCIETY.

*Feb. 20.*—The reading of a paper, entitled, *Experiments on the Velocity and Force of Sound*, made at Madras, by John Goldingham, Esq. FRS. was resumed and concluded.

Mr. Goldingham commenced his paper with an account of former experiments, calculations, and assertions, relating to the velocity and force of sound, upon the diversities of which, he stated, the experiments he was about to detail would probably throw some light. He had made some experiments in the years 1793 and 1796, but the results of them had not been published; and more favourable circumstances for the prosecution of the inquiry had since occurred. The experiments and attendant

observations on the subject, which Col. Beaufoy, in the *Annals of Philosophy* for Sept. 1814, had recommended to be made in England, Mr. G. had instituted at Madras.

The observatory at Madras is peculiarly well adapted for the requisite experiments and observations: it is situated in  $80^{\circ} 17' 21''$  E. long. and  $13^{\circ} 4' 9.1''$  N. lat. between Fort St. George and the artillery cantonments at St. Thomas's Mount; being distant from the former 13,932½ feet, and from the latter 29,547 feet. These distances were determined by a precise measurement, founded on part of Col. Lambton's Trigonometrical Survey.

At the Fort, as is usual in fortified places, a gun is fired morning and evening; in the morning at day-light, and in the evening at eight o'clock; at the artillery cantonments, a gun is fired at sun-rise and at sun-set. The guns are both 24 pounders, are each charged with eight pounds of powder, and are pointed, though not exactly to the observatory, yet very nearly in its direction. The distance of the Mount gun from the observatory being about double that of the Fort gun, a good opportunity was afforded of determining whether sound travels at the same rate through paths of different length.

The observations were made with Arnold's chronometers, usually making 100 beats in 40 seconds; sometimes there were three observers, the author and two Bramin assistants at the observatory, but more commonly there were only two. Each of them began to count on seeing the flash of the gun, and ceased on hearing its report; and then wrote down his observation without communicating it to the other; the observations were afterwards given to Mr. G. for registration. The states of the barometer, thermometer, and hygrometer, the direction of the wind, and the general state of the weather, were noticed at the time of each observation.

Mr. Goldingham gave, in eleven tables, the results of the observations thus made; from which it appears that the mean velocity of sound, by the observations on the Fort gun, is  $1142\frac{1}{8}$  feet per second; and by those in the Mount gun  $1142\frac{1}{6}$  feet; the mean of both series being  $1142\frac{1}{6}$  feet per second: so that the mean rate at which sound travels, as deduced from these observations, is, almost precisely, that which Newton and Halley have assigned.

Mr. Goldingham's observations likewise show that the velocity of sound is considerably affected by the different states of the atmosphere and of the weather, and by the wind, contrary to what has been asserted. The close agreement of the mean velocity by the Mount gun with that by the Fort gun evinces that sound does travel equably through paths of different length. The last table showed the mean velocity at Madras for each month; the velocity increases to a maximum at the middle of

the year, being then 1164 feet per second: the minimum is 1099 feet.

At this meeting, a paper was also read, on the Question as to the Evolution of Heat during the Coagulation of the Blood, by Charles Scudamore, MD. &c. (Communicated by the President.)

A part only of this paper was read, and the remainder postponed to a future meeting.

Feb. 27.—The reading of Dr. Scudamore's paper was resumed and concluded.

In this paper, Dr. Scudamore examined the results as to this subject, which had been obtained by Mr. John Hunter, Dr. J. Davy, and the late Dr. Gordon, of Edinburgh, and expressed his belief that the experiments which he had made upon it would explain the reason of their discrepancies. He then described those experiments; in some of them the successive temperatures of the coagulating blood were compared with those of a solution of starch in water while cooling, and also with those of pure water under the same circumstances; and he inferred from the whole, that a slight evolution of heat does take place during the coagulation of the blood. It commences when the fibrin begins to concrete, but continues throughout the coagulation; in some instances the temperature rose  $1^{\circ}$ . Some of the discordancies in the statements of former experimenters appear to have arisen from the difference in temperature of different parts of the same portion of coagulating blood.

The following paper was also read at this meeting: On the Double Organs of Generation in the Lamprey, Conger Eel, Common Eel, and Barnacle, which impregnate themselves, and in the Earth Worms, the Individuals of which Class mutually impregnate each other. By Sir Everard Home, Bart. VPRS.

The author of this paper had formerly shown that the tere-dines were hermaphrodites, subsequently that such also was the nature of the lamprey; and had recently ascertained that the conger and the common eel were similarly characterized. He was inclined to adopt the opinion of the President of the Society, that the last mentioned animals are in reality the same species, their difference in size and colour arising from the circumstance, that the one lives in salt water, and the other in fresh. To determine the fact, an experiment had been instituted by Sir Humphry, and was now going on in Cornwall: young eels, it had been found, soon died in salt-water, but an old one did not appear to suffer any inconvenience in it, and had begun to turn green.

These remarks were succeeded by a particular account of the double organs of generation of the animals mentioned in the title of the paper, and of their mode of copulation, with corrections of the mistakes that former observers had made when examining their anatomy: the species of barnacle examined was the *Lepas anatifera*.

A series of microscopical drawings by Messrs. Bauer and Clift illustrated the details in this paper.

*March 6.*—On a new Phenomenon of Electromagnetism, by Sir H. Davy, Bart. PRS.

The experiment described in this paper had been first made by the author about fifteen months ago; but had been repeated by him, very recently, through the kindness of Mr. Pepys, with a battery consisting of two plates, each containing 100 square feet, which had been constructed, under the direction of that gentleman, for the London Institution; and of which, Sir Humphry hoped, Mr. Pepys would communicate a description to the Society. The experiment, however, might be made with a pair of plates each containing three or four square feet only.

Sir Humphry had conceived, immediately after the publication of Mr. Faraday's ingenious experiments on electromagnetic rotation, that if mercury could be made to revolve by electromagnetism, some new light might be thrown upon the subject, as the motions of that substance could not be affected by the disadvantages attending mechanical suspension. On immersing, accordingly, the two wires of a battery in mercury, and presenting a magnet to them, either above or below, the mercury was made to revolve around the wires. From the appearance of the motion, Sir Humphry was induced to believe that it could not be owing to any simple action, but that it must be the result of a composition of forces; and that some kind of motion would be produced in mercury by the agency of electricity alone. In order to determine this, he covered the surface of the mercury with weak acids, and with finely divided matter, as the seed of lycopodium, the white oxide of mercury, &c.; but no distinct effects could be observed. It then occurred to him, that, from the circumstances of this experiment, the motion, if any took place, must be chiefly confined to the lower surface of the mercury; and he therefore reversed it. Two copper wires, one-sixth of an inch in diameter, and carefully polished at their extremities, were passed, perpendicularly, through the bottom of a glass basin, and made non-conductors by wax, except at their extremities. Mercury was then poured in, to the height of one-tenth of an inch above the wires, and, upon connecting them with the battery, it was thrown into violent motion, being raised, over the wires, into conical elevations of one-tenth or one-twelfth of an inch, from which waves flowed in all directions. Upon bringing a magnet over the wires, the conical eminences were depressed, the circular motion ceased, and currents were produced in the mercury in opposite directions; and, upon its nearer approach, the eminences were converted into vortical depressions.

That the motion given to the mercury by the agency of electricity, as just described, did not arise from the heat produced, was experimentally determined; when the large battery was

employed, the whole apparatus was converted into a thermometer, and, when the motion commenced, there was no increase of volume in the mercury; nor could the motion have been produced by electric repulsion.

Sir H. Davy would not enter into the conjectural part of the subject any further than to state, that the experiments he had described were unfavourable to the idea, that the phenomena of electromagnetism were produced by the motions of a single fluid; and that the motion communicated to mercury by electric agency seemed to be produced by a fluid, moving either in right lines or in undulations from each wire as a centre. He concluded by observing, that he thought it right to state a circumstance, which, though known to many Fellows of the Royal Society, was not generally understood: this was, that we owe to the sagacity of Dr. Wollaston, the first suggestion of electromagnetic rotation; and that, had not an experiment on the subject, made by Dr. W. in the laboratory of the Royal Institution, and witnessed by Sir Humphry, failed, merely through an accident which happened to the apparatus, he would have been the discoverer of that phenomenon.

March 13.—On Fluid Chlorine, by M. Faraday, Esq. (Communicated by the President.)

Previous to the year 1810, it had been supposed that the crystals which form in aqueous solutions of chlorine at temperatures below  $40^{\circ}$  were pure chlorine, but Sir H. Davy then showed that they were an hydrate of that substance. During the late cold weather, Mr. Faraday had made some experiments on this hydrate, and an analysis of it, an account of which will be found in the 15th volume of the Journal of Science; it consists of 27.7 chlorine, and 72.3 water, or, nearly, of one proportion of the former to ten of the latter. The President of the Royal Society suggested, that interesting results might be obtained by examining the decomposition of this compound under pressure, and requested the author to make some experiments on the subject.

Some of the crystals, dried as much as possible by being pressed in bibulous paper, were introduced into a closed glass tube, the open extremity of which was then hermetically sealed. When this tube was immersed in water at the temperature of  $60^{\circ}$ , no alteration took place in the crystals; but when it was placed in water at  $100^{\circ}$ , they were decomposed, and two fluids resulted; one of a pale-yellow colour, and apparently water; the other of a deeper greenish yellow; resembling chloride of azote. This did not mix with the water, but when the tube was cooled to  $70^{\circ}$ , they again crystallized in union. Above the fluids there was an atmosphere of chlorine, the intensity of the colour of which indicated that it was of great density. Upon dividing the tube, a report took place, as of an explosion, the yellow fluid instantly disappeared, and a strong atmosphere of chlorine was produced.

the author at first conceived that the yellow fluid might be a new hydrate of chlorine; but he found that it was also produced by introducing a portion of the gas, dried over sulphuric acid, into a glass tube to which a stop-cock was attached, and then forcing in air by means of a condensing syringe, at the same time applying cold; so that in reality it was *fluid chlorine*.

This fluid may be distilled out of the water by means of a spirit-lamp; at whatever temperature it is produced, it is perfectly limpid and fluid, and it remains so at 0°; it is very volatile; and when exposed at the common pressure of the atmosphere, a portion volatilizes, the remainder being cooled down so low by the evaporation, as to preserve, for a time, its fluidity. By comparing the weights of apparently equal volumes of water and chlorine, the specific gravity of the latter seemed to be 1.33; and that this estimate cannot be far from the truth, the manner in which the chlorine lies in the water is evinced by.

A note had been annexed to this paper by Sir H. Davy, in which he stated, that Mr. Faraday's experiments had induced him to think, that other gases beside chlorine might be reduced to a liquid state by the pressure of their own atmospheres, when evolving from substances containing them; and that, in order to verify this conjecture, he placed sulphuric acid and muriate of ammonia in different parts of a glass tube, and, after sealing the tube, brought them into contact; when an orange-coloured fluid was produced, which was muriatic acid. Although the two substances employed were as pure as they could be obtained, yet the orange-colour of the resulting liquid, he considered, might be owing to iron. Sir Humphry concluded with some remarks upon the superiority which this method of condensing the gases possesses over those of mechanical compression and the application of cold.

#### LINNEAN SOCIETY.

*Feb. 4.*—The reading of a paper, on *Lansium* and other Malayan Plants, by Dr. William Jack, FLS. which had been commenced at the preceding meeting, was resumed and concluded; and the following paper was read: Catalogue of the Land and Freshwater Shells found in the County of Suffolk, by the Rev. Revett Shephard, FLS. Dr. Maton and Mr. Rackett having given the habitats of the land and freshwater shells in the midland and western counties, in vol. viii. of the Linnean Transactions; the purpose of this paper was to supply those of Suffolk and Essex. The author adopted the Linnean arrangement, the genera of which, he was of opinion, were the best for land and freshwater shells; though, at the same time, he expressed his approbation of M. Draparnaud's work, observing that his genera, considering them as subdivisions of the Linnean, were *secundum naturam*.

*Feb. 18.*—The following papers, by Major-Gen. Hardwi FRS. and FLS. were read, all of which were illustrated  
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beautiful drawings made by Indian artists :—Description of the *Sciurus sagitta* of Linnæus ; Account of the *Buceros galeatus* of Shaw ; Description of a new Species of *Phasianus* ; Description of the supposed Female of *Phasianus cruentus* ; Description of a small *Antelope*, a Native of the Himalaya Range, and of the Mountains in the Nepal Frontiers, called by the Natives *Goral* ; Description of an Insect which appears to be a new Species of the *Scutigera* of Latreille, or *Cermatia* of Leach.

March 4.—Description of the Skeleton Head of the long-snouted Alligator of the Ganges, or *Lacerta Gangetica* of Linnæus, presented to the Linnean Society, together with the entire Skeleton of a young Subject of the same Species, by Major.-Gen. Thomas Hardwicke, FRS. &c.

At the same meeting was read, A Description of a Serpent hitherto supposed of the Genus *Boa*, and the *Boa Phrygia* of Shaw.

March 18.—The following papers were read :

Observations on the generic Character of *Locusta*, with the Description of a remarkable Species. By the Rev. Lansdown Guilding, BA. FLS. &c.

The Natural History of *Phasma ramulus*. By the same.

Observations on the Genus *Asalaphus*, with the Description of a new Species. By the same.

On the Nature of the Marine Production commonly called *Flustra arenosa* ; considered by Ellis and Gmelin as belonging to the Order Vermes Zoophyta ; but rather to be considered as the Matrix of *Nerita glaucina*, by John Hogg, Esq. BA. FLS. St. Peter's College, Cambridge.

Description of the Taille Bat, by Major-Gen. Hardwicke.

Description of *Fuvera agavephylla*, the *Agave Cubensis* of Linnæus and Jacquin, and the *A. Mexicana* of Lamarck, by M. Felix de Avelear Protero, Prof. Bot. Coimb.

On the Generic and Specific Characters of the *Chrysanthemum Indicum* of Linnæus, and of the Plants called Chinese Chrysanthemums, by Joseph Sabine, FRS. FLS. &c.

#### GEOLOGICAL SOCIETY.

Jan. 3.—A paper was read, on the Beds of Limestone and Clay of the Ironsand of Sussex, by Gideon Mantell, Esq. MGS. and Charles Lyell, Esq. MGS.

Mr. Mantell traces the direction of the calcareous beds connected with the ironsand formation in the county of Sussex, and enumerates their several localities ; to which he subjoins drawings and descriptions of some of the most remarkable fossils found in the limestone of Tilgate Forest. He then adds a letter addressed to him by Mr. Lyell, containing an account of the strata in the neighbourhood of Horsham, with a section of the quarry of Stammerham, and with remarks on the phenomena presented by the grooved and furrowed surfaces both of the *calcareous* and *sandstone* beds of that country.

A Notice was then read, accompanied with Specimens, by Charles Daubeny, MD. FRS. and MGS. Professor of Chemistry at Oxford, illustrative of the Strata cut through in the Seven Rakes Mine, near Matlock, Derbyshire.

After describing the qualities of the strata of limestone and toadstone, their dimensions and connexions with each other, and the minerals which they contain, both in veins and regularly disseminated through the mass; Dr. Daubeny concludes with general observations on the phenomena which they present. He considers that there would be great difficulty in reconciling the facts there observed with that theory which refers to an igneous origin the formation of the toadstone.

*Feb. 21.*—Two letters were read, communicated by the President, addressed by Joseph Byerly, Esq. to B. Fayle, Esq. containing some notices on the Geology of Sierra Leone.

At Sierra Leone and in the immediate neighbourhood, sienite, porphyry, and basalt, are the predominant rocks.

*Feb. 21 and March 7.*—A paper was read, entitled, "Notes on the Geography and Geology of Lake Huron, including a Description, accompanied by Drawings, of new Species of Organic Remains," by John Bigsby, MD. MGS.

In this paper, the author enters in some detail into a geographical and geological description of the coast and islands of Lake Huron in North America. The greater part of the northern shore is composed of primitive rocks; while the Manitouline islands which stretch nearly across the centre of the lake, with the southern coast, are entirely composed of secondary calcareous formations. To this paper is subjoined a map of Lake Huron, and plates illustrative of the organic remains which are contained in great abundance in the limestone rocks.

*March 21.*—A paper was read, entitled, "Observations on the Belemnite," by J. S. Miller, Esq. ALS. communicated by the Rev. W. D. Conybeare, MGS.

The author commences this paper with an historical sketch of the various opinions which have been entertained with regard to the belemnite, and of the works of those naturalists who have treated of that fossil. He enumerates the various names which ignorance or superstition assigned to it in the earlier periods; and, lastly, the almost equally discordant and imperfect theories which have been successively advanced on the same subject by writers of a more recent date. Mr. Miller then offers his own opinion on the original structure and nature of this organic body, and adds the reasons and the experiments which have led him to his conclusions. He considers the belemnite to have been an animal of the Cephalopodous division of the mollusca, inhabiting a fibrous spathose conical shell, divided into chambers connected by a siphunculus, and beyond which shell extended a protecting guard or sheath. Mr. Miller refers the internal radiated texture to its original organic structure, and not to any subsequent process of crystallization. To this paper is

subjoined an enumeration and description of the various species of belemnites, accompanied by plates illustrative of their form and structure.

#### ASTRONOMICAL SOCIETY.

The Third Anniversary Meeting of this new association, which is rapidly advancing in importance and prosperity, was held at the Society's Apartments, No. 55, Lincoln's Inn Fields, on Friday the 14th of February last. At this meeting, a report from the Council was read, stating, among other matters, that the Society now consisted of 187 effective members and associates, including the greater number of the eminent astronomers of Europe; that the funds were in a flourishing condition; and that great progress had been made in establishing an astronomical library, which would shortly be opened for the use of the members. A just tribute of respect to the memory of the lamented President of the Society, was likewise paid in this report, as well as to those of Sir Henry Englefield, Dr. Hutton, M. Delambre, M. Tralles, and several other valuable members, of whom the Society had been deprived by death during the past year; and it concluded by calling upon the members and associates, generally, to promote the objects of the Institution by every means in their power, and particularly by the transmission to the Society of such papers and observations as might become useful by their registry and comparison. A minute investigation of the heavens was also recommended to be effected by dividing them into small portions, each to be examined by an individual member.

The following officers were then elected for the ensuing year:

*President.*—Henry Thomas Colebrooke, Esq. FRS. and FLS.

*Vice-Presidents.*—Francis Baily, Esq. FRS. and FLS.; Major Thomas Colby, Roy. Eng. LLD. FRSL. and E.; Davies Gilbert, Esq. VPRS. and FLS.; Sir Benjamin Hobhouse, Bart. FRS.

*Treasurer.*—Rev. William Pearson, LLD. FRS.

*Secretaries.*—Charles Babbage, Esq. MA. FRSL. and E.; John Millington, Esq. MGS. Prof. Mech. Phil. Roy. Inst.

*Foreign Secretary.*—J. F. W. Herschel, Esq. MA. FRSL. and E.

*Council.*—Capt. F. Beaufort, RN. FRS.; George Dollond, Esq. FRS.; Benjamin Gompertz, Esq. FRS.; Stephen Groombridge, Esq. FRS.; James Horsburgh, Esq. FRS.; Daniel Moore, Esq. FRS. FSA. and FLS.; Peter M. Roget, MD. FRS.; Major-Gen. John Rowley, Roy. Eng. FRS.

*March 14.*—Several new members were proposed, and others elected. A considerable number of astronomical books were presented to the library of the Society; and two papers were read. The first was a demonstration by means of a functional equation, of the result of two forces acting upon a particle of *matter*; communicated by Dr. Mickleham, late Professor of *Astronomy* in the University of Glasgow. The second was

entitled, "On the Results of Computations relative to the Parallax of  $\alpha$  Lyrae from Observations made with the Greenwich Mural Circle, compared with those of Dublin; by Dr. Brinkley, Prof. of Astronomy, Trin. Col. Dublin:" this was a highly interesting paper.

## ARTICLE XII.

### SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

#### I. *Mr. Faraday's Liquefaction of the Gases.*

WE have already described, at p. 304, in our report of the proceedings of the Royal Society, the means by which Mr. Faraday has succeeded in reducing chlorine to the liquid state; and we are happy to announce that he has also succeeded, by analogous modes of operation, in liquefying the following æriform substances: euchlorine, nitrous oxide, sulphurous acid, sulphuretted hydrogen, carbonic acid, and cyanogen. All the liquids thus produced are colourless, with the exception of euchlorine; and all are perfectly fluid, and highly volatile.

#### II. *Re-opening of Mr. Sowerby's Museum.*

WE feel much satisfaction in announcing that the late Mr. Sowerby's Museum of the Natural Productions of Great Britain, at No. 2, Mead-place, Lambeth, is again opened for the inspection of his friends and the lovers of natural history, every Tuesday from eleven till four o'clock, under the direction of his sons and executors, James de Carle Sowerby, FLS. and Charles Edward Sowerby.

#### III. *Prof. Daubeny on Rocks that contain Magnesia.*

Dr. Daubeny having withdrawn his paper on this subject from the Royal Society, in the hope of its appearing at some future time, in a more complete state, has requested us to correct two mistakes in our report of it, published in the *Annals* for February, p. 150.

We have there represented Dr. D. as saying, that the presence of magnesia in the oolites "has not been ascertained with certainty;" whereas, he states, there are several specimens of them in the catalogue at the end of his paper, which are mentioned as containing that earth. In the second place, Dr. Daubeny observes, we have transferred to his mode of separating lime from magnesia, the objections which he had alleged against preceding methods, and from which he imagines his own to be exempt; "for it will be seen by reference to my paper in the *Edinburgh Philosophical Journal*," he continues, "that the difficulty of driving off the ammoniacal salts and water originally present, without at the same time decomposing the magnesian sulphate, was my inducement for substituting that scheme of analysis which is inserted in the *Annals*, the peculiarity of which consists in its enabling the operator to calculate the quantity of magnesia by merely ascertaining the amount of the sulphate of lime obtained, deducing from thence that of the lime, and comparing the result with the quantity taken up originally by the acetic acid."

IV. *On the Question as to the Existence of Metallic Veins in the Transition Limestone of Plymouth.* By Mr. Prideaux, and the Rev. Richard Hennah.

(To the Editor of the *Annals of Philosophy*.)

SIR,

Plymouth, Feb. 15, 1823.

Observing some doubts expressed in your last number of the accuracy of Mr. Hennah's statement, that our "lime rocks contain no metallic veins;" I am induced to offer you some reasons for coinciding fully in his opinion.

I believe I speak much within compass in saying, that the face of that rock is open to an extent of above four miles; and that more than half of it has been in regular work within the last five years. For nearly the whole of this extent we get to the upper surface, and in several different places we trace its repose on, and alternation with, the clay slate, for a considerable distance. The cliffs and quarries are from 50 to 150 feet high, and cut the strata at all angles. And the limestone itself forms only the hilly boundary of the sea, nine-tenths of it being exposed.

I think, therefore, it is almost impossible that a metallic vein, however minute, existing in it, should have escaped detection.

The specimen you describe, as found at Cat-Down, looks like a fact in opposition to this conclusion; but I have several times traced the whole course of the extensive quarries in that situation from top to bottom (though my opportunities are not so frequent as Mr. Hennah's); and although varieties of crystallization, and not metallic specimens, were my pursuit, the latter could hardly have escaped me; nor have the men employed in the quarries ever mentioned such appearances in my hearing.

That the mines of Tavistock yield specimens of crystallized carbonate of lime, bearing abundant marks of their metallic connexion, and much like the one in question, you are doubtless aware. I should hardly suppose, therefore, that you could have been deceived in it, though the little mineral dealers here are likely enough to make the mistake. But the considerations above related appear to me so decisive that were I to find a detached specimen, such as you describe, at Cat-Down, any where but *in situ*, I could not believe it to be the produce of those quarries, where I have seen nothing to which it appears at all related.

I am, Sir, your most obedient,

JOHN PRIDEAUX.

SIR,

Citadel, Plymouth, March 8, 1823.

I perused, a few days ago, in the 26th number of the *Annals of Philosophy*, a quotation from a recent publication of mine, on the "Lime Rocks of Plymouth," in which I state that it is my opinion, that they contain *no metallic veins*, which quotation is accompanied with an account of a particular specimen of quartz, &c. making part of a collection said to be from our quarries; and with a suggestion, that the circumstance merited further inquiry. Being myself desirous to promote any investigation, which has for its object the correction of any error which I may have inadvertently fallen into, or the establishment of any point which I believe to be true, I hope your correspon-

dent will excuse me for requesting you to permit me, through the same medium, to express my doubts as to the accuracy of his information respecting *the place* from whence the specimen he so minutely describes originally came.

First, I would ask, whether he himself met with the specimen in question at the quarry? If so, in which of the quarries was it found? As there are many at Cat-Down, all of which I have frequently and carefully examined, without ever finding in them, or in any others, from the Dock Yard to the Ferry House at Cat-Down, *quartz in mass*, independent of galena, and the other substances composing the stone.

Secondly, if your correspondent did not find the specimen himself, but purchased it with others, I would beg of him to compare it with such as he *knows to be the legitimate produce of our lime rocks*; and I think he cannot fail of remarking the great dissimilarity, if not the total want of any *family likeness* between them. And then let him reflect how little dependance is to be placed on the veracity of those who generally have such articles for sale; and whose duplicity, art, or ignorance, I have daily opportunities of detecting.

I would further state, that I have in my possession a specimen so similar to the one above-mentioned, that I should almost be tempted to say, that both came from the same place. Mine is certainly from one of the mines in the vicinity of Tavistock, where the mixture of quartz, galena, &c. &c. is frequent and often curious.

I will only add my sanguine expectation that this will meet with a candid reception, and an early place in your *Annals*, and conclude in the last words of my own publication; "that it will give me real pleasure at all times to receive any information from others, whose personal knowledge and actual researches may enable them to throw any new light on the subject of these pages."

I remain, Sir, your obedient servant,

RICHARD HENNAH.

\* \* The Editor is enabled to state, in reply to the foregoing letters, and in order that the question to which they relate may be decided, that the small specimen described in the notice on the subject, inserted in the *Annals* for February last, was purchased in the latter part of September, 1822, with specimens of limestone and of stalactite unquestionably obtained from the Plymouth rocks, of Edmund Moss, quarryman, at Cat-Down. The author of the notice certainly has not mistaken *calcareous spar* for *quartz*, as a paragraph in Mr. Prideaux's letter implies him to have done.

#### V. On the Depression of the Barometer in Dec. 1821.

By Mr. A. Edwin.

The following observations on the extraordinary depression of the barometer in Dec. 1821, were made in Owen's-row, near Islington, by Mr. A. Edwin; and have been communicated by him, in compliance with the request of Prof. Brandes, inserted in the *Annals* for October last. The basin of the barometer is situated about 40 feet above the bed of the New River, which runs before the house.

On Dec. 11, the barometer was at 30.13 inches, and thence gradually descended, with some slight intermediate elevations, until it fell to 28.74 inches on the 24th at 8 a. m.

1821.	Time of ob- servation.	Baron.	Ther- (out).	Wind.	Strength.	R a.	Weather.
Dec. 24	8 a. m.	28-74	43	SW	Smart gusts.	—	Cloudy and showery; a gale, with heavy rain between 6 and 7 a. m.
	5 p. m.	28-21	44	SE	Brisk.	—	A faint rainbow appeared at 10 a. m.
	8½ p. m.	28-07	—	—	Strong gusts.	1-164	Cloudy, with gentle rain; 6 to 8 p. m. a gale, with very heavy rain.
	11 p. m.	27-03	47	—	Brisk.	—	Cloudy, with steady rain till midnight.
25	12½ a. m.	27-88	47	—	Calm.	—	Cloudy and mild.
	1 a. m.	27-87	46½	—	Ditto.	—	Ditto.
	2 a. m.	27-87	46½	—	Ditto.	—	Ditto.
	3½ a. m.	27-86	—	—	Ditto.	—	A few stars appeared in the SE.
26	4 a. m.	27-84	—	—	Ditto.	—	Cloudy.
	5 a. m.	27-84	—	NW	Smart gusts.	0-041	Cloudy, with heavy showers of rain.
	6 a. m.	27-90	—	Ditto.	Ditto.	—	Cloudy, with heavy rain; 7, cloudy.
	8 a. m.	28-01	39	NW	Ditto.	—	Cloudy.
27	5 p. m.	28-33	39	W	Gentle.	—	Clear.
	11 p. m.	28-42	33	—	—	—	Very clear.
	8 a. m.	28-32	35	E	—	—	Cloudy.
	5 p. m.	28-27	40½	E	Gentle.	0-353	Cloudy, with gentle rain; wet afternoon.
28	11 p. m.	28-38	32½	—	Brisk.	—	Clear.
	8 a. m.	28-76	37	W	Gentle.	—	Chiefly cloudy.
	5 p. m.	28-85	41	SW	Strong gusts.	0-325	Cloudy, with steady rain; heavy showers of hail and rain at 11 a. m. and 4 p. m.
	11 p. m.	28-90	40½	—	Brisk.	—	Cloudy.
29	8 a. m.	28-71	41½	S	Smart gusts.	—	Chiefly clear.
	5 p. m.	28-15	46	S	—	0-595	Cloudy and showery; a heavy gale, with rain and sleet at 2 p. m.
	11 p. m.	28-07	45½	NW	Very brisk.	—	Cloudy; a heavy shower of rain.
	8 a. m.	28-07	43	—	Brisk.	—	Cloudy.
30	5 p. m.	28-48	45	W	Strong gusts.	0-155	Cloudy, with steady rain.
	11 p. m.	28-71	45½	—	—	—	Cloudy.
	8 a. m.	28-38	36	N	Very brisk.	0-113	Very clear; stars remarkably bright and numerous.
	11 p. m.	28-79	33	NW	Gentle.	—	Partially cloudy, with a light fog.

The mean height of the barometer for the month was 29-212 in. and that of the thermometer 49-290°.

VI. *Alkohometrical Application of the Thermometer.*

In the *Annals* for October last (p. 395), we gave an account of M. Groening's discovery on this subject; and we now present a tabular view of the results of his experiments:

Spirits of wine holding, according to Tralle's alcoholometer at a temperature of 60° Fahrenheit.	Temperature of the boiling liquid, according to Fahrenheit.	Temperature of the vapours, according to Fahrenheit.
--	---	--

Per cent. of alcohol.

95° .....	173·18° .....	170·37°
90 .....	174·31 .....	171·5
85 .....	174·87 .....	172·06
80 .....	175·46 .....	172·63
75 .....	176·56 .....	173·75
70 .....	177·71 .....	174·87
65 .....	178·83 .....	176·0
60 .....	179·40 .....	176·56
55 .....	179·97 .....	177·12
50 .....	181·62 .....	178·83
45 .....	182·19 .....	179·40
40 .....	183·31 .....	180·5
35 .....	185·0 .....	—
30 .....	187·25 .....	185·0
25 .....	189·30 .....	187·25
20 .....	192·40 .....	190·62
15 .....	195·77 .....	194·0
10 .....	200·84 .....	199·06
5 .....	205·30 .....	204·12
0 .....	212·0 .....	210·0

VII. *Account of a new Mineral, named Chloropal.*

Prof. Bernhardt, of Erfurt, and Dr. Rudolph Brandes, have given the description and analysis of a new mineral, which they call Chloropal, and of which they distinguish two varieties, the conchoidal and the earthy.

It occurs not far from Ungbwar, in the Comitatus of the same name, and had received the trivial name of green iron earth. The conchoidal variety is pistachio-green, the powder yellowish-white; it is scarcely translucent on the edges, fracture conchoidal, hardness between fluor and calcareous spar, fragile; its specific gravity, according to the mean result of different experiments, approaches very nearly to 2,000. The parallelepiped fragments into which it is apt to break have on the face which, in their natural plate, has been turned upwards, a positive magnetic pole; on its lower face a negative. Four other poles occur on the lateral edges, of which two adjoining are positive, and two opposite negative. The mineral has, therefore, three magnetical axes, which pass each other at right angles, and agree in that respect with opal; but is quite different from quartz and other similar minerals: it does not phosphoresce.

The earthy variety has the same magnetic properties as the former; its fracture is earthy; its specific gravity 1,870; that of another piece 1,727. Both occur with opal.

The composition of the conchoidal chloropal is:

Silica.....	46.0
Oxide of iron.....	35.3
Magnesia.....	2.0
Alumina.....	1.0
Potash and manganese.....	Traces
Water.....	18.0
	<hr/>
	100.0

The earthy chloropal contains:

Silica.....	45.0
Oxide of iron.....	32.0
Magnesia.....	2.0
Alumina.....	0.75
Potash and manganese.....	Traces
Water.....	20.0
	<hr/>
	99.75

Experiments were made to discover fluoric acid, sulphuric acid, lime, and potash, but none was found.

#### VIII. On the Galvanic Ignition of Charcoal. By Mr. W. West.

(To the Editor of the *Annals of Philosophy*.)

SIR,

*Leeds, Feb. 1823.*

IN the *Annals of Philosophy* for Aug. 1822, there appeared a communication from Dr. Hare, of Philadelphia, to the Editor, containing an account of various interesting phenomena produced by means of his galvanic deflagrator: one of these is the ignition of charcoal.

Dr. Silliman, in a letter quoted in the above communication, after describing the appearance of the charcoal at the respective poles, the cone and deposit at the positive, the concavity at the negative, goes on to state as a discovery his having observed these appearances, and adds, "I deem it indispensable that the experiments be performed with the deflagrator."

About twelve months since, when exhibiting the ignition of charcoal before the members of the Philosophical Society here, I was struck with the effect on the charcoal being so opposite at the two poles; the crater on the negative side being well defined, and about one-eighth of an inch deep, the cone on the positive charcoal corresponding, and having at its apex a filament which continued lengthening until broken off by its own weight. I pointed out this in my lecture, but though, as far as I recollect, it had not then been mentioned in any work, I imagined all who had observed the ignition of charcoal with a powerful battery must have seen the same effects produced. I have since repeated the experiment several times, and have verified most of the particulars mentioned by Dr. Silliman; I think that the vapour arising from the charcoal is more abundant when the ignition takes place in vacuo than when in the atmosphere.

I wish to observe more accurately than Dr. Silliman appears to have done, whether there be an increase of weight after ignition in the charcoal at the positive pole; but having little prospect of leisure at pre-

sent, I take this mode of calling attention to the subject, since some who would be likely to pursue it might be discouraged by Dr. Silliman's statement of the inefficacy of the common battery.

The subject is curious and highly interesting, since it involves the following questions:

Is the charcoal fused?

Is it vaporized?

Is it transferred from the one pole to the other, and, if so, in what manner?

Would not satisfactory answers to these questions throw much light on many abstruse points, and especially on that most difficult question in physics, the relation of radiant, or empyreal, or imponderable bodies to ordinary matter?

WILLIAM WEST.

IX. *On a Mineralogical Work of Agricola, &c.* By G. Cumberland, Esq.  
Hon. Mem. G.S.

(To the Editor of the *Annals of Philosophy*.)

SIR,

Bristol, Dec. 9, 1823.

Your correspondent, Mr. Conybeare, at p. 366 of the *Annals* for Nov. 1822, in his analysis of the work of Vannoccio Biringuccio, asks from what work of Georgio Agricola he relates "the discovery of a mass of silver ore, in one of the Saxon mines, sufficiently large to make a table and a seat, or stool (tripode)." In return for the satisfaction his paper has afforded me, I have the pleasure to inform him that Vannoccio found it in a dialogue, entitled "Il Bermanno," in compliment to his friend Bermanno, the physician; placed at the end of Agricola's treatise "On the Generation of Things underground, with their Nature and the Nature of Fossils;" the other interlocutors being Nicola Ancone and Giovanni Nevio. In this dialogue we have a very minute account and history of the mines near Friberg, in Bohemia, one of the richest in silver of which, was, according to his statement, that called Georges, from St. George (as may be seen leaf 404, for the book is so pagged). At leaf 446, on the blank side, Bermanno states, that pure masses of silver are only found in Bohemia.

In Sneberg, in a mine called Georgio, more is found, he observes, than in all the other mines in Germany, for (as he has heard) there was once so large a mass extracted, that Duke Albert of Saxony, who, above all other princes of Germany, excelled in glory, arms, and wealth, and who was the father of that Georgio who now is the sovereign Prince, having stopped to see the mines, and called for some refreshment, made use of that great mass of silver for his table, and those who attended him, and had likewise dismounted; and during his repast, he observed, "the Emperor Frederick is powerful and rich, but he cannot sit down to-day at such a table as this!" These, says he, were the words of the Duke Albert, astonished greatly at this prodigious mass of pure silver; "but I was more astonished," continues Bermanno, "when in Sneberg I heard a calculation of the amount of silver it had produced." To which Nevio replies, "You tell us things very uncommon; pray what might that mass weigh?" and Bermanno answers, "A little more than (x mi-peso) ten thousand pounds.

This dialogue consists of forty-seven leaves, the first of which is numbered 520, instead of 420, is really very interesting, and is

written in pure and elegant Italian; being a translation from the Latin of the author. It has a preface by Michael Tramezzino, the printer in Venice, 1550. Georgio Agricola states at leaf 421, that his motives for having entered so copiously into the study of metals, was their utility in the practice of the Greek and Roman physicians, of the fruits of which he modestly leaves others to judge, referring for testimonials to Bartolomeo Baccho and Lorenzo Bermanno, men of letters, as well as expert metallurgists, whom he has often, he says, wearied with his inquiries.

During the dialogue, they ascend to the mining country, and while examining the operations of the mines, discuss many interesting subjects relative to the acquaintance of the ancients with the nature of metals, and to the uses to which they applied them in medicine; also respecting the nature of plumbagine, pyrites, red silver ore, earths of various colours, minium, cinnabar, rubrica, spars, gypsum, &c.

In Froben's Folio, 1563, of Agricola's *Arte Metalli*, translated from the Latin into Italian by Michelangelo Florio, of Florence, and by him dedicated to Queen Elizabeth of England, there is another dedication of the work, by Agricola himself, to the Duke of Saxony, in which, after recapitulating all that Greek and Latin authors have written on the subject, he adds, "In our language, I find two only; one relating to experiments on ores and metals, which work is very obscure, and its author unknown; the other on mineral veins, which is said to have for its author, Pandolfo, an Englishman." Query, who was this Pandolfo? A book in German also, he says, was written by Calbo Fribergo, a physician of but little reputation; and all these, it should seem, wrote previously to Vannoccio Biringuccio; but of him he speaks so handsomely, that you will permit me to give it in his own words:

"Poco ha iziandio che Vannoccio Biringuccio da Siena, *homo dotto & sperimentato in molte cose*, fece un libro in lingua Italiana, nel quale tra trattato del modo di fondere, spartire, et congiugnere insieme i metalli. Ha eziandio con brevità trattato del modo di cuocere alcune vene et pue chiamamēte ha dichiaavato e mostrato il modo de fare alcuni sughi: e quando. Io lessi queste sui cose, mi ternaron a mente quei che gia vidi fare in Italia: ma l'altre cose che io scrivo, o egli non l'ha punto toeche a legiermente;" adding, that the book was given him by Badoaro, a noble Venetian, when passing through Marienburg as Ambassador to King Ferdinand. Might not, therefore, this work be interesting to English artists, if carefully translated?

I am, Sir, yours, &c.

G. CUMBERLAND.

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### ARTICLE XIII.

#### NEW SCIENTIFIC BOOKS.

PREPARING FOR PUBLICATION.

Dr. Pring, of Bath, will shortly publish an Exposition of the Principles of Pathology.

Capt. Franklin's Narrative of his Journey from Hudson Bay to the

Copper Mine River, is in the Press. It will be illustrated by numerous Plates.

Mr. Bowdich has in the press, a Sketch of the Portuguese Establishment in Congo, Angola, and Benguela, with some Account of the modern Discoveries in the Interior of Angola and Mosambique.

JUST PUBLISHED.

An Epitome of the Elementary Principles of Natural and Experimental Philosophy; including Mechanics, Pneumatics, Acoustics, Hydrostatics, and Hydraulics: with a copious Account of the Progress and present State of the Steam Engine. By John Millington, MGS, Professor of Mechanics at the Royal Institution, Secretary to the Astronomical Society, &c. &c. 1 Vol, 8vo. with 14 Plates. The author announces a Second Part, which is to treat of Magnetism, Electricity, Optics, and Astronomy.

A Geometrical System of Conic Sections for the Use of Mathematical Students at the Royal Liverpool Institution. 8vo. 6s. 6d.

Elements of Practical Mechanics. By Giuseppe Venturoli, Professor of Mathematics in the University of Bologna. To which is added, a Treatise upon the Principles of Virtual Velocity, and its Uses in Mechanics. Translated from the Italian, by Daniel Cresswick, DD. Fellow of Trinity College, Cambridge. 8vo. 8s.

Observations on the Effects of Lightning on Floating Bodies, with an Account of the new Method of applying fixed and continuous Conductors of Electricity to the Masts of Ships. In a Letter to Vice-Admiral Sir Thomas Byam Martin, Comptroller of his Majesty's Navy, &c. &c. By W. Snow Harris, MRCS. 4to. With Six Plates. 12s.

An Essay on the Medicinal Efficacy and Employment of the Bath Waters; illustrated by Remarks on the Physiology and Pathology of the Animal Frame, with Reference to the Treatment of Gout, Rheumatism, Palsy, and Eruptive Diseases. By Edward Barlow, MD. Graduate of the University of Edinburgh, and one of the Physicians of the Bath Hospital, &c. 8vo. 8s.

.. Digitalium Monographia, sistens Historiam Botanicam Generis; tabulis Omnium Speciarum hactenus cognitarum illustratam, ut plurimum confutis ad Icones F. Bauer, penes G. Cattley Arm. cure J. Lindley, FLS. et FHS. Folio. With 28 Plates, 4l. 4s. Coloured, 6l. 6s.

The Natural History of the Lepidopterous Insects of New South Wales. Collected, engraved, and faithfully painted after Nature, by J. W. Lewin, ALS. late of Paramatta, New South Wales. 4to. With 19 Plates, 1l. 11s. 6d.; or finely coloured, 2l. 8s.

The Birds of New South Wales, and their Natural History. By the same Author. 4to. With 26 Plates, 2s. 2s.; or coloured, 6l. 6s.

The Linnean System of Conchology, describing the Orders, Genera, and Species of Shells, arranged into Divisions and Families, with a View to facilitate the Student's Attainment of the Science. By John Mawe. With 36 Plates. Plain and coloured Frontispiece, 1l. 1s.; or the whole beautifully coloured, 2l. 12s. 6d.

An Account of some recent Discoveries, Hieroglyphical Literature, and Egyptian Antiquities; including the Author's original Alphabet, as extended by Mr. Champollion; with a Translation of five published Greek and Egyptian Manuscripts. By Thomas Young, MD. FRS. &c. With Plates. 8vo. 7s. 6d.

A Statistical and Commercial History of the Kingdom of Guatimala, in Spanish America; containing important Particulars relative to its Productions, Manufactures, Customs, &c.; with an Account of its Conquest by the Spaniards, and a Narrative of the principal Events down to the present Time. From original Records in the Archives, actual Observation, and other authentic Sources. By Don Domingo Juarros. Translated by J. Baily, Lieut. Roy. Mar. Illustrated with Maps. 16s.

Columbia; being a Geographical, Statistical, Agricultural, Commercial, and Political Account of that Country; adapted for the general Reader, the Merchant, and the Colonist. With a Map, and Portraits of Bolivar and Zea. 2 Vols. 8vo. 1*l.* 16s.

## ARTICLE XIV.

### NEW PATENTS.

G. Richards, of Truro, Cornwall, architect, for certain improvements in grates, stoves, furnaces, and other inventions; for the consumption of fuel, and in the flues connected with them, whereby they are rendered more safe, and the smoke prevented from returning into the rooms in which they are placed; and also, for an improved apparatus for cleansing the same.—Dec. 26.

T. Rogers, of Store-street, Bedford-square, Middlesex, Esq. for a method or apparatus for the purpose of attaching trowsers and gaiters to boots and shoes.—Dec. 26.

J. Neville, of New-walk, Shad Thames, Surrey, civil engineer, for an improved method of producing and applying heat to, and constructing and erecting furnaces and other reservoirs, severally used for the various purposes of roasting or smelting metallic ores, or other substances, and likewise for effecting a saving in fuel, and producing a more complete combustion of smoke than at present takes place, as well as a better mode than any now in use, of collecting and preserving any volatile substance contained in, or combined with, metallic ores or other substances in the separation of which heat is necessary.—Jan. 8, 1823.

W. Johnson, of Great Totham, Essex, Gent. for a means of obtaining the power of steam for the use of steam-engines with reduced expenditure of fuel.—Jan. 8.

W. Lister, of Baildon, Otley, Yorkshire, cotton-spinner, for certain improvements in the method and machinery for preparing and spinning wool, silk, mohair, and other animal fibre, of any quality or length of staple.—Jan. 16.

R. Copland, of Wilmington-square, Clerkenwell, Middlesex, Gent. for combinations of apparatus for gaining power; part of which are improvements upon a patent already obtained by him for a new or improved method or methods of gaining power, by new or improved combinations of apparatus applicable to various purposes.—Jan. 16.

G. Miller, of Lincoln's Inn, Middlesex, Brevet Lieut. Col. in the Royal Brigade, for a method or plan of communicating the spiral motion to shot and shells when fired from plain barrels, and for igniting, by percussion, shells to which the spiral motion has been thus communicated.—Jan. 16.

## ARTICLE XV.

## METEOROLOGICAL TABLE.

1823.	Wind.	BAROMETER.		THERMOMETER.		Evap.	Rain.	Daniell's hyg. at noon.
		Max.	Min.	Max.	Min.			
2d Mon.								
Feb. 1	N E	28.97	28.75	40	36	—	99	
2	N E	29.00	28.75	42	39	—	15	
3	N E	29.40	29.00	45	30	—	25	
4	Var.	29.80	29.40	37	27	—		
5	E	29.80	29.75	34	27	—		
6	E	29.75	29.35	34	27	—	—	
7	E	29.58	29.35	44	30	—	61	
8	W	29.82	29.58	44	33	—		
9	W	29.82	29.62	44	37	—	11	
10	S W	29.62	29.60	45	34	—	38	
11	S W	29.60	29.50	52	42	—	03	
12	S W	29.73	29.50	50	40	—	01	
13	W	29.73	29.57	45	37	—		
14	W	30.00	29.57	48	33	—		
15	N W	30.30	30.10	40	34	—	12	
16	N E	30.30	30.30	40	32	—	—	
17	N E	30.30	29.91	38	30	—	—	
18	N W	29.91	29.27	45	38	—	28	
19	W	29.96	29.27	47	30	—		
20	N W	29.96	29.72	46	34	—	01	
21	N W	29.72	29.68	49	39	—	—	
22	S W	29.80	29.68	46	34	—	40	
23	S W	29.80	29.65	46	41	—	43	
24	N W	29.80	29.79	48	34	—		
25	S	29.79	29.24	48	33	—	10	
26	W	29.24	29.10	45	27	—		
27	N	29.53	29.13	42	31	—	—	
28	N W	30.03	29.53	42	30	.96		
		30.30	28.75	52	27	.96	2.92	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

## REMARKS.

*Second Month.*—1—3. Rain. 4, 5. Fine. 6. Snow began about two, p.m. 7. Rain. 8, 9. Fine. 10—12. Rain. 13. Fine. 14. Rain: some hail in the afternoon. 15. Rain. 16, 17. Cloudy. 18. Fine morning: rain in the evening. 19. Fine day: very wet and stormy night. 20. Very fine: a distinct lunar halo in the evening. 21. Overcast. 22. Rainy. 23. Heavy rain: a violent driving shower of hail, which continued for about one minute, when it abated a little: night very boisterous. 24. Cloudy. 25. Rain: overcast. 26. Cloudy and fine. 27. Fine; a little snow, p.m. 28. Fine.

## RESULTS.

Winds: N, 1; NE, 5; E, 3; S, 1; SW, 5; W, 6; NW, 6; Var. 1.

## Barometer: Mean height

For the month..... 29.620 inches.

For the lunar period, ending the 2d..... 29.774

For 14 days, ending the 12th (moon south)..... 29.617

For 13 days, ending the 25th (moon north)..... 29.612

## Thermometer: Mean height

For the month..... 38.000°

For the lunar period..... 30.224

For 29 days, the sun in Aquarius..... 36.034

Evaporation..... 0.96 in.

Rain..... 2.42

*Laboratory, Stratford, Third Month, 24, 1823.*

R. HOWARD.

ANNALS  
OF  
PHILOSOPHY.

MAY, 1823.

ARTICLE I.

*Additional Remarks on the Rothe Todte Liegende and Weiss Liegende of German Geologists.* By Thomas Weaver, Esq.  
MRIA. MRDS. MWS. MGS.

(To the Editor of the *Annals of Philosophy*.)

SIR,

Tortworth, April 4, 1823.

IN my former papers, I endeavoured to elucidate the subject of the rothetodtliegende formation, as well as that of the weissliegende, in such a manner as might obviate all further misconception or doubt; \* not by gratuitous assumption, but by strict induction from the observations and descriptions of those who have best studied the subject in Germany; at the head of whom stands Freiesleben. In conducting this discussion, it has been my object constantly to bear in mind that the solution of a problem cannot be valid, unless it embrace all the conditions necessarily belonging to it; nor the sense of an author be justly given, unless the scope of his argument be fully considered in all its branches and bearings. Partial views generally lead to false results. Yet it seems I have failed in conveying conviction where I particularly wished to impress it.† Nor is it difficult to account for the failure, since my opponent persists in attaching a meaning to the word rothetodtliegende, for which I confess I can find no adequate German authority. However averse to

\* See *Annals of Philosophy*, Oct. and Nov. 1821, and Aug. 1822.

† See the interesting "Memoir illustrative of a general Geological Map of Europe, by the Rev. W. D. Conybeare," in the *Annals of Philosophy* for Jan. Feb. and March, 1823.

controversy, a desire of supporting and vindicating the true state of the case, impels me once more to resume the pen.

Mr. Conybeare insists that the rothetodtliegende is the equivalent of the calcareous or new conglomerate of England, overlying the carboniferous series; and he even contends that Freiesleben considers and treats the rothetodtliegende and the true coal formation as appertaining to two different series; adding, that the invariable order in ascending is, 1. Coal formation. 2. Porphyry. 3. Rothetodtliegende; and sections are quoted from Keferstein, as demonstrative of this position.

I have already stated that the great object of Freiesleben's extended work was the description of the four following groups, or formations; opposite to which I place their English equivalents. They are considered in four sections:

- |   |  |
|---|--|
| I. The upper or shell limestone formation. . . . .  | } Lias and oolitic series.   |
| II. The upper, or newer, or variegated, sandstone formation. . . . .  |  |
| III. The lower or alpine limestone formation, including the <i>weissliegende</i> as the lowest bed. . . . . | } Magnesian limestone formation, including the <i>calcareous</i> or <i>new conglomerate</i> as the lowest bed. |
| IV. The lower or older sandstone, or rothetodtliegende, formation. . . . .                                  |  |

I have maintained that the *weissliegende* alone (and not the rothetodtliegende) is the representative of the calcareous or new conglomerate of England.\* The general description of the new conglomerate is to be found in the *Annals of Philosophy* for November, 1821, and more at large in Freiesleben, vol. iii. p. 239—280. It is to that author the Germans owe the clear exposition of the true character of the *weissliegende*, and the proof that it belongs to a newer series than the carboniferous; while almost all preceding German writers had considered the *weissliegende* as the uppermost bed of that series, and included it accordingly, and some even had called it rothliegende. But to continue so to apply the latter denomination is obviously to persevere in an antiquated error, exploded by Freiesleben. And conversely, to

\* I purposely avoid using in this paper the term "new red conglomerate or sandstone," by which this formation has been partly designated in England; since it has led, and, if continued, is likely still to lead to misconception. As used by some English writers, it denotes the calcareous conglomerate; sometimes it includes the magnesian limestone, and sometimes it is extended to the new red sandstone formation also, properly so called. A vagueness of language thus arises, which is productive of considerable obscurity; offering nearly a counterpart to that which has proceeded from the indiscriminate use of the term *rothetodtliegende* in Germany, as applied to all parts of the carboniferous series.

call the true rothetodtliegende the new conglomerate, and then to build on this misapplication of the term, is a sure method to render a subject, otherwise simple and clear, involved and confused; and thus calculated to mislead both the writer and reader, confirming the remark of Bacon, that "while men believe their reason governs their words; it often happens, that words have power enough to react upon reason."

I have maintained that the rothetodtliegende formation is the equivalent of the carboniferous series, extending from the old red sandstone to the coal formation inclusive, or *vice versâ*. Now to prove that Freiesleben's own view of the matter is identical with that which I have given, I think it will be sufficient, in addition to the facts detailed in my former papers, to transcribe in this place the Synoptical Table of that author, prefixed to his fourth volume, which exhibits the method according to which he considers and treats this subject; and then to offer a few remarks intermingled with such illustrative extracts from the body of the work, as bear more immediately on the question.

"SECT. IV.—*The Lower Sandstone Formation, or Rothe Todte Liegende.* Die Untere Sandstein Formation (das Rothe Todte Liegende).

Occupying pages 67—210 of the fourth volume, and considered under the following heads:

I. *The Rothe Liegende (Red Sandstone) separately considered.*

Das Rothe Liegende an und für sich betrachtet, p. 73—137.

Beds composing the Rothliegende, viz. conglomerate, breccia, sandstone, slaty micaceous sandstone, indurated slaty clay, and clay marl, p. 73—99.

Structure, p. 99—107.

Relative position, affinities, and graduations, p. 107—118.

Intermingled minerals, including also beds of limestone, compact splintery, sub-lamellar, or granular, p. 118—123.

Distribution of ferruginous matter, p. 123—127.

Veins in the rothliegende, p. 127—131.

Petrifications, p. 131—136.

Springs and mineral waters, p. 136, 137.

II. *The Subordinate Members.* Untergeordnete Gebirgsarten, p. 137—191.

1. Porphyry and amygdaloid, p. 137—147.

2. Coaly shale, p. 147—169.

3. Coal, 170—191.

Distinctions hitherto made respecting the relation of the coal formation to that of the rothliegende, p. 170—172.

Occurrence of coal, 172—191.

In Sangerhausen, p. 173—175.

In Stollberg, 175—178.

In Anhalt Bernburg at Opperde, &c. and in the Circle of the Saale, 179.

- In the Forest of Thuringia, p. 179—191.
- III. *Occurrence and Distribution of the Formation in general.*
- Vorkommen und Verbreitung, p. 191—210.
- In Mansfeld, extending into Anhalt, p. 191—193.
- In the Circle of the Saale, 194—195.
- In Sangerhausen and Stollberg, 195—197.
- In Kiffhäuser, Bottendorf, and Gera, 198—200.
- In the Forest of Thuringia, 200—206.
- In other parts of Germany, Riegelsdorf, Hesse, Wirtemberg, Mark, Silesia, Bohemia, 207—208.
- In countries beyond Germany, 208—210.

*Appendices and Additions to vol. iv. p. 210—392.*

1. On transition clayslate and greywacke tracts in the Hartz, Neustadt, &c. p. 213—228.
2. Geological and mining observations on the collieries at Opperoode, Meisdorf, &c. in Anhalt-Bernburg, and at Wettin, Löbegrün, Petersberge, &c. in the Circle of the Saale, p. 229—268.
3. Letters relating to the shell limestone, new red sandstone, and lower limestone formations, described in the three first volumes, p. 269—294.
4. Additional observations on the shell limestone, new red sandstone, lower limestone, and rothetodtliegende formations, p. 295—392."

With this Synoptical Table for a guide, and Freiesleben's descriptions in illustration, it has been a great surprise to me that any one should ever have doubted that the older sandstone, or rothetodtliegende formation of the Germans, was the representative of the carboniferous series.\*

After the details into which I have formerly entered, the more immediate remarks required on the preceding synoptical table may be confined, *first*, to the rothliegende separately considered; and *secondly*, to the nature of its connexion with the coal formation.

1. Now with respect to the *rothliegende considered separately*,

\* It may be useful in this place to refer to the origin of the expression rothetodtliegende, or red dead lieg. It has been applied to the carboniferous series in general for this reason; that in some quarters the coal formation associated with red sandstone as one of its members; in others, the old red sandstone itself (both more or less strongly characterised by the diffusion of red oxide of iron), form the immediate support of the weissliegende or new conglomerate, and bituminous marl shale, the two former thus acting in relation to the two latter as the red dead lieg; that is, as the red substratum, comparatively barren of metal, in contradistinction to the superincumbent new conglomerate and bituminous marl shale, both of which are abundantly metalliferous.

I may here repeat the remark, which I made in a former paper (*Annals of Philosophy*, January, 1823), that the expressions todtliegende, rothliegende, and rothetodtliegende, are synonymous, the two former being frequently employed for the sake of brevity.

if we pay due attention to its *physical characters*, and to its *relative position*, *affinities*, and *graduations*, I know not how it is possible to resist the evidence thus afforded, that it represents, in its *lowest position*, the old red sandstone of English geologists.

The *general characters* of the beds composing the rothliegende as consisting of *conglomerate*, *breccia*, *sandstone*, *slaty micaceous sandstone*, *indurated slaty clay*, and *clay marl*, have been given in the *Annals of Philosophy* for Aug. 1822, p. 84—86; and I there adverted to their agreement with those of the old red sandstone of Gloucestershire (and of the adjoining counties), and also of that of Ireland. But no where is the analogy to be found more complete in all its parts than in the old red sandstone of Scotland, particularly that portion of it which borders the northern side of the Scotch great coal tract.\* I know no other portion of the carboniferous series, with which the rothliegende in its lowest position can be said to correspond so perfectly.

With regard to the *relative position* and *affinities* of the rothliegende, the following extracts from Freiesleben may suffice.

Vol. iv. p. 107, et seq. "The rothliegende is always bounded on the one side by one or the other member of the cupriferous shale tract, lying generally immediately below the calcareous or new conglomerate (weissliegende) or the cupriferous marl shale; but its confines on the other side are less determinate. Here it graduates principally into tracts of greywacke and clayslate, or porphyry, accordingly as it reposes upon, or forms basins in, one or the other of them."†

"In several districts, its immediate lower boundary is formed by one or the other of these tracts; but in some others a formation of coaly shale or coal is found interposed between them."‡

\* See Dr. Boué's instructive description of the *poudingues et grès rouges* of Scotland, p. 102—119, of the *Essai Géologique sur l'Ecosse*; and compare it with Freiesleben's description of the *rothliegende*, vol. iv. p. 73—137. The two descriptions agree so perfectly as to answer nearly one for the other.

† To render the above paragraph perfectly clear to the English reader, it is necessary to bear in mind that the *red sandstone* of the coal tracts of Germany, bears the name of *rothliegende* as well as the fundamental portion of the carboniferous series, namely, the old red sandstone. In point of position, the former might be partly compared to the red sandstone found in some of the English coal fields, which, appearing at intervals, sometimes forms beds of great thickness and extent, dividing the more common coal bearing strata from each other, and partly known by the name of *pennant stone*, e. g. in the Gloucestershire south coal field. It is one of the peculiarities of several of the coal fields in the north of Germany, that coarse red conglomerate and red sandstone are found frequently alternating on a greater or smaller scale with the other coal measures, the former partly resembling analogous beds in the fundamental old red sandstone. This is particularly observable in Lower Silesia, where a bed of coarse red conglomerate generally forms the immediate roof of the coal seams; a bed of slate clay of seven feet in thickness being there a rare occurrence. See Von Buch, *Geog. Beob.* vol. i. p. 90 and 101. Also Freiesleben's description of the Opperrode and Petersberge coal districts as quoted by me in the *Annals of Philosophy*, August, 1822, p. 87—89; and Von Raumer in the *Annals of Philosophy*, Oct. 1821, p. 248—250, and Aug. 1822, p. 91—93.

‡ This observation requires attention, and will be duly noticed hereafter, p. 329.

"Generally speaking, the rothliegende seems the more disposed to receive foreign ingredients into its composition, as well as beds, the nearer it approaches to the fundamental rocks upon which it reposes. And it is not to be denied that it possesses many points of agreement with transition tracts; so much so, that some geologists are disposed to rank it rather with the transition series, and to commence the floetz series only with the rocks peculiarly belonging to the bituminous or cupriferous marl shale" \* (of which the weissliegende, or new conglomerate, is the first or lowest member).

The connexion of the rothliegende with transition greywacke and clay slate tracts, is exemplified by a reference to the districts of Mannsfeld, Sangerhausen, and Stollberg, where it reposes upon, and partly graduates into them.

Its connexion with porphyry (trap also occurring in the association), is stated to be particularly well exhibited in the Forest of Thuringia, the two formations not only alternating with, but appearing in a manner diffused in each other; e. g. in the northern declivity of the Schneekopf, and again extending from the Grossen Buche into the Schmückengraben in the same mountain. Its intimate alliance with porphyry is shown also in Mannsfeld.—(See *Annals of Philosophy*, Aug. 1822, p. 87.)

In all these cases, the ingredients which compose the rothliegende are said to vary more or less according to the constitution of the transition or primary tracts upon which it reposes.—(See *Freiesleben*, vol. i. p. 32—34, and p. 43—46; and also vol. iv. p. 67—99.)

Both relative position and physical characters, therefore, prove that the rothliegende constitutes, in its lowest position, the fundamental portion of the series, or what is designated in England as the old red sandstone.

2. The same inference is to be drawn, from considering the connexion of the rothliegende with the coal formation.

On this subject, *Freiesleben* (see p. 170—172 of vol. iv.) adverts, in the first place, to the distinctions formerly made in Germany respecting the relation of the coal formation to the rothliegende; some writers having considered a portion only of the coal formation as included in the rothliegende, while others incorporated the whole of the coal formation with the rothliegende; thus constituting, instead of one group with two divisions, one simple unbroken series. As an exemplification of the latter mode of considering the subject, he produces the arrangement of *Karsten*, who gives the following beds as a type of the general series, taken in an ascending order:

† "See Von Hoff in *Leonhard's Taschenbuch*. Jahrgang. viii."

1. Conglomerate of ancient rocks.
2. Siliceous conglomerate.
- 3 to 8. Coal, sandstone, and shale.
9. Trap rocks.
10. Clay ironstone.
11. Rothliegende.
12. Weissliegende.

All comprehended in one general group, entitled the *older sandstone*, or *rothetodtliegende formation*.\*

Now what is the observation of Freiesleben upon this series? He says it is too general, and not sufficiently distinct, and, therefore, for the sake of greater clearness and precision, he considers it necessary to *describe* the rothliegende and the coal formation under *separate heads*; and he arranges the subject accordingly, as displayed in the synoptical table, leaving for his third and last division the account of the occurrence and distribution of the series in general.

It is obvious from the structure of that table, that *coal* is considered by him in a *general point of view* as a *member* of the *older sandstone or rothetodtliegende series*; and so far he continues to adhere to the arrangement of his predecessors. But he qualifies this general view by a more particular one, expressed in the following words: "I have throughout my present exposition considered the rothliegende and the true coal tracts (eigentliche steinkohlengebirge, with siliceous conglomerate, slate-clay, and bituminous shale), as *two very closely allied formations*, but *which ought nevertheless to be distinguished from each other*. And hence in my descriptions I always make a distinction between the *true coal tracts*, and *those single beds of coal* that occur incidentally subordinate to the rothliegende."† This particular

\* The series given above is also quoted by Mr. Conybeare, on which that gentleman observes, "If we look among these rocks for the representative of our own old red sandstone, it must be sought in No. 1 and 2, not in No. 11;" an observation in which I perfectly agree with him, since it is one of the points for which I have been contending; in confirmation of which I have to observe, that No. 1 and 2 of that series do in fact represent the *lowest portion* of the rothliegende of Freiesleben.

But that Karsten's group is to be considered as a general type of the older sandstone series, and not as expressing with precision the order of succession throughout, appears evident from the introduction of trap rocks and clay ironstone under Nos. 9 and 10. The two principal members of the series are first expressed, namely, the old red sandstone by No. 1 and 2, and the coal formation by No. 3 to 8. Then trap and clay ironstone are noticed, the former as being incidental both to the old red sandstone and to the coal formation, and the latter as more peculiarly belonging to the coal formation. With respect to the rothliegende, No. 11, I must repeat that it is in this position a term, denoting the upper portion of the coal measures, where interstratified with and divided by *red sandstone* on a greater or smaller scale as one of its members. And lastly, with regard to the weissliegende No. 12, I must also repeat that this formation had been considered as the uppermost bed of the older sandstone series by most German writers, until Freiesleben demonstrated that it was the lowest member of the succeeding alpine limestone formation (answering to the English magnesian limestone), thus corresponding with the calcareous or new conglomerate of England.

† I give the original passage in this place. "In meiner gegenwärtigen darstellung habe ich jederzeit die formation des rothliegenden und die eigentlichen steinkohlenge-

view, however, does not invalidate his general position; namely, that all the true coal tracts are comprehended in the older sandstone or rothetodtliegende series; an arrangement in which all German geologists have always concurred. This state of the case is proved, not only by the synoptical table, but by the words of the author's text, from which I subjoin the following extracts:

Vol. iv. p. 179. "It is quite certain that the bed of coal near Opperde, in Anhalt, *lies in* the rothliegende, and I must affirm the same of the coal near Wettin, both from my own observations, and from the accounts of others, that may safely be relied on. But of these I need take no further notice in this place, as I mean to describe them in distinct dissertations in the second appendix to this volume."

I have given the substance of these descriptions in the *Annals of Philosophy*, Aug. 1822, p. 87—89, to which I beg leave to refer the reader; and in relation to which I will here only add two other extracts.

Vol. iv. p. 194. "The rothliegende extends from Hettstädt eastward beyond the river Saale;" and "nearer toward Wettin its outcrop is covered by the coal tract (näher nach Wettin zu, wird sein ausgehendes von steinkohlengebirge bedeckt)." And p. 123, where the same fact is related in the following words: "I have been assured that beds of oolitic limestone have been met with in the rothliegende that lies below the Wettin coal seams." This language, than which none can be more explicit, is quite consistent with that of Lehman, who speaks of the rothliegende as "*la base sur laquelle sont appuyés les lits du charbon de terre,*" and which I quoted upon a former occasion.\*

Again, vol. iv. p. 208. "The coal tracts (steinkohlengebirge) situated near Beuthen, Pleiss, and Troppau, in Upper Silesia, near Schweidnitz, in Lower Silesia, and in the south-western and north-eastern portions of the county of Glatz, are likewise ascribed by Von Buch, as well as by later writers, to the formation of the todtliegende.† The todtliegende, however, appears in many parts of those countries in its usual form; that is, destitute of coal." The same fact is repeated, p. 190, 191, with the addition, "Reuss likewise states, that considerable beds of coal

birge (mit kieselconglomerat, schieferthon, und brandschiefer), als zwey, einander zwar ganz nahe stehende, aber dennoch von einander zu trennende formationen, betrachtet; daher ich auch immer noch einen unterschied zwischen dem weiterhin zu beschreibenden (untergeordneten) vorkommen einzelner steinkohlenfloetze im rothliegenden und zwischen dem eigentlichen steinkohlengebirge annehme."

\* I avail myself of this opportunity to notice an oversight committed in transcribing the account of the lower coal field near Wettin (not Löhögün, as stated by Mr. Conybeare), by having inserted "*rothe todtliegende,*" instead of "*rothes thonartiges liegendes,*" as the lowest bed (No. 16) observed in that coal field. The error, however, is of no real importance, since the expression signifying an argillaceous bed of the rothliegende, it amounts to the same thing.

† "Von Buch. Mineralogical Description of Landeck."

occur in the todtliegende in Bohemia." \* And the same observation is to be found in vol. i. p. 46.†

Yet with all these facts before him, Mr. Conybeare assures us, "that the plan of Freiesleben's work does not extend to the coal formation, properly so called." And why? Because that author has made a distinction between the single beds of coal that occur incidentally subordinate to the rothliegende, and the continuous coal tracts. With equal justice might it be said, that the plan of a writer on the north-east of England did not extend to the coal formation, properly so called, because a distinction was made between the single beds of coal incidentally disposed in the carboniferous limestone of that tract, and the continuous coal measures.

I think it needless to repeat in this place what I have already stated in former communications concerning the occasional juxta-position of the old red sandstone and the coal formation, as well as concerning their association with limestone, porphyry, and trap, respectively.

There is, however, one part of the German description (adverted to above, p. 325) that requires distinct notice, since from the language employed, it has probably given rise to considerable misapprehension. It is where it is said, that a formation of coaly shale or coal is interposed in some districts between the rothliegende and the subjacent tracts of clayslate and greywacke, or porphyry; particularly in certain parts of the Forest of Thuringia. This representation seems to imply nothing more than that, in the general expanse of the carboniferous series, a part of the coal formation is itself in particular quarters in contact with transition or primary tracts. Yet, even in these cases, rothliegende is said to form the basis of the coaly shale or coal. See e. g. the account of the coaly shale near Goldlauter, in the Upper Trogberg, near Breitenbach, and other places, which indeed are considered by Von Hoff and Jacob as belonging to the coal formation (Freiesleben, vol. iv. p. 142—169). See also the local occurrences of coal in various parts of the Forest. (Ib. p. 179—191.) In several of these cases, both limestone and porphyry are associated in the series.

The preceding references and extracts from Freiesleben have, I conceive, clearly proved, that the rothetodtliegende series of the Germans is the representative of the carboniferous series of the English. And no where can I find any evidence in that author, by which it could be shown, that the rothetodtliegende series has any connexion beyond that of juxta-position with the weissliegende; a formation, which, corresponding both in physical characters and in relative position with the calcareous or

\* "Reuss. Mineralogische und Bergmännische Bemerkungen über Böhmen."

† In stating that Von Buch and Reuss refer the true coal tracts of Silesia and Bohemia to the todtliegende formation, it is the intention of Freiesleben to show that they belong to the first floetz or carboniferous series, in contradistinction to formations of coal of later origin (e. g. wood coal) also found in those countries.

new conglomerate of England, can alone be considered as forming the first member of the succeeding series; namely, of the gypseous and saliferous. The *weissliegende*, however, has, as already remarked, been sometimes called *rothliegende* by some German writers, and this misapplication of the term has thus also led to a confusion in description, which really does not exist in nature. And here, perhaps, Mr. Conybeare may find one of the sources of the misconception into which he has fallen.

From all that has been stated, it is evident that *Freiesleben* affords no support whatever to the position of my opponent; namely, that the *rothetodtliegende* is the equivalent of the calcareous or new conglomerate of England; and that which is attempted to be derived from the sections of *Keferstein* must, for the same reasons, appear invalid. The order noticed there in ascending is, it seems, 1. Coal formation; 2. Porphyry; 3. *Rothliegende*; 4. Alpine, or lower limestone formation; 5. New red sandstone.\*

Upon this section I have to observe, in the first place, that the *weissliegende* or new conglomerate being by *Freiesleben* included in the alpine or lower limestone formation, I presume it is so included in No. 4 of this section; and consequently that the *rothliegende* No. 3 cannot be intended by *Keferstein* to be its representative. And, in the second place, *Freiesleben* having clearly shown the porphyry and coal to be comprehended in his general *rothliegende* series, the term *rothliegende* applies to the uppermost beds of the coal formation as well as to the lowest bed of the carboniferous series.† To exhibit, therefore, the coal formation, porphyry, and *rothliegende*, in the above order, as a type of the general series, is manifestly an imperfect mode of representing their mutual relations; since the *rothliegende* (as has been shown from *Freiesleben*) is found alternating with porphyry below the coal formation, and the coal formation itself occurs also alternating with porphyry. The fact appears to be that in the section adverted to above, Nos. 5 and 4 (the latter of which comprehends also the new conglomerate as the lowest member) constitute together the saliferous and gypseous series; while Nos. 3, 2, and 1, belong to the carboniferous series, yet displayed in a manner that, so far from being luminous, conveys only an imperfect idea of the general subject. Of the two, the series presented by *Karsten*, though by no means complete (and from which the *weissliegende* must be excluded), is, as a general type of the *rothetodtliegende* formation, superior to that of *Keferstein*. Hasty generalizations and forced constructions, instead of promoting, tend rather to retard the progress of

\* I quote from Mr. Conybeare, not having myself yet seen *Keferstein's* memoirs. I presume these memoirs are compiled rather from the recorded observations of others than from original researches conducted by that author himself.

† See the preceding remarks and notes on this head.

science; and correct approximations can only be produced by close comparisons, and a careful attention to those details that are but too often overlooked.

Mr. Conybeare has promised to produce detailed quotations from Lehman, Karsten, Von Buch, Von Humboldt, Freiesleben, Von Raumer, D'Aubuisson, and Keferstein (I place these names nearly in chronological order), all of which shall concur in proving, that the *great mass of the rothetodtliegende* occupies a position superior to the coal measures. Now, supposing even that this were the case, it could be of no avail to the argument of my opponent, as *rothetodtliegende* does not signify the new conglomerate. It is doubtless owing to such a prepossession in the mind of that gentleman, that he has ventured to assert that the whole of the *rothliegende* (with its beds of limestone and porphyry), extending from the Hartz to the Petersberge on the Banks of the Saale, is in a position superior to the coal formation. I must, however, take the liberty of observing, that this statement appears wholly unjustified, being *an inversion of the fact*, and decidedly at variance with the detailed descriptions and general scope of Freiesleben's work.

My opponent seems to have been misled, and to have adopted this notion, partly by misconceiving the true import of a term, and partly by taking an imperfect view of the series connected with the coal in the Ihlefeld, Opperode, and Petersberge districts. In the first of these, which ranges to the south-east past Neustadt, the constituent members appear (so far as they are exposed) arranged in the following ascending order:\*

1. Coarse grained *rothliegende*, becoming gradually finer.
  2. Fine grained *rothliegende*.
  3. Common indurated clay.
  4. Floor shale.
  5. *Coal*, divided by intervening shale into three layers, 10, 8, and 6 inches thick respectively, forming altogether a seam 30 inches thick.
  6. Roof shale.
- The roof shale and floor shale, as well as that which divides the coal seam, contain impressions of ferns and reeds.
7. Thin slaty indurated clay.
  8. Trap, for a short distance, which is partly amygdaloidal.
  9. Porphyry, extensively, in abrupt cliffs, and in mountain masses.

10. *Rothliegende*.

The real purport of the term *rothliegende* in this series has been already explained; and I need here only add, that the group (being succeeded by the *weissliegende* or new conglomerate, and the cupriferous marl shale), gradually thins off to the eastward, until in the district of Questenberg, the *weissliegende*

\* See Freiesleben, vol. iv. p. 175—178, and also p. 146, 147; likewise vol. i. p. 43, 44.

the necessity of further comment. Nor should I have been drawn thus far into controversy, had I not considered the exact determination of the question to be of primary importance, in ascertaining the true relations of a portion of the structure of the earth.

Since, however, the term *rothetodtliegende* formation has been productive of so much misapprehension in the minds of foreigners,\* I venture to suggest to German geologists the expediency of abstaining from the use of it altogether. By whatever means a more perfect harmony might be established between the British and German descriptions, it could not fail to redound to the advantage of science. This object might be readily attained if German writers, adopting in part the language of English geologists, would for the future express the group by the term "first floetz, or carboniferous series," instead of "*rothetodtliegende* formation," and the individual members (*whenever circumstances will admit of the distinct division*) by those of "old red sandstone, carboniferous limestone, and coal formation."† And, in like manner, both countries might speak the same language if the succeeding group were designated by the expression "second floetz, or gypseous and saliferous series," and its individual members by those of "calcareous conglomerate, lower alpine or gypseous limestone, and new red sandstone, formations."

I shall close this paper by adverting to a few remarks contained in the memoir of Mr. Conybeare, referred to above, which require notice, being connected with the present question.

1. My information respecting the Portishead case was derived from the "order of superposition of strata" of Prof. Buckland, appended to Phillips's *Outlines of the Geology of England and Wales*, 1818. I find that Mr. Greenough also rested on the same authority, when stating that imperfect coal in thin beds occurred in the lower part of the old red sandstone (See group, No. 22, of the Geological Map of England and Wales). But the position being now retracted, the quotation becomes of course invalid.

2. It is stated by Mr. Conybeare, that "the coal seams which occur in the tract of the carboniferous limestone are reduced to slight traces, which have never yet been worked."

This statement does not correspond with the representation

\* E. g. Omalius d'Hallo, who has erroneously applied the term *rothetodtliegende* to conglomerates belonging to the gypseous and saliferous series.—(See that author's *Geological View of the adjacent Parts of France and the Netherlands*, in the 24th vol. of the *Journal des Mines*.)

† Von Raumer, in his description of the carboniferous series of Lower Silesia, the county of Glatz, and part of Bohemia and Upper Lusatia, avoids the expressions *roth-Regende*, *totdliegende*, and *rothetodtliegende* altogether; designating the general series by the term "the red sandstone (*rother sandstein*) formation."—(See the *Annals of Philosophy*, Oct. 1821, and Aug. 1822.)

of Dr. Boué in Scotland, from which it appears that the greater number of the Scotch collieries are situated in that portion of the tract which abounds in limestone.\*

According to that author, the carboniferous series, designated by him under the general term of *terrain du grès rouge*, or *la grande formation de grès rouge*,† appears to be arranged in the following order in the great coal tract of Scotland :

1. Old red sandstone. Poudingues et grès rouges (in a restrictive sense).‡
2. Coal formation. Grès houiller, divided into
  - a. Lower portion. Assises inférieures.
  - b. Upper portion. Assises supérieures.
  - c. Uppermost portion. Assises les plus supérieures.

The *old red sandstone*, as the foundation of the whole, contains beds of trap and felspar rocks (partly porphyritic and amygdaloidal), with which it also alternates, and likewise some beds of limestone.

The *lower portion* of the *coal formation* is characterised by an inconsiderable quantity of coal, by variable masses of anthracite, by beds of trap and felspar rocks (partly porphyritic and amygdaloidal), by beds of limestone, and by sandstone that is sometimes of a *reddish* hue.

In the *upper portion* of the *coal formation*, the trap beds seem gradually to disappear, and then follows only a *fine series of coal measures*, associated with numerous alternating *beds of limestone*. It is in this portion of the coal tract that the greatest number of the Scotch collieries are situated.

The *uppermost portion* of the *coal formation* is distinguished by the absence of beds of limestone, by its abundance of coal and of vegetable impressions, and by the appearance of shells resembling freshwater species. This portion of the tract, which most nearly agrees in its general relations with the great coal fields of England, is, however, of rare occurrence in Scotland, and appears to be confined to certain parts of Clackmannanshire, and the environs of Falkirk and St. Andrew's. Of the Clackmannanshire coal fields, a very able account has been given by Mr. Bald in the *Wernerian Memoirs*.

Dr. Boué dwells in particular on the numerous beds of limestone that are distributed throughout the greater portion of the Scotch collieries, and on the few localities in the coal tract in which that mineral is found wanting.§

\* I have briefly referred to Dr. Boué's account of the coal fields of Scotland in my Comparative View in the *Annals of Philosophy*, Oct. 1821, and again in Aug. 1822.

† See *Essai Géologique sur l'Ecosse*, p. 98—102; also p. 163, 362, 371, &c.

‡ See its description, p. 102—119 of the *Essai Géologique sur l'Ecosse*.

§ It was, I presume, from observing the intimate state of association prevailing among the different members of the carboniferous series in Scotland (the extreme fundamental part only being free from coal, and the extreme highest part only being destitute of limestone), that Prof. Jameson was induced to follow the German method by ranging the

It is important both to the landed proprietor and the miner to know, that valuable seams of coal do sometimes occur within the domain of the carboniferous limestone; and in Scotland, it is manifest that the limestone and coal are frequently found in alternation. Nor is the carboniferous limestone of the north-east of England wholly free from them, as shown in Mr. Winch's valuable paper in the fourth volume of the Geological Transactions.\*

I have upon former occasions adduced these and other examples to show, that though a general order may be every where perceived in the arrangement of the carboniferous series, yet that this order is subject to variation *in detail* in different countries, and even in the same tract of country. Now, it being established that in some countries the carboniferous limestone is productive of valuable seams of coal; if it happen in another country, that the carboniferous limestone is almost or entirely wanting, and the old red sandstone and coal formation are thus found in a state of juxtaposition, why should it be thought

whole under one great head, entitled "the first floetz sandstone, or *old red sandstone formation*."\* Applied in this manner, the expression is equivalent to that of the "old or first floetz sandstone, or *rothetodliegende formation*," of the Germans, and to the "first floetz series," or "carboniferous series," of English geologists. These collective terms, like the "grande formation de grès rouge" of Dr. Boué, and "the red sandstone formation" of Von Raumer, are but various modes of expressing the same complex idea. The one, rightly understood, is as significant as the other. But the subdivisions adopted by English geologists, when applicable, render the subject more distinct.

\* I subjoin the following extracts from Dr. Boué's *Essai Géologique sur l'Ecosse*, in which his general view of the coal formation of Scotland is exhibited.

P. 168. "*Les assises inférieures (du grès houiller) sont caractérisées par une quantité peu considérable de houille souvent sèche, par des amas variables d'anthracite, par des couches trappéennes et feldspathiques, par des calcaires compactes contenant des corps marins, et quelquefois par des grès rougeâtres.*"

"*Dans les assises supérieures, les couches trappéennes semblent disparaître, et il n'y a plus qu'une belle série des grès houillers associés avec des calcaires en partie compactes, en partie rendue sublamellaires par des débris d'êtres marins, et en partie marneux, empâtant des coquillages et des morceaux de végétaux.*"

"On observe cependant encore çà et là, des dépôts charbonneux où les calcaires paraissent manquer entièrement ou presque entièrement, où les impressions de fougères et de plantes marécageuses monocotylédones sont extrêmement abondantes, et où il y a des lits contenant des coquillages voisins de certaines bivalves fluviatiles; et l'on est amené à soupçonner d'après ces caractères, et d'après les analogies géognostiques, qu'il serait possible que ces dernières parties fussent les portions les plus supérieures de cette grande déposition houillère."

P. 193. "*Les assises supérieures du dépôt charbonneux forment presque seules pour le mineur le véritable terrain houiller exploitable; néanmoins certaines parties, avons nous dit, méritent d'être distinguées à cause de leur manque de couches calcaires, leur abondance de houille et d'impressions végétales et leurs coquilles fossiles fluviatiles; ces dernières espèces de dépôts fort considérables en Angleterre sont rares en Ecosse, et je ne puis placer parmi eux, que certaines parties de Clackmannanshire et des environs de Falkirk et de St. Andrews, sans vouloir aucunement leur assigner une place exacte.*"

P. 352. "*Dans la série houillère, nous avons surtout insisté sur la quantité considérable de couches de calcaire à encrines que contenaient la plupart des houillères Ecossoises, et sur le petit nombre de localités où les calcaires venaient à manquer presque entièrement, et où l'on apercevait quelques coquillages fluviatiles.*"

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\* See Notes to Cuvier's Essay on the Theory of the Earth, Third Edition, p. 287.

improbable that the old red sandstone itself should contain incidentally single beds of coal? Applying this observation, the single beds of coal adverted to by Freiesleben as lying in the rothliegende, in contradistinction to the continuous coal tract, will be found to imply nothing more than the distinction now taken.

It follows from all that has been advanced, that to lay down rigid rules with respect to the *details* of any particular series, forming part of the structure of the earth, so that they should be always applicable in the same manner in different countries, is to expect nature to appear in shackles, which she is not in the habit of wearing. The prevalence of a general order of succession is indisputable; but no less so is the variable state in which correlative members of the same series are associated together; being found in one country distinct from, and in another more or less frequently interstratified, with each other. Of this truth, the members of the carboniferous series afford many illustrative examples; while the limestone forms in general the great connecting link between the fundamental old red sandstone free from coal, on the one hand, and the coal measures free from limestone, on the other.

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When on the eve of transmitting the preceding pages to the press, the *Annals of Philosophy* for March, 1823, arrived, containing the continuation of Mr. Conybeare's interesting memoir. I perceive no reason to alter any thing that I have written, professing, as I do, to have faithfully expounded the positions of Freiesleben; positions quite in accordance with analogous relations in Great Britain. It is for those geologists who advocate doctrines in opposition to demonstrate their fallacy. Here, however, I cannot avoid complaining that, while the authority of Freiesleben is repeatedly appealed to in the course of this controversy, his distinctions are not only frequently suffered to pass without due attention, but his statements are tried by a language in a great measure foreign to his own. The work of Freiesleben should be judged not partially, but as a consistent whole, taken all together.

With great respect for my adversary, as well as for Prof. Buckland, I cannot surrender my opinion of the accuracy of a writer (without ample proof to the contrary), who, during a residence of seven years in a country, made its geological relations an express object of his study; comparing them also with those of analogous tracts in other parts of Germany, both by his own researches, and those of other naturalists. The opinion attributed by my opponent to Von Humboldt (an illustrious name, and carrying weight with it on any subject), will not, I apprehend, on due examination, be found at all discordant with the

statements of Freiesleben.\* That of Dr. Boué, expressing that what is *exclusively* called rothetodtliegende by Mr. Conybeare, is not the old red sandstone of *English* geologists, will be readily acceded to, since the former constitutes in fact (with the exception of the weissliegende) the upper part of the carboniferous series of Freiesleben; but, let it be remembered, this is only one part of the question, and not inconsistent with the general view of that author.† The account given by D'Aubuisson also, though insufficient in several respects, is not incompatible with this view, by whom indeed the rothetodtliegende is expressly referred to the *same formation* as the *terrain houiller*, composing the principal part of its mass.‡ The language of Beudant likewise is of a similar import, who also expressly ascribes the rothliegende in question to the same series as the carboniferous, designating the latter by the general term of the *red sandstone formation*, and comparing it with that of Scotland as described by Prof. Jameson, Dr. Mac Culloch, and Dr. Boué.§ On this subject, Mr. Conybeare observes, "whether it (namely, the rothetodtliegende *exclusively* so called by him) be more properly referable to the upper part of the carboniferous, or the lower part of the saliferous series, is a distinct question, on which much division of opinion exists, and which is after all not very material. I have endeavoured to compromise the matter by treating it as an intermediate link between them." I cannot subscribe to this doctrine. There can be no compromise upon a question of truth and error; nor can it surely be deemed immaterial to which series the rothliegende in dispute belongs, if we place a just value on a correct knowledge of the respective relations of the two series. My surprise is, that any division of

\* A reference, however, to the Comparative View taken by that distinguished naturalist of the English and Continental floetz formations, will at once show that it is deficient in several important particulars.—(See D'Aubuisson, *Traité de Géognosie*, vol. ii. p. 225.)

† See Dr. Boué in vol. iv. of the *Wernerian Memoirs*.

‡ See *Traité de Géognosie*, vol. ii. p. 252. "Le grès rouge, que nous appellerons *grès houiller*, le terrain à houille en faisant partie, repose immédiatement sur le terrain primitif ou intermédiaire." P. 263. "La grande formation de *grès houiller* se divise très convenablement en deux parties; l'une comprend le *terrain houiller* proprement dit; et l'autre le grès, appelé, dans la Thuringe, *grès rouge*, avec ses couches subordonnées: mais tout en distinguant ces deux parties, nous remarquerons qu'elles appartiennent à la même formation; et quoique le terrain houiller soit le plus souvent au-dessous, il lui arrive quelquefois d'être *entremêlé* et même d'être *superposé* au grès rouge." And p. 306. "Le grès, masse principale du terrain houiller, prend souvent une grande extension, en abandonnant, au moins en majeure partie, la houille avec l'argile shisteuse qui l'enveloppe, et il constitue des terrains d'une grande étendue. Il a été principalement observé en Thuringe, où il est connu sous le nom de *rothetodtliegende*. Werner le nomme *grès rouge*, par suite de sa couleur habituelle dans ce pays."

§ See e. g. the extract from that author's *Travels in Hungary*, inserted in the 14th number of the *Edinburgh Philosophical Journal*, in particular, p. 269, and 273; and more generally, p. 267—273, which convey a clear account of the geological relations of the *Saxon pitchstone*, as well as of that of Italy (at Grantola on the Lago Maggiore). In both cases, the pitchstone is associated with porphyry, red sandstone, and conglomerate, the whole of which are referred to the carboniferous series.

opinion should exist after the detailed exposition of Freiesleben, which tended to reconcile all differences; and that greater weight should not have been attached to the judgment and descriptions of that author, the correctness of which upon other subjects has not hastily been called in question. But be this as it may, I am not aware that any German writer ever included any portion of the rothetodtliegende formation in the gypseous and saliferous series; while many, on the contrary, did comprehend the weissliegende in the carboniferous series. The former position, therefore, assumed by my antagonist, is in opposition to all German authority.\*

The propagation of error is sometimes as rapid as simple. A

\* If there be any one series in geology more distinct than another, as constituting in itself a complete system, wholly independent of preceding and subsequent series, it is the carboniferous. It is true that in its lower line of boundary, where imposed upon transition tracts, we often hear graduations spoken of, as taking place from the one series into the other. These, however, can be so considered only in a *mineralogical* sense (particularly when the transition sandstone, one of the later members of that series, and the first floetz or old red sandstone come in contact): certainly not in a *geological* sense; for though, in the first place, the two series may in certain quarters be in a conformable position, yet if the line of apposition be examined throughout its extent, a general unconformability in the arrangement of their respective strata will be found to prevail, the carboniferous series being merely adapted to the *form of the surface* of the transition (or primary, as it may happen); and, in the second place, the transition series is commonly distinguished by a considerable variety of trilobites and other organic remains, while the first floetz or old red sandstone is free from such remains; and the only trilobite that I am acquainted with (beside the *Oniscites Derbiensis* of Martin) as occurring in the carboniferous limestone is a distinct species, and, I believe, also peculiar to that limestone.\*

The upper confines of the carboniferous series are also equally well marked. In England, the calcareous or new conglomerate (the first member of the gypseous and saliferous series) is, I apprehend, invariably found in an unconformably overlying position; in some quarters partially overspreading the surface of the coal fields, and extending thence in like manner to the carboniferous limestone, and even to the old red sandstone. The weissliegende or new conglomerate in Germany also, with its companion the cupriferous marl shale, is represented by Freiesleben as partially overspreading the carboniferous series, conforming to the figure of its surface, following its sinuosities, and surrounding the detached portions of that series that appear in isolated hills; being also in certain quarters in contact with transition tracts. Hence arises a variableness in the range and dip of the weissliegende, which are sometimes conformable, sometimes unconformable to the disposition of the subjacent members of the carboniferous series. That the weissliegende or new conglomerate is wholly distinct from that series is also proved; 1. By its several affinities to the cupriferous marl shale; and 2. By its being affected in common with that shale, and with the lower alpine limestone in general, and the zechstein in particular, by various disturbances, which do not extend to the carboniferous series beneath.—(See e.g. Freiesleben, vol. iii. p. 51, et seq. and p. 239, et seq.; also vol. iv. p. 31—31.)

I may here remark, that in one respect there is a marked difference between the composition of the calcareous or new conglomerate of England, and that of the weissliegende of Germany. In the former, rounded and angular fragments of limestone are very common, and frequently predominant; while in the latter they are of rare occurrence. Both conglomerates thus bear a close relation to the carboniferous series on which they respectively repose, and from whose detritus they were principally derived; the limestone frequently prevailing in the British carboniferous series, and being, comparatively speaking, only incidental in the German.

\* It is the tuberculated species depicted in pl. 4, fig. 12, of Brongniart's valuable *Histoire Naturelle des Trilobites*, derived from the Dublin limestone. I found four specimens of the same trilobite in the Mendip limestone. Mr. Miller has observed it likewise in the limestone of Cork and Bristol. It has been met with also incidentally in the carboniferous limestone of other parts of Great Britain.

340      *Mr. Smithson on the Crystalline Form of Ice.* [MAY, judgment is pronounced by a name of celebrity; it is adopted and repeated by other names, perhaps of equal repute; and thus that which was originally a mistake, becomes a rule established by authority. But on recurring to first principles, the error is discovered, and truth at length prevails.

To conclude in the words of Lord Bacon, "the harmony of a science, supporting each part the other, is, and ought to be, the true and brief confutation and suppression of all the smaller sorts of objections."

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## ARTICLE II.

*On the Crystalline Form of Ice.* By James Smithson, Esq. FRS.

(To the Editor of the *Annals of Philosophy*.)

SIR,

March 4, 1823.

I HAVE just seen a memoir in the *Annales de Chimie et de Physique* for Oct. 1822, but published about a month ago, on the crystalline form of ice.

Mr. Hericart de Thury is said to have observed ice in hexagonal and triangular prisms; and Dr. Clarke, of Cambridge, in rhomboids of  $120^\circ$  and  $60^\circ$ .

M. Haüy supposed the form to be octahedral, and so did Romé de l'Isle; and, if I mistake not much, there is in an ancient volume of the *Journal de Physique* by Rozier, an account of ice in acute octahedrals.

Are these accounts and opinions accurate?

Hail is always crystals of ice more or less regular. When they are sufficiently so to allow their form to be ascertained, and which is generally the case, it is constantly, as far as I have observed, that of two hexagonal pyramids joined base to base, similar to that of the crystals of oxide of silicium or quartz, and of sulphate of potassium. *One of the pyramids is truncated*, which leads to the idea that ice becomes electrified on a variation of its temperature, like tourmaline, silicate of zinc, &c.

I do not think that I have measured the inclination of the faces more than once. The two pyramids appeared to form by their junction an angle of about 80 degrees.

Snow presents in fact the same form as hail, but imperfect. Its flakes are skeletons of the crystals, having the greatest analogy to certain crystals of alum, white sulphuret of iron, &c. whose faces are wanting, and which consist of edges only.

In spring and autumn; that is, between the season of snow and that of hail, the hail which falls partakes of the nature of both, is partly the one and the other; its crystals, though regular, are opaque, of little solidity, and consist, like snow, of an imperfect union of grains, or smaller crystals.

## ARTICLE III.

*Account of some Specimens of Rocks, &c. from Van Dieman's Land, and from New South Wales.* By N. J. Winch, Esq. Hon. MGS.

(To the Editor of the *Annals of Philosophy*.)

SIR,

Newcastle-upon-Tyne, March 21, 1823.

THE Literary and Philosophical Society of this place has lately been presented with a considerable number of minerals by the Rev. T. H. Scott, of Whitfield, collected by himself in Van Dieman's Land and New South Wales, during his travels in those distant regions. And notwithstanding duplicate specimens, and probably many which were not duplicates, had been given to the Universities of Cambridge and Oxford, and the Geological Society of London, still we possess a sufficient number to throw considerable light on the structure of Australia. By many theorists it has been conjectured that these lands are of more recent formation than those of the other parts of the globe, but with how little justice such an opinion has been adopted, the following brief description of some of the best defined species of our collection will evince.

*From Van Dieman's Land.*

1. A hard slaty sandstone of a pale-brown colour with impressions of flustræ, resembling those of Humbledon Hill, near Sunderland. Pectenites, and some other species of bivalve shell, in size and shape like a hazel nut. From a hill near Hobart's Town, 800 feet above the level of the sea.

2. A very fine grained white sandstone. Near Hobart's Town. If in sufficient quantity, this must be a valuable material for the purposes of building.

3. Coal, of the same species as the Newcastle coal. From Adventure Bay, eight miles distant from Hobart's Town.

4. Black bituminous shale, with spangles of silvery mica, and slight impressions of the leaves of some phænogamous plant. Above and below the coal at Hobart's Town.

5. Limestone, of a brownish-grey colour, compact texture, and splintery fracture, with veins of white quartz. Eight miles north-west of Hobart's Town. Not unlike some of the beds in our encrinal limestone formation.

6. A bluish grey trap rock, resembling the blue millstone rock of Andernach on the Rhine. Its structure is cellular, the cells containing minute globules of black obsidian, occasionally coated by a thin pellicle of iron ochre, owing to decomposition. The rocky part is easily reduced to a black slag, by the action of the blowpipe, and with the addition of borax, melts into a pale green glass filled with air bubbles. The obsidian is reduced with difficulty into a brilliant black glass. Masses of this rock

are found on stony plains, and it is used as mill stones in the colony.

*From New South Wales.*

1. Coal, resembling that of the north of England.
2. Coal shale, of an ash-grey colour, with impressions of the leaves of some phænogamous plant, probably an eucalyptus. These leaves are lanceolate, from four to six inches in length, by one or one and a half inch in breadth, and have left black impressions on the stone.
3. Wood, mineralized by siliceous matter; the interior of a dark-brown colour, and compact texture, the exterior, formed on the sap wood, pale-brown, and containing longitudinal pores.
4. Old red sandstone, of a dark reddish-brown colour, and made up of small grains of sand, and silvery mica, and enveloping rounded pebbles of white quartz. Pitt's Amphitheatre.
5. Greywacke, consisting of greenish-grey clayslate, inclosing small fragments of chesnut-brown flinty slate, and specks of calcareous spar. From the hills beyond Bathurst.
6. Chlorite slate, of a greenish-grey colour and silky lustre. From the same hills.
7. Gneiss, composed of white felspar, black mica, and glassy quartz. From Cox's river.
8. Granite, consisting of fine grained white felspar, glassy quartz, and silvery mica. Near Cox's river.
9. Large grained granite, chiefly composed of flesh-red felspar, with glassy quartz and silvery mica. From Lawson's Peak, beyond the Blue Mountains.
10. Epidote, of a pale-green colour, and granular texture.
11. Felspar porphyry, of a yellowish and greenish-white, with crystals of the same colour, mixed with opalescent quartz. Fragment of a rounded mass.
12. Rock crystal, six-sided prismatic crystals, of a smoke-grey colour, terminated by six-sided pyramids.
13. Clay ironstone, of a reddish-brown colour, in nodules.
14. Bog iron ore, of the same colour.
15. Bole, of a bright-red colour.
16. Iron pyrites.

From the examination of these minerals (provided the series be complete), the conclusions to be drawn are, that, with the exception of the diluvium, no formation more recent than our magnesian limestone, has been found in Van Dieman's land, or our coal formation in New South Wales. That the mountain limestone, old red sandstone, greywacke, porphyry, clayslate, chlorite slate, gneiss, and granite, follow each other in the same order of succession, as is the case in other parts of the world; that no pumice or recent lava have been detected; and that the most remarkable phenomenon is the existence of impressions of leaves of phænogamous plants in the shales.

N. J. Winch.

## ARTICLE IV.

*On Reaumur's Experiments on the Congelation of the Metals.*

By Mr. J. B. Longmire.

(To the Editor of the *Annals of Philosophy*.)

SIR,

April 3, 1823.

THE experiments of Reaumur on cast-iron, bismuth, and antimony, lead to the conclusions, that these metals expand during congelation, and are lighter when solid than when fluid. But as such conclusions are at variance with the general law, that heat expands all bodies whose natural state is solidity, it becomes important to show, that Reaumur's experiments do not militate against this law.

Reaumur found that the metals just mentioned, when fluid, supported bodies specifically heavier than themselves in the solid state; so that they should have contracted in melting, and would expand again in cooling.

A floating body is lighter than the fluid that supports it, *provided such fluid be quiescent*. None, however, of the melted metals, if exposed to the air, have the requisite degree of stillness, to form the fluid medium for obtaining accurate specific weights; but least of all, have cast-iron, bismuth, and antimony; metals that cool quickly, and that are in violent agitation, when passing down to the point of congelation. Fluid iron, for instance, has on its surface, bright glowing waves, rapidly repeated in variable situations; the hottest particles from below rise to the surface in these waves shed laterally to cool, and while they sink, others ascend, and give out part of their heat: this motion is repeated till the iron begins to consolidate. The other metals before-mentioned are nearly as much disturbed in cooling as fluid iron; hence in this state they are all too much agitated to determine with accuracy the difference of density between their solid and fluid states. So it follows that Reaumur's experiments depending on the aptitude of those melted metals for this purpose, are inconclusive, and cannot be considered as proving any thing against the general law, that all bodies, whose natural state is solidity, are heaviest in this state, and expand on receiving heat.

The force of the ascending particles in the melted metals before-mentioned, will account for the extra specific weight of the bodies that they support. But the amount of the ascensional force is not determinable; and whether these metals contract or expand in cooling is not yet shown by experiment. Such an experiment indeed is not easily performed; but to reconcile demonstratively the congelation of these metals with the law regulating that of all the others, is a desirable and important undertaking.

## ARTICLE V.

*Astronomical Observations, 1823.*

By Col. Beaufoy, FRS.

*Bushey Heath, near Stanmore.*Latitude  $51^{\circ} 37' 44.3''$  North. Longitude West in time  $1^{\circ} 20.93''$ .

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Mar. 30.	Emersion of $\pi$ Scorpio from the moon. ....	{	12h	54'	16.4"	Mean Time at Bushey.
April 12.	Emersion of Jupiter's first satellite .....	{	8	53	55	Mean Time at Bushey.
		{	8	55	16	Mean Time at Greenwich.

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## ARTICLE VI.

*On the Ultimate Analysis of Vegetable and Animal Substances.*

By Andrew Ure, MD. FRS.\*

THE following is an account of Dr. Ure's process and apparatus for the ultimate analysis of vegetable and animal compounds, as described in this paper. The French chemists who have operated most with peroxide of copper in the analysis of organic substances, being aware of its quick absorption of humidity from the atmosphere, direct it to be used immediately after ignition, and to be triturated with the organic matter in a hot mortar of agate or of glass. "Yet this precaution," Dr. Ure observes, "will not entirely prevent the fallacy arising from the hygrometric action; for I find that peroxide thus treated does absorb, during the long trituration essential to the process, a certain quantity of moisture, which, if not taken into account, will produce serious errors in the analytical results. It is better, therefore, to leave the powdered peroxide intended for research, exposed for such time to the air, as to bring it to hygrometric repose, then to put it up in a phial, and by igniting 100 grains of it in a proper glass tube, sealed at one end, and loosely closed with a glass plug at the other, to determine the proportion of moisture which it contains. This, then, indicates the constant quantity to be deducted from the loss of weight which the peroxide suffers in the course of the experiment. The mortar should be perfectly dry, but not warm."

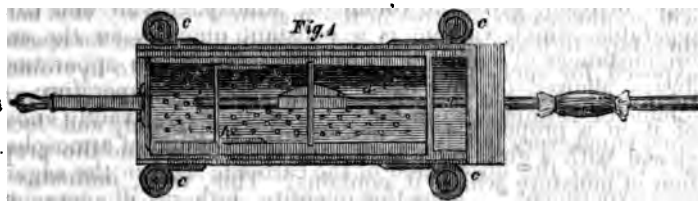
"Experimenters have been at great pains to bring the various organic objects of research to a state of thorough desiccation before mixing them with the peroxide of copper; but this prac-

\* Abstracted from the Philosophical Transactions for 1822. Part II.

tice introduces a similar fallacy to that above described. . . . . The plan which I adopt for the purpose of desiccation seems to answer very well. Having put the pulverulent animal or vegetable matter into short phials, furnished with ground glass stoppers, I place the open phials in a large quantity of sand heated to  $212^{\circ}$  F. in a porcelain capsule, and set this over a surface of sulphuric acid in an exhausted receiver. After an hour or more, the receiver is removed, and the phials instantly stopped. The loss of weight shows the total moisture which each of them has parted with; while the subsequent increase of their weight, after leaving them unstopped for some time in the open air, indicates the amount of the hygrometric absorption. This is consequently the quantity to be deducted in calculating experimental results."

"Many chemists, particularly in this country, have employed the heat of a spirit-lamp, instead of that produced by the combustion of charcoal, for igniting the tube in which the mixed materials are placed. I have compared very carefully both methods of heating, and find that for many bodies, such as coal, and resin, which abound in carbon, the flame of the lamp is insufficient; while its application being confined at once to a small portion of the tube, that uniform ignition of the whole, desirable towards the close of the experiment, cannot be obtained.\* I was hence led to contrive a peculiar form of furnace, in which, with a handful of charcoal, reduced to bits about the size of small filberts, an experiment may be completed without anxiety or trouble, in the space of half an hour. Since I have operated with this instrument, the results on the same body have been much more consistent than those previously obtained with the lamp; and it is so convenient that I have sometimes finished eight experiments in a day."

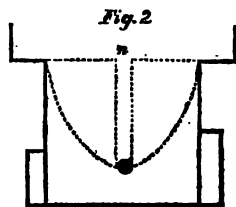
.. Dr. Ure next gives a particular account of the whole apparatus he employs, illustrated with an engraving. "Fig. 1 is an



horizontal section of the furnace, in which we perceive a semi-cylinder of thin sheet iron, about eight inches long, and three

\* Some remarks, by Dr. Prout, on the relative accuracy of analyses performed by means of the charcoal and of the lamp apparatus, will be found in the *Annals* for Dec. last, p. 425.—*Edit.*

and a half wide, perforated with holes, and resting on the edge of a hollow prism of tin-plate, represented more distinctly in fig. 2, where *n* shows a slit, through which the sealed end of the glass tube may be made to project, on occasion. Fig. 1, *i* is a handle attached to the semi-cylinder, by which it may be slid backwards or forwards, and removed at the end of the process. *d* is a sheath of platinum foil, which serves, by aid of a wire laid across, to support the middle of the tube, when it is softened by ignition. At *g* the plates which close the ends of the semi-cylinder and tin-plate prism, rise up a few inches to screen the pneumatic apparatus from the heat."



A third occasional screen of tin-plate is hung on for the same purpose. "All these are furnished with slits for the passage of the glass tube. This is made of crown glass, and is generally about nine or ten inches long, and three-tenths of internal diameter. It is connected with the mercurial cistern by a narrow tube and caoutchouc collar. This tube has a syphon form, and rises about an inch within the graduated receiver. By this arrangement, should the collar be not absolutely air-tight, the pressure of the column of mercury causes the atmospheric air to enter at the crevice, and bubbles of it will be seen rising up without the application of heat. At the end of the operation, the point of the tube is always left above the surface of the mercury, the quantity of organic matter employed being such as to produce from six to seven cubic inches of gaseous product, the volume of the graduated receiver being seven cubic inches."

"As the tubes with which I operate have all the same capacity, viz. half a cubic inch; and as the bulk of materials is the same in all the experiments, one experiment on the analysis of sugar or resin, gives the volume of atmospheric air due to the apparatus, which volume is a constant quantity in the same circumstances of ignition. And since the whole apparatus is always allowed to cool to the atmospheric temperature, the volume of residual gas in the tubes comes to be exactly known, being equal, very nearly, to the primitive volume of atmospheric air left after the absorption of the carbonic acid in the sugar or resin experiment.\* Thus this quantity, hitherto ill appreciated or neglected in many experiments, though it is of very great consequence, may be accurately found. At *k*, fig. 1, a little tin-plate screen is shown. It is perforated for the passage of the

\* If *a* be the capacity of the graduated receiver, and *b* the spare capacity of the tubes, then the above volume is  $b - \frac{b}{a + b}$ .

tubé, and may be slid along, and left at any part of the semi-cylindric cage, so as to preserve from the influence of the heat any requisite portion of the sealed end of the tube."

For the analysis of volatile liquids, Dr. Ure employs a little bulb, which, after it has been filled and exactly weighed, "is immediately slid down to the bottom of the tube, and covered with 150 or 200 grains of peroxide of copper. The bulb has a capacity equal to three grain measures of water, and its capillary point is sometimes closed with an inappreciably small quantity of bees' wax, to prevent the exhalation of the liquid, till the peroxide be ignited."

The furnace is provided with a cover, with an oblong orifice at its top, which serves for a chimney, "and may be applied or removed by means of its handle, according as we wish to increase or diminish the heat. *c c c c* are tin cases inclosing corks, through which the iron wires are passed, that support the whole furnace at any convenient height and angle of inclination."

"The tightness of the apparatus at the end of the process is proved by the rising of the mercury in the graduated receiver, by about one-tenth of an inch, as the tube becomes refrigerated."

"My mode of operating with the peroxide of copper," continues Dr. Ure, "is the following :

"I triturate very carefully in a dry glass mortar from one to two and a half grains of the matter to be analyzed, with from 100 to 140 grains of the oxide. I then transfer it by means of a platinum-foil tray and small glass funnel, into the glass tube, clearing out the mortar with a metallic brush. Over that mixture, I put 20 or 30 grains of the peroxide itself, and next, 50 or 60 grains of clean copper filings. The remaining part of the tube is loosely closed with 10 or 12 grains of amianthus, by whose capillary attraction the moisture evolved in the experiment is rapidly withdrawn from the hot part of the tube, and the risk of its fracture thus completely obviated. The amianthus serves moreover as a plug, to prevent the projection of any minute particles of filings, or of oxide, when the filings are not present. The tube is now weighed in a very delicate balance, and its weight is written down. A little cork, channelled at its side, is next put into the tube, to prevent the chance of mercury being forced backwards into it, by any accidental cooling or condensation. The collar of caoutchouc is finally tied on, and the tube is placed, as is shown in fig. 1, but without the plate *k*, which is employed merely in the case of analysing volatile liquids. A few fragments of ignited charcoal are now placed under the tube at the end of the furnace next to the cistern, and the remaining space in the semi-cylinder is filled up with bits of cold charcoal. The top may then be put in its place, when the operation will proceed spontaneously, the progressive advance

of the ignition from one end to the other being proportioned to the expansion of glass, so that the tube very seldom cracks in the process. Indeed I have often used the same tube for a dozen experiments, in the course of which it became converted into *vitrite*, or Reaumur's porcelain."

Since the evolved gas is saturated with moisture, Dr. Ure reduces it to the volume of dry gas, by help of a table, which he gives, computed by the well-known formula from his table of the elastic force of steam, published in the Phil. Trans. for 1818.

In certain cases, where the quantity of hydrogen is small, or where, as in the example of indigo, its presence has been denied, Dr. Ure employs pulverulent calomel instead of peroxide of copper. "The organic compound being intimately mixed with that powder, and gently heated, the muriatic acid gas obtained demonstrates the presence, though half of its volume will not give the total quantity of hydrogen; for a proportion of this elementary body continues associated with oxygen in the state of water."

Dr. Ure next gives, in detail, one example of the mode of computing the relation of the constituents from the experimental results, and then states the other analyses in a tabular form.

"1.4 grain of sulphuric ether, specific gravity 0.70, being slowly passed in vapour from the glass bulb through 200 grains of ignited peroxide of copper, yielded 6.8 cubic inches of carbonic acid gas at 66° Fahr. which are equivalent to 6.57128 of dry gas at 60°. This number being multiplied by 0.127 = the carbon in one cubic inch of the gas, the product 0.8345256, is the carbon in 1.4 grain of ether; and  $0.8345256 \times \frac{3}{4} = 2.2254$  = the oxygen equivalent to the carbonic acid. The tube was found to have lost 4.78 grains in weight, 0.1 of which was due to the hygrometric moisture in the oxide, and 1.4 to the ether. The remainder, 3.28, is the quantity of oxygen abstracted from the oxide by the combustible elements of the ether. But of these 3.28 grains, 2.2254 went to the formation of the carbonic acid, leaving 1.0546 of oxygen, equivalent to 0.1318 of hydrogen. Hence, 1.4 ether, by this experiment, which is taken as the most satisfactory of a great number, seem to consist of

Carbon. ....	0.8345
Hydrogen. ....	0.1318
Water. ....	0.4337
	<hr/>
	1.4000

And in 1 grain we shall have

Carbon. ....	0.5960	....	3 atoms	2.25	....	60.00
Hydrogen. ..	0.1330	....	4 atoms	0.50	....	13.33
Oxygen. ....	0.2710	....	1 atom	1.00	....	26.66
	<hr/>			<hr/>		<hr/>
	1.0000			3.75		100.00

Or, 3 volumes olefiant gas =  $3 \times 0.9722 = 2.9166$   
 2 volumes of water. ....  $2 \times 0.625 = 1.25$

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4.1666

which suffering a condensation equal to the whole vapour of water, will give an ethereous vapour, whose specific gravity is 2.5.

"The proportion of the constituents of sulphuric ether, deduced by M. Gay Lussac from the experiments of M. Th. de Saussure, are 2 volumes olefiant gas + 1 volume vapour of water, which 3 volumes are condensed into 1 of vapour of ether, having a specific gravity = 2.58. The ether which I used had been first distilled off dry carbonate of potash, and then digested on dry muriate of lime, from which it was simply decanted, according to the injunction of M. de Saussure. Whether my ether contained more aqueous matter than that employed by the Genevese philosopher, or whether the difference of result is to be ascribed to the difference in the mode of analysis, must be decided by future researches."

"By analogous modes of reduction, the following results were deduced from my experiments. I ought here to state, that in many cases the materials, after being ignited in the tube, and then cooled, were again triturated in the mortar, and subjected to a second ignition. Thus, none of the carbon could escape conversion into carbonic acid. I was seldom content with one experiment on a body; frequently six or eight were made."

TABLE OF ORGANIC ANALYSES.

	Substance.	Carbon.	Hydrogen.	Oxygen.	Azote.	Water.	Excess.
1	Sugar .....	43.38	6.29	50.33		56.62	
2	Sugar of diabetes .....	39.52	5.57	54.91		51.13	Oxygen
3	Starch .....	38.55	6.13	55.32		55.16	10.35
4	Gum arabic .....	35.13	6.08	55.79	3?	54.72	6.03
							7.15
5	Resin .....	73.60	12.90	13.50		15.20	Hydrogen
6	Copal .....	79.87	9.00	11.10		12.05	11.20
7	Shell lac .....	64.67	8.22	27.11		30.51	7.06
8	Resin of gualac .....	67.88	7.05	25.07		28.00	4.82
9	Amber .....	70.68	11.62	17.77		20.00	3.93
10	Yellow wax .....	80.69	11.37	7.94		8.93	9.40
11	Caoutchouc .....	90.00	9.11	0.88		0.99	10.39
12	Splint coal .....	70.90	4.30	24.30		27.90	9.00
13	Cannel coal .....	72.22	3.93	21.05	2.08	23.68	1.20
14	Indigo .....	71.37	4.38	14.25	10.00	16.00	1.30
15	Camphor .....	77.38	11.14	11.48		12.91	2.52
16	Naphthaline .....	91.06	7.07	0.70?		0.79?	9.71
17	Spermaceti oil .....	78.91	10.97	10.12		11.34	8.64
18	Common oil of turpentine .....	82.51	9.62	7.87		8.85	11.01
19	Purified oil of turpentine .....	84.09	11.05	3.06		4.00	11.73
20	Naphtha .....	83.04	12.31	4.65		5.23	8.33
21	Asiatic castor oil .....	74.00	10.29	15.71		17.67	7.25
22	Alcohol, spec. grav. 0.812 .....	47.85	12.24	39.91		44.06	0.98
23	Ether, spec. grav. 0.70... ..	59.60	13.03	27.01		30.05	

TABLE OF, ORGANIC ANALYSES.

Substance.	Carbon.	Hydrogen.	Oxygen.	Azote.	Water.	Excess.
24 Bleached silk .....	50.69	3.94	34.04	11.33	35.43	Oxygen 2.55
25 Cotton .....	42.11	5.06	52.83		45.56	12.33
26 Flax, by Lee's process. ....	42.81	5.05	51.07		49.05	7.07
27 Common flax .....	40.74	5.57	52.79	0.09	50.16	8.02
28 Wool .....	53.07	2.80	31.02	12.03	25.07	8.03
29 Cochineal .....	50.75	5.81	36.53	6.91	39.06	Hydrogen 14.01
30 Cantharides .....	48.64	5.99	36.29	9.08	40.83	14.53
31 Urea .....	18.57	5.93	43.68	31.92	49.14	0.47
32 Benzoic acid .....	66.74	4.94	28.32		31.86	1.04
33 Citric acid .....	33.00	4.63	62.37		41.67	Oxygen 25.33
34 Tartaric acid ..	31.42	2.76	65.82		24.84	43.74
35 Oxalic acid .....	19.13	4.76	76.20		42.87	38.09
36 Ferroproussic acid .....	36.82	27.89 of iron.		35.29		

“ Remarks on the preceding Analyses.”

“ The sugar which I employed,” says Dr. Ure, “ had been purified by Mr. Howard’s steam process, and was so well stove-dried, that it lost no appreciable portion of its weight, when enclosed along with sulphuric acid in *vacuo*. The diabetic sugar has a manifest excess of oxygen, which, I believe, to be the case with all weak sugars, as they are called by the sugar refiners. I consider this excess of oxygen as the chief cause which counteracts crystallization, and, therefore, the great obstacle to the manufacturer. The smallest proportion of carbon, which I have ever found in any cane sugar, was upwards of 41 per cent. The experiments on starch and gum were among the earliest which I made, and the results differ so much from those given by other experimenters, that I shall repeat the analyses at the earliest opportunity. The constituents of the above three bodies, referred to the prime equivalent scale, will be approximately as follows : \*

	Sugar.	Starch.	Gum.
Carbon .....	5 atoms	5 atoms	4 atoms
Oxygen .....	4	5	5
Hydrogen ...	4	4	4

“ Starch is liable to a similar deterioration with sugar ; that is, some species of it make a much firmer coagulum with hot water than others ; a difference probably due to the proportion of oxygen. The starch here employed was that of commerce, and was not chemically desiccated : hence, the redundancy of water beyond the equivalent proportion. A little hygrometric moisture was present also in the gum, as it was not artificially

\* The following are the equivalent numbers employed by Dr. Ure in this paper :—  
Oxygen, 10; hydrogen, 0.125; carbon, 0.15; azote, 1.75.

dried. A note of interrogation is placed after azote. That doubt will I trust be solved, when I complete my analyses of grains, roots, and leaves, with the view of tracing the origin of azote in the bodies of graminivorous animals . . . . . With regard to resin, I believe the quantity of its carbon to be somewhat underrated in the table. Though three experiments were made on it, I now perceive that I had omitted to retriturate and reignite; and the carbon of resin is very difficult of oxygenation. Its true composition is probably, carbon, 8 atoms; hydrogen, 8; oxygen, 1. A still more symmetric arrangement would be derived from carbon, 8 atoms; hydrogen, 9; oxygen, 1. This proportion corresponds to 8 atoms of olefiant gas and 1 atom of water; and I think it is very possibly the true constitution of resin. Had the loss of weight suffered by the contents of the tube, during their ignition, been a few hundredth parts of a grain more, the experimental result would have coincided with this theoretical view. Copal approaches to carbon, 10 atoms; hydrogen, 7; oxygen, 1. Lac may be nearly represented by carbon, 6 atoms; hydrogen, 4; oxygen, 2; or 2 atoms of olefiant gas + 1 atom carbonic oxide . . . . . Resin of guaiac gives carbon, 7 atoms; hydrogen, 4; oxygen, 2."

"Although the experiments on amber were conducted carefully with retrituration and reignition, no good atomic configuration of it has occurred to me. It approaches to 10 carbon + 10 hydrogen + 2 oxygen."

"Wax is apparently composed of carbon, 13 atoms; hydrogen, 11; oxygen, 1; or, in other words, of 11 atoms olefiant gas + 1 atom carbonic oxide + 1 atom carbon. Had the experiment given a very little more hydrogen, we should have had wax as consisting of 12 atoms olefiant gas + 1 atom carbonic oxide. This is possibly the true constitution."

"Caoutchouc seems to consist of carbon, 3 atoms; hydrogen, 2; or it is a sesqui-carburetted hydrogen. The oxygen deduced from experiment is in such small quantity, as to leave a doubt whether it be essential to this body, or imbibed in minute quantity from the air during its consolidation."

"Splent or slate coal, specific gravity 1.266, abstracting its incombustible ashes, approaches in constitution, to carbon, 7 atoms; hydrogen, 3; oxygen, 2. Cannel coal from Woodhall, near Glasgow, specific gravity 1.228, resembles a compound of carbon, 9 atoms; hydrogen, 3; oxygen, 2. In both of these bodies, there is an excess of carbon beyond the 3 atoms of olefiant gas and 2 of carbonic oxide. The former coal has 2 extra atoms of carbon, and the latter, 4 atoms. Hence this coal is found at the Glasgow gas works to yield a very rich burning gas."

"The elements of indigo may be grouped as follows: carbon, 16 atoms; hydrogen, 6; oxygen, 2; azote, 1; or, in other

terms, we shall have 1 atom cyanogen, 6 atoms olefiant gas, 2 atoms carbonic oxide, and 6 atoms of carbon in excess."

"I had intended to pursue, at considerable detail, my researches on this curious azotized product of vegetation, but the subject having been lately taken up, and ingeniously prosecuted by my pupil and friend, Mr. Walter Crum, I was induced to leave it in his hands. He announced to me the presence of hydrogen in indigo, before I had analyzed this substance myself; and drew my attention particularly to the fallacy occasioned by the hygrometric water of the peroxide of copper. It is likely that some slight modification may require to be made in my tabular proportion of the constituents, for I did not resume the subject of indigo, after I had become most familiar with the manipulations."

"Camphor is very nearly represented by carbon, 10 atoms; hydrogen, 9; oxygen, 1; or 9 atoms olefiant gas + 1 atom carbonic oxide. Naphthaline is, in my opinion, a solid bicarburet of hydrogen, consisting of carbon, 2 atoms; hydrogen, 1."

"It is very difficult, even by the best regulated ignition, to resolve the whole carbon of this very volatile body into carbonic acid; hence, the carbon may come to be underrated in the result." Naphthaline is obtained during the rectification of the petroleum of the coal gas works. It is found encrusting the pipes in the form of a greyish crystalline mass; and when purified by a second sublimation at the temperature of about 220°, it forms beautiful thin plates, white and glistening. It has a powerful petroleum odour. With brine of the specific gravity 1.048, these plates, when once thoroughly wetted (which is difficult to effect), remain in equilibrium; that is, float in any part of the liquid. That number, therefore, represents the specific gravity of naphthaline. It is insoluble in water, but very soluble in ether, and moderately so in alcohol. With iodine, it fuses at a gentle heat into a brown liquid, forming as it cools a solid resembling plumbago, which dissolves readily in alcohol, and is thrown down by water. Naphthaline is soluble in oils. In water heated to 168° Fahr. it fuses, and remains like oil at the bottom of the liquid; but when stirred, it rises, and spreads on the top in little oily patches. At 180° it rises spontaneously from the bottom in oily globules, which, as the temperature is raised, dissipate in the air, undergoing motions similar to those of camphor floating on water.

"Spermaceti oil is constituted apparently of carbon, 10 atoms; hydrogen, 9; oxygen, 1; or, in other words, of 9 atoms olefiant gas + 1 atom carbonic oxide. The experimental proportion is, however, more nearly carbon, 10 atoms; hydrogen, 8; oxygen, 1. There is here an atom of carbon in excess."\*

\* "This is probably the truer view. The former would make it coincide with camphor."

"Common oil of turpentine, specific gravity 0·888, comes very closely to the following arrangement: carbon, 14 atoms; hydrogen, 10; oxygen, 1. Oil of turpentine, purified with alcohol by Dr. Nimmo's method, seems to approach to the constitution of naphtha, or of a mere carburet of hydrogen. Its specific gravity is 0·878. But as from the mode of preparing it, a minute portion of alcohol may remain in it, I do not think it necessary to investigate its atomical structure."

"Naphtha, specific gravity 0·857, obtained by distillation from petroleum, is very nearly represented by carbon, 22 atoms; hydrogen, 20; oxygen, 1. It, therefore, consists of 20 atoms olefiant gas, 1 atom carbonic oxide, and 1 atom of carbon held in solution."

"Castor oil is an interesting unctuous body, from its great solubility in alcohol. It consists nearly of carbon, 7 atoms; hydrogen, 6; oxygen, 1. It is composed, therefore, of 6 atoms olefiant gas + 1 atom carbonic oxide."

"Alcohol, specific gravity 0·812, is composed very nearly of carbon, 3 atoms; hydrogen, 5; oxygen, 2; or, of 3 atoms olefiant gas = 2·625, 2 water = 2·25. And in volumes, 3 olefiant gas =  $\cdot 9722 \times 3 = 2\cdot 9166$ ; 4 aqueous vapour =  $\cdot 625 \times 4 = 2\cdot 500$ .

"Thus alcohol of 0·812, by the above analysis, which I believe merits confidence, from the care and consistency of the experiments, differs from M. Gay-Lussac's view of absolute alcohol, deduced from M. Th. de Saussure's experiments, in containing an additional volume of aqueous vapour. At the specific gravity ·814, alcohol would have exactly this atomic constitution. If the condensation be equal to the whole 3 volumes of olefiant gas; that is, if the 7 volumes of constituent gases become 4 of alcohol vapour, we shall have its specific gravity at this strength = 1·3722; the additional volume of aqueous vapour producing necessarily this abatement in the density."

"Fibres of the bleached threads of the silk-worm were subjected to analysis. Their composition is apparently, carbon, 10 atoms; hydrogen, 4; oxygen, 5; azote, 1; or, 4 of olefiant gas, 5 of carbonic oxide, and 1 of nitrous oxide; or of 1 atom prussic acid, 3 atoms olefiant gas, and 5 atoms carbonic oxide."

"Cotton fibres, unbleached, seem to consist of carbon, 11 atoms; hydrogen, 8; oxygen, 10. Flax, by Lee's patent process, consists of carbon, 7 atoms; hydrogen, 5; oxygen, 6. It contains more carbon, and is therefore probably stronger than common flax, prepared by a putrefactive maceration. This seems composed of carbon, 1 atom; hydrogen, 1; oxygen, 1. But this is the theoretical representation of sugar by M. Gay-Lussac and Dr. Prout; and hence, these chemists would readily explain, how linen rags may pass into the form of sugar by the action of sulphuric acid. Wool approximates to carbon, 10 atoms; hydrogen, 3; oxygen, 4; azote, 1."

"Cochineal seems to be made up of carbon, 15 atoms; hydrogen, 11; oxygen, 8; azote, 1. Cantharides approximate to carbon, 11 atoms; hydrogen, 10; oxygen, 7; azote, 1."

"My result with urea differs so considerably in the proportion of azote from that of Dr. Prout and M. Berard, that I am disposed to doubt of the accuracy of my experiments, though they were made with the utmost care, and were most consistent in the repetition. I could perceive no smell whatever of nitrous gas in the gaseous products, which were made to traverse a column of copper filings three inches long, in a state of ignition. I shall renew the inquiry on urea, and employ the lowest temperature compatible with the formation of carbonic acid."

"The prime equivalent of benzoic acid crystals, I find by saturation with water of ammonia, to be 14.5; and it consists apparently of carbon, 13 atoms; hydrogen, 6; oxygen, 4. Of crystalline citric acid, the prime equivalent is 8.375 by my experiments; and it consists probably of carbon, 4 atoms; hydrogen, 3; oxygen, 5; or, of 4 atoms carbon, 3 water, and 2 oxygen. Two of these atoms of water are separated, when citric acid is combined with oxide of lead in what is called the dry citrate. Hence, the acid atom is in this case 6.125. The prime equivalent of crystalline tartaric acid is 9.25 by my results; and it seems made up of carbon, 4 atoms; hydrogen, 2; oxygen, 6; or of carbon, 4 atoms; oxygen, 4; water, 2. From my experiments I have been led to conclude, that into dry tartrate of lead these two atoms of water *do* enter as a constituent; and hence, that the crystals of tartaric acid are as dry as is compatible with its constitution. Oxalic acid crystals have 7.875 for their prime equivalent, and are composed of carbon, 2 atoms; hydrogen, 3; oxygen, 6; or of 2 atoms carbon, 3 oxygen, 3 water. Into the dry oxalate of lead, these 3 atoms of water *do not* enter. Hence I find the dry acid to be composed of carbon, 2 atoms; oxygen, 3; or, of 1 atom carbonic acid + 1 atom carbonic oxide, as was first suggested, I believe, by Dobereiner. Crystallized oxalate of ammonia consists of 1 atom acid, 1 atom ammonia, and 2 atoms water, = 8.875. By a gentle heat, 1 atom of water may be separated; and an oxalate of ammonia, as dry as is compatible with its neutrality, remains."

"I have analyzed, by the peroxide of copper, the citrate, tartrate, and oxalate of lead; and on comparing the results thus obtained, with those derived from the analysis of the crystalline acids, I have come to the above determinations.

"Ferroprussic acid, the ferrocyanic acid of the French chemists, has proved hitherto a stumbling block to me, in reducing the results of my experiments to the atomic theory. I have subjected it to very numerous trials in many states of combination, and have sought, with great pains, to accommodate the results to the doctrine of prime equivalents; but hitherto without success. The following facts, however, may perhaps be deemed of consequence.

"In the first place, the prime equivalent of the crystallized ferroproussiate of potash is 13.125, compared to oxide of lead 14, and to nitrate of the same metal 20.75; that is, 13.125 of the former salt neutralize 20.75 of the latter. In the second place, 14 parts of oxide of lead yield 21 parts of dry ferroproussiate of lead; or the atomic weight of dry ferroproussic acid is 7.

"The mean of my analyses of ferroproussiate of lead gives the relation of the constituents of the acid, as marked in the table. These proportions, reduced to the atomic weight 7, afford

Carbon. ....	2.5774
Azote. ....	2.4703
Ferrous matter. ....	1.9523
	<hr/>
	7.0000

"Were we to suppose the prime equivalent of the ferroproussic acid 7.5 instead of 7; and were we further to suppose that the carbon in the above result should be  $2.25 = 3$  atoms, and the azote = 3.5, or 2 atoms, then we might conceive an atom of dry ferroproussic acid to be made up of

Carbon. ....	3 atoms	2.25
Azote. ....	2	3.50
Iron. ....	1	1.75
		<hr/>
		7.50

"But experiment does not permit me to adopt this theoretical representation.

"The best mode that has occurred to me for analyzing ferroproussiate of potash, is to convert it, by the equivalent quantity of nitrate of lead, into the ferroproussiate of this metal; then to separate the nitrate of potash by filtration; and, after evaporation, to determine its weight. In this way, 13.125 grains of crystallized ferroproussiate of potash afford 12.33 grains of nitre, which contain 5.8 of potash.\* By heating nitric acid in excess on 21 grains of ferroproussiate of lead, I obtained 2.625 grains of peroxide of iron, equivalent to 1.8375 of the metal. Hence I infer, that the iron in the ferroproussiate of lead is in the metallic state; for the joint weights of the carbon and azote contained in 7 grains of the dry acid is 5.0477; and the difference, 1.9523, approaches too closely to the above quantity, 1.8375, for us to suppose the metal to be in the state of protoxide. In fact,  $2.625 \text{ parts of peroxide} \times 0.9 = 2.3625$  of protoxide, is a quantity much beyond what experiment shows to be present."

\* "By careful desiccation, 1.69 grain of water may be separated from 13.125 grains of the salt."

## ARTICLE VII.

*Memoir illustrative of a general Geological Map of the principal Mountain Chains of Europe.* By the Rev. W. D. Conybeare, FRS. &c.

(Continued from p. 289.)

*Sands immediately inferior to the Chalk (Green and Iron Sand).  
Craie Chloritee; in some Instances, Quadersandstein?*

This formation, where most extensively displayed in England (the only country where it has yet received full attention), consists of the following deposits, each of considerable thickness.

1, and lowest. Beds of sand highly charged by brown oxide of iron containing lignites, associated occasionally with coarse limestone containing remains of turtles, crocodiles, and marine shells.

2. Blue marl.

3. Beds of sand usually coloured by green oxide of iron, but occasionally passing into the brown oxide, containing alcyonia and sponges, and abundant shells often chalcedonized.

4. Marl containing several peculiar multilocular shells (hamites turrilites, &c.), and bones of Saurian animals, passing by its lowest beds into No. 3, on which it rests, and by its upper beds into the inferior members of the chalk formation which cover it.

All these varieties, however, are by no means universally found even in England, and nothing beyond a general conformity to the type of the series taken collectively can be reasonably expected in distant countries.

### *Localities.—(A.) England.*

In England, this series is very imperfectly exhibited in the northern counties, where traces only of it are to be seen under the chalk wolds of York and Lincolnshire, and crossing the æstuary of the Wash at Hunstanton cliff in Norfolk. In Cambridge, Bedford, and Buckinghamshire, the iron sand, No. 1, is well exhibited; but the upper members are more confused. It does not indeed appear that the green sand exists as a distinct deposit, but is rather blended with the marl, No. 4 (there called *galt*). In Oxfordshire and Berkshire the tract which lies along the course of these formations is low, and much concealed by diluvial debris; but insulated portions of the lowest deposit (the iron sand) sometimes form summits on the ranges of the oolitic hills. In Wiltshire, the green sand, No. 3, is shown in great force, as also on the confines of Dorset and Devon, where it forms the summit of the elevated platform of Blackdown Hills. Insulated ridges of similar character extend on the west of Exeter as at Haldon Hill, almost close to the transition district;

but the best opportunities of studying the whole formation are afforded by the eastern coast of Dorsetshire (the Isle of Purbeck), by the Isle of Wight, and more especially by the great denudation of the beds beneath the chalk in the south-eastern counties, including the Weald of Kent, Surrey, and Sussex, where all the beds above enumerated are displayed on the fullest scale. In the other British Islands, this formation only occurs in the north-east of Ireland, where it may be seen near Belfast supporting the chalk which underlies the great basaltic area of that district.

(B.) *France.*

In France these formations have been observed among those which circle round both the northern basin of the Seine and the south-western basin of the Garonne.

1. They range beneath the escarpment of the chalk hills surrounding the denudation of Boulogne, a continuation of that just mentioned as occupying the south-eastern counties of England.

2. They skirt round the exterior of the chalky zone bounding the basin of Paris, forming a broad sandy tract. Mr. de la Beche has distinctly described the commencement of these chains from the channel near the mouth of the Seine, and illustrated it in his excellent sections published in the *Geological Transactions* for 1822.

M. Omalius d'Halloy has described the series under the title of the lower chalk, which seems very unfortunately chosen, since it is only mineralogically applicable to a very small part of it (that corresponding to our chalk marl), and has led to much confusion both as to the description of the chalk formation itself, and its constituent fossils. This author notices the following subdivisions: 1. Chalk; sometimes of a coarser texture, occasionally mixed with clay, sand, and chlorite, containing pale flints abundantly. 2. Tuffeau; coarse sandy chalk mixed with chlorite. 3. Sands and sandstones; often mixed with calcareous matter. 4. Greyish clay; commonly of a marly character, sometimes mixed with chlorite: the passages of these modifications into one another, and their alternations prevent the decided determination of their order of superposition, further than the assigning the highest position in the series to No. 1.

The green sand occurs beneath the chalk at Valenciennes; it is there called *Turtia*.

3. In the basin of the Garonne this formation has been particularly observed by M. Boué along its northern border, stretching from the Island of Aix near la Rochelle, to Perigord.

Humboldt has extracted from an unedited memoir of M. Fleury de Bellevue, some very interesting particulars concerning a large deposit of lignites connected with this formation in the vicinity of the Isle of Aix. They consist of dicotyledonous plants, partly petrified, partly bituminized, and sometimes in the state of jet: they are generally compressed, and lie sometimes

is regular horizontal beds, sometimes confusedly heaped together. They form a kind of submarine forest, extending in a band a league and a half in breadth from the north-west of the Isle of Oleron, 14 leagues to the interior of the mainland along the right bank of the Charente, about a metre below the level of the high tides. They are associated with a coarse grit containing bones of large marine animals. The geological constitution of the neighbourhood is said to be following an ascending order: 1. Compact lithographic limestone of an even fracture (La Rochelle St. Jean d'Angely), beds of oolite (point of Chatelaillon and Matha). 3. Lumachelle and beds of polypiers, with impressions of *gryphœa angustata*. (These three deposits are considered as representing the Jura limestones, and the latter as identical with the coral rag of England). 4. Great bed of lignite with marine peat, succin asphalté, and plastic clay. 5. Ferruginous and chloritose sand, slate clay, and argillaceous and calcareous beds with *trigoniæ* and *cerithia*, and fragments of lignite. On the south-west of the Charente, No. 4 and 5 are wanting, and a very white limestone said to be the lowest chalk rests immediately on the oolites. M. Boué has traced the prolongation of these lignites from Rochefort by Perigueux to Saltat in the Perigord.

Similar instances of the occurrence of lignite in this formation have been observed in England in the Isle of Purbeck, the Isle of Wight, and in the Weald of Sussex. Mr. Mantell has described the vegetable remains of the last-mentioned deposits in the forty-second page of his work on the geology of that district.

The iron ores of Perigord occur in this formation. M. Boué has also observed iron sand, green sand (with crabs and echinities), and chalk marle, on the SW. of the basin of the Garonne between Bayonne, St. Severs, and Dax. These deposits repose on the Jura limestone, which forms a band at the foot of the Pyrenees. Quadersandstein with lignites succeeds—then a few beds of Muschelkalk covering the great saliferous sandstone.

#### (C.) *Alps.*

On the northern borders of the Alps, the highest beds of the exterior calcareous chains consist of a dark-coloured limestone often mixed with sand and green particles, and agreeing in its fossils with this part of the English series, with the addition of nummulites, which are rare (although they do occasionally occur) in these beds in England. Similar beds are mentioned, and in a similar position, on the skirts of the Maritime Alps, near Nice, in Mr. Allan's account of that neighbourhood. (Ed. Phil. Trans.) They form the second limestone of the memoir referred to.

In Cuvier and Brogniart's *Description Géologique des Environs de Paris*, will be found an account of a formation of the same epoch with the craie chloritée, (the English green sand, and chalk marl,) in the chain of Buet, with a particular enumeration

of its fossils ; and a description of a similar formation overlying the beds of Jura limestone at the loss of the Rhine, near Bellegarde.

(D.) *Germany.*

The prolongation of the Jura chain through Franconia is covered from near Ratisbon with formations, probably coeval with those now described.

I have already hinted my suspicions that more than one formation are confounded under the name of quadersandstein : that rock so called, which is obviously interposed between the Muschelkalk and Jura limestone, is probably the sand and sandstone of our inferior oolite ; I still however conceive that much of the quadersandstein in the north of Germany will not be found placed in that relation, but rather corresponds with our green and iron sands ; yet it is highly difficult to pronounce concerning a rock which is so seldom seen covered by any thing but diluvial detritus, and whose true place in the series must, therefore, rather be inferred from theory than ascertained by observation.

(E.) *Poland.*

Mr. Buckland considers the sand containing lignite which overlies the saliferous sandstones of the Wielickza mines, which M. Beudant hesitatingly refers to the tertiary molasse as belonging in truth to the green sand. Mr. Beudant's inference rests only on the occurrence of lignite, which he erroneously considered as peculiar to the tertiary sands, but which, as we have seen, is equally common in this formation. Mr. Buckland's opinion is rendered greatly more probable by the general structure of the country.

(To be continued.)

## ARTICLE VIII.

*A Means of Discrimination between the Sulphates of Barium and Strontium.* By James Smithson, Esq. FRS.

(To the Editor of the *Annals of Philosophy*.)

SIR,

April 2, 1823.

To distinguish barytes and strontian from one another, it is directed in No. 19 of the Journal of the Royal Institution to dissolve in an acid which forms a soluble salt with them, to decompose by sulphate of soda, and to add subcarbonate of potash to the filtered liquor. If the earth tried is strontian, a precipitate falls ; if barytes, not.

When these matters are in a state to be soluble in an acid, a more certain, I apprehend, and undoubtedly a much easier pro-

ceeding, is to put a particle into a drop of marine acid on a plate of glass, and to let this solution crystallize spontaneously. The crystals of chloride of barium in rectangular eight-sided plates are immediately distinguishable from the fibrous crystals of chloride of strontium.

I have not repeated the process above quoted; but if sulphate of strontium did possess the solubility in water there implied, this quality presented a ready method by which mineralogists would be enabled to distinguish it from sulphate of barium. On trial I did not find water, or solution of sulphate of soda, in which sulphate of strontian had long lain, produce the least cloud on the addition of what is called subcarbonate of soda.

The means I have long employed to distinguish the two sulphates apart was to fuse with carbonate of soda, wash, dissolve in marine acid, &c.; but this process requires more time and trouble than is always willingly bestowed, and may even present difficulties to a person not familiarized with manipulations on very small quantities.

A few months ago a method occurred to me divested of these objections. The mineral in fine powder is blended with chloride of barium, and the mixture fused. The mass is put into spirit of wine, whose flame is coloured red if the mineral was sulphate of strontium. The red colour of the flame is more apparent when the spirit is made to boil while burning, by holding the platina spoon containing it over the lamp.

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## ARTICLE IX.

*Observations on the Temperature, and general State of the Weather, on the Coast of Africa from the River Sierra Leone (8° 30' N) to the Equator, but principally in the Gulph of Guinea from Lat. 5° N. to the latter.* By Capt. B. Marwood Kelly, Royal Navy, late of his Majesty's Ship Pheasant.

(To the Editor of the *Annals of Philosophy*.)

SIR,

April 4, 1823.

FROM local peculiarities in the state of the weather on these parts of the coast, it is usual to divide the year into seasons of a denomination different from other parts of the globe; in place of spring, summer, autumn, and winter, they are here called the tornado,\* rainy, foggy, second or after rains, and fine seasons.

\* These violent convulsions in the atmosphere so terrific to sailors, and which would be no less so to landmen, if the state of cultivation was so far advanced, as to expose the husbandman's labour to the ravages of these dreadful tempests, first shows itself on the eastern quarter of the horizon by a deep black cloud heavily charged with electric fluid. This cloud continues increasing in size, sometimes for an hour or two before it is put in motion, and constantly emitting vivid flashes of lightning, accompanied by heavy and

From Sierra Leone and the rivers in its immediate vicinity, as far as Cape Appollonia, the tornado season sets in about the middle of April, and continues to the middle of June, when it is succeeded by the rains; at this time these violent convulsions in the atmosphere are frequent; rarely two days pass without one being experienced; and even in the early parts of the month of April, they are sometimes felt. The load of vapour from which the atmosphere is unburdened by them, renders the air pure and wholesome, and the rain which falls in torrents for the space of an hour dries up so immediately that they may be deemed as contributing much to the salubrity of the climate; even to shipping, except at anchor in the rivers, if due precautions are taken, they are attended with no danger, as the gust of wind always comes from the land, north of Cape Palmas, and in a parallel with it, east of that Cape, and with quite sufficient warning even for a merchant ship, with but few men, to make the necessary preparations.

About the middle of June, the rainy season commences, and continues to the beginning, and sometimes even to the latter end of November: from the little cessation of rain which takes place during this period, the ground soon becomes drenched, and from it a miasma arises which engenders those pestilential remittent fevers so destructive of human life in this part of the world.

As the rains begin to subside, they are succeeded by thick hazy weather, arising from a rapid evaporation of the moisture still remaining in the ground.

About the latter end of December, and during the whole of January, a wind occasionally blows, possessing properties, and attended by circumstances, peculiar to itself; it is known by the name of the Harmatan, and blows from the eastward with considerable strength. It is always attended by thick hazy weather, notwithstanding which it is so dry and parching, that all wood-work warps and shrinks, and, if united by glue, becomes detached. Paper and books appear as if they had been placed close by a fire. On the human frame its effects are considerably felt; the lips and nostrils become sore and inflamed, and the

distant thunder. After a time, it rises a little above the horizon, to which its lower edge is parallel, and extremely black, and there remains stationary for a short time; when it is again put in motion, the most frightful flashes of forked lightning, accompanied by the heaviest possible claps of thunder, now issue from it in rapid succession; when it has reached a little beyond the zenith, a sudden chill is felt in the temperature, and then follows a more violent squall, or gust of wind, and rain, than the most fertile imagination can picture; but which seldom lasts longer than half an hour. I always made a practice of taking in every sail, and putting the ship before the wind: and I am of opinion that in some of the most violent, even without sail set, if that precaution was not taken, *any* ship would be thrown on her beam ends. On shore, all animated nature seems extinct; nothing is seen, nothing is heard; every creature, whether man, bird, or beast, having sought refuge and shelter from the approaching storm; but no sooner is it over than the air, which was before close and sultry, becomes so delightfully pure and invigorating as to reanimate the whole animal creation.

throat parched, and other exceedingly uncomfortable sensations excited, although it is generally said to give wounds and ulcers a strong predisposition to heal. The maximum of the thermometer is seldom above 75° Fahr. 10° lower than it is both before and after it. Its duration varies from two or three to seventeen or eighteen days; this may be considered as part of the fine season, which continues till the tornados again commence. It is not peculiar to this part alone, but prevails throughout the whole extent of tropical Africa.

The Gold Coast, which is said to commence at Cape Appollonia, but more properly at Cape Three Points, and ends at Cape Saint Paul, comes next in the line of coast; and as it differs both in height and appearance from that further to windward, so does it both in climate and salubrity.

The tornado season commences early in March, and ends about the middle of May; they are by no means either so violent or frequent as on the coast east or west of it. Towards their close, and immediately preceding the rains, strong southerly squalls with heavy rain are sometimes experienced, but unaccompanied by thunder and lightning.

About the middle of May, the first rains commence, and continue for six weeks. Europeans who have spent some years in the country, suffer much inconvenience from intermittent fever, but it is seldom attended with danger.

Early in the month of July, the first rains cease (it is here that cessation commences), and is followed by a dense fog which continues till August. During this period those persons who are not inured to the climate are subject to attacks of the bilious remittent fever, which often proves fatal; the season when this pestilential disease prevails is comparatively short, arising I imagine from the surface of the land being composed of a light sandy soil, which easily admits the water that falls to run off into the sea, or to be taken up by evaporation; it is a singular fact that there are no springs on the Gold Coast, and the inhabitants are entirely dependent on reservoirs, in which water is caught and preserved during the rains for the whole year's consumption.

From the beginning of August to the middle of September, the weather is particularly fine and pleasant, the mean temperature afloat not being more than 78° Fahrenheit.

To this succeeds the second rains, which last till the end of October, but these are so moderate as not to be more than occasional showers. The weather from this time is fine till the tornado season again commences. In December and January the harmatan occasionally blows as on the windward coast, and with the same effects.

The Bight of Benin,\* from Cape Saint Paul to the river Ramos, is (with the exception of the tornados being much more

\* The Bight of Benin is formed by Cape Saint Paul and Formosa, which are distant from each other about 103 leagues.

tempestuous), subject to the same periodical division of seasons as the Gold Coast till the middle of September, when the second rains set in with the greatest possible violence, frequently attended with the heaviest tornados. I also found that on the eastern side of it, from Lagos quite round over that alluvial land through which the great rivers flow, a slight tornado came off about sunset every evening during the month of November.

In the Bight of Biafra, the tornado season commences in the beginning of February, and lasts to the middle of March, when it is succeeded by the first rains. These continue to the middle of May, and are then followed to the end of that month by the fogs, but they are not nearly so dense as in the Bight of Benin, especially in the neighbourhood of the islands. From this time to the middle of September the weather is particularly fine, but seldom unaccompanied with haze. At the latter period, the second rains set in, and fall extremely heavily to the end of October, when they begin to subside, and are followed by fine weather till the tornados recommence in February.

The above description of the weather within the before-mentioned limits on the Coast of Africa, although it appears to be divided, and governed by laws with the most perfect regularity, is by no means to be considered as not subject to any variation; as, for example, in July, 1819, on the Gold Coast and Bight of Benin, although generally subject to fogs, I experienced a considerable quantity of rain till I reached the river Ramos, from thence around Cape Formosa, and into the Bight of Biafra, the weather was particularly fine although attended with some haze.

Again in the year 1820, I was cruising in the Bight of Biafra during the whole of the months of June and July, and nothing could exceed the delightful state of the weather; but in the end of July, 1821, I rounded the Bight of Benin in a thick fog, and on approaching Cape Formosa, and running along the north coast of Biafra, I found heavy rains constantly falling from midnight to noon, which continued for nearly a month.

Also, although a portion of the year is called the tornado season, tornados are not uncommon during the periodical rains, insomuch that in the neighbourhood of Sierra Leone, the end of September is frequently called the second tornado season.

The following daily statement of the temperature is the mean of three observations of the thermometer taken generally at 8 a. m. 1 p. m. and 8 p. m.

The thermometer was hung against the middle partition of the Pheasant's cabin, with the doors, windows, and ports, always open to admit a free circulation of air.

1819.		1820.		1821.	
June 1	80½°	June 1	79°	June 1	81°
2	82½	2	80½	2	80½
3	83½	3	81	3	80½
4	83½	4	81½	4	81
5	82	5	81	5	80
6	82	6	80½	6	81
7	81½	7	80½	7	79½
8	84½	8	79	8	79½
9	85	9	78½	9	79½
10	84½	10	79½	10	80
11	84½	11	79½	11	79½
12	83½	12	78½	12	79½
13	83	13	78½	13	78½
14	84½	14	77½	14	78½
15	83½	15	77½	15	78½
16	84½	16	78	16	79
17	81½	17	78½	17	79
18	83½	18	78	18	78
19	81½	19	77½	19	79
20	82½	20	76½	20	78½
21	82½	21	78½	21	78
22	79½	22	76½	22	78
23	81	23	77	23	77½
24	80½	24	77	24	78
25	79½	25	76½	25	77½
26	79½	26	76½	26	77½
27	78½	27	77½	27	76
28	78½	28	77½	28	75
29	78½	29	77½	29	77½
30	80	30	78½	30	78
Mean of the month ..	82	Mean of the month ..	78	Mean of the month..	78½
Maximum therm. on the 9th at 1 p.m....	87	Maximum therm. on the 4th at 1 p.m....	82	Maximum therm. on the 6th at 1 p.m....	85
Minimum therm. on the 20th at 8 a.m..	78	Minimum therm. on the 21st at 8 a.m..	75	Minimum therm. on the 28th at 8 a.m..	74

*June, 1819.*—From the 1st to 5th, between lat. 11° and 8° 30' N, fine weather; the 6th and 7th off Sierra Leone, tornados and heavy rain; from the 7th to 17th at anchor in the river. Weather occasionally fine; at others showery, and tornados. From the 17th to 30th, on the coast between Sierra Leone and Cape Palmas, continued heavy rain, with heavy tornados occasionally.

*June, 1820.*—From the 1st to 16th, running from Cape Coast to the Island of Saint Thomas, north of the parallel of 3° N, a considerable quantity of rain fell; but south of it, had fine weather; from the 17th to 30th cruising between the parallel of 2° N and the equator. Fine weather, but cloudy; and I must here observe, after three years' experience, that I have always found the weather more cloudy, and the thermometer lower, on or near the equator, than a few degrees north or south of it.

*June, 1821.*—From the 1st to 23d, at anchor at the Island of Ascension particularly fine weather. From the 24th to 30th, on the passage from Ascension to Cape Coast, fresh breezes, and cloudy throughout.

1819.		1820.		1821.	
July 1	80½°	July 1	77½°	July 1	80°
2	79½	2	77½	2	80
3	78½	3	77½	3	79½
4	79½	4	79	4	79½
5	78½	5	77½	5	79½
6	78	6	76½	6	78½
7	78½	7	76½	7	77½
8	78½	8	76½	8	77
9	79½	9	75½	9	78½
10	80	10	77	10	78½
11	79	11	77½	11	78½
12	78½	12	77½	12	78½
13	79	13	77½	13	78½
14	79½	14	77½	14	77½
15	79	15	76½	15	78
16	80	16	78	16	77
17	80	17	77	17	76½
18	81	18	77	18	76½
19	80½	19	77½	19	78½
20	81½	20	77½	20	78
21	77½	21	77	21	80
22	78½	22	77½	22	80
23	78	23	77½	23	79½
24	78½	24	77½	24	79½
25	78½	25	77½	25	80
26	78	26	77½	26	79½
27	77½	27	78½	27	79
28	77½	28	76	28	78½
29	78½	29	75½	29	78½
30	78½	30	76½	30	78½
31	78½	31	77	31	78½
Mean of the month...	79	Mean of the month...	77½	Mean of the month...	78½
Maximum therm. on the 1st at 1 p. m. ...	82	Maximum therm. on the 4th at 1 p. m. ...	82	Maximum therm. on the 12th at 1 p. m. ...	82
Minimum therm. on the 31st, 23d, 26th, 27th, 28th at 8 a. m. and p. m. ....	77	Minimum therm. on the 7th, 8th, 9th, 15th, 29th, at 8 a. m. ....	75	Minimum therm. on the 17th at 8 p. m. ...	75

*July, 1819.*—From the 1st to 9th, from the vicinity of Cape Palmas to Cape Coast Roads, continued heavy rain. From the 10th to 16th at anchor in Cape Coast Roads, cloudy weather, with frequent showers. From the 17th to 20th on various other parts of the Gold Coast, fine weather. From the 21st to 25th, in the Bight of Benin, continued heavy rain. From the 26th to 31st, in the Bight of Biafra, fine weather.

*July, 1820.*—The whole of this month between 1° 50' N, and 0° 20' S, moderate and cloudy weather throughout.

*July, 1821.*—From the 1st to 18th on the Gold Coast, moderate breezes and hazy, with showers at times. From the 18th to 23d, rounding the Bight of Benin, in thick hazy weather. From the 24th to 31st, on the coast between Cape Formosa and the river Bonni, cloudy and unsettled weather, with rain, especially from midnight to noon.

1819.		1820.		1821.	
August 1	77 $\frac{3}{4}$ °	August 1	77°	August 1	79 $\frac{1}{2}$
2	77 $\frac{1}{2}$	3	75 $\frac{1}{2}$	2	76
3	77 $\frac{1}{2}$	2	76 $\frac{1}{2}$	3	75
4	78	4	79	4	77 $\frac{1}{2}$
5	76 $\frac{3}{4}$	5	78 $\frac{3}{4}$	5	77
6	78 $\frac{1}{2}$	6	78	6	76
7	77 $\frac{1}{2}$	7	77 $\frac{1}{2}$	7	76
8	77 $\frac{1}{2}$	8	79 $\frac{1}{2}$	8	76 $\frac{1}{2}$
9	77 $\frac{1}{2}$	9	74 $\frac{1}{2}$	9	77 $\frac{1}{2}$
10	77 $\frac{1}{2}$	10	74 $\frac{1}{2}$	10	78 $\frac{1}{2}$
11	76 $\frac{3}{4}$	11	73	11	80
12	75	12	72 $\frac{1}{2}$	12	79 $\frac{1}{2}$
13	76 $\frac{3}{4}$	13	72 $\frac{1}{2}$	13	78 $\frac{1}{2}$
14	78	14	73 $\frac{1}{2}$	14	79
15	78 $\frac{1}{2}$	15	73 $\frac{1}{2}$	15	78 $\frac{1}{2}$
16	79	16	74 $\frac{1}{2}$	16	79
17	77 $\frac{1}{2}$	17	75	17	79 $\frac{1}{2}$
18	77 $\frac{1}{2}$	18	74 $\frac{1}{2}$	18	78 $\frac{1}{2}$
19	77	19	74 $\frac{1}{2}$	19	77
20	77 $\frac{1}{2}$	20	72 $\frac{1}{2}$	20	77
21	78 $\frac{1}{2}$	21	73	21	78 $\frac{1}{2}$
22	78 $\frac{3}{4}$	22	73	22	78
23	78	23	72	23	79
24	77	24	73	24	77 $\frac{1}{2}$
25	78	25	73 $\frac{1}{2}$	25	77 $\frac{1}{2}$
26	78 $\frac{1}{2}$	26	73	26	77 $\frac{1}{2}$
27	78 $\frac{1}{2}$	27	73 $\frac{1}{2}$	27	77 $\frac{1}{2}$
28	78 $\frac{1}{2}$	28	73	28	77
29	78 $\frac{1}{2}$	29	75	29	78 $\frac{1}{2}$
30	78 $\frac{1}{2}$	30	76 $\frac{1}{2}$	30	77 $\frac{1}{2}$
31	79 $\frac{1}{2}$	31	76	31	77
Mean of the month...	78	Mean of the month...	74 $\frac{1}{2}$	Mean of the month...	77 $\frac{1}{2}$
Maximum therm. on the 31st at 1 p. m.	80	Maximum therm. on the 8th at 1 p. m.	80	Maximum therm. on the 1st at 1 p. m.	82
Minimum therm. on the 12th all day...	75	Minimum therm. on the 13th at 8 a. m.	71	Minimum therm. on the 3d at 8 a. m.	74

*August, 1819.*—From the 1st to 17th, running from the Bight of Biafra to Sierra Leone; while in the neighbourhood of the equator, cloudy, but settled weather, with fresh southerly winds. After passing the lat. of 4° N (about the parallel of Cape Palmas), continued and heavy rain. From the 18th to 25th, at anchor in Sierra Leone River, heavy rain. From the 26th to 31st, between Sierra Leone and Cape Mount, cloudy weather, with rain occasionally.

*August, 1820.*—The whole of this month on the Gold Coast, the weather generally fine.

*August, 1821.*—From the 1st to 6th, at anchor in George's Bay, in the Island of Fernando Po, unsettled weather with rain. 7th, 8th, 9th, and 10th, in the vicinity of the above island, with a continuation of the same weather. From the 11th to 31st, between the parallels of 1° and 3° N, and 8° and 4° E, variable and unsettled weather, frequently attended with rain.

1819.		1820.		1821.	
September	1	79°	September	1	75½°
	2	79		2	73½
	3	79½		3	76½
	4	79		4	76½
	5	78½		5	77½
	6	78½		6	77½
	7	78½		7	78½
	8	78½		8	78
	9	76½		9	78½
	10	75½		10	78
	11	77½		11	77½
	12	79		12	78
	13	76½		13	78
	14	76½		14	77½
	15	76½		15	78
	16	76		16	78½
	17	75½		17	79
	18	76½		18	78½
	19	74½		19	78½
	20	77		20	79
	21	78		21	76
	22	77		22	77½
	23	77½		23	77½
	24	78½		24	79
	25	78		25	79½
	26	79½		26	80
	27	80		27	81
	28	78½		28	78½
	29	79½		29	78½
	30	79½		30	79½
Mean of the month ..	77½	Mean of the month ..	78	Mean of the month ..	78½
Maximum therm. on		Maximum therm. on		Maximum therm. on	
the 27th at 1 p. m. .	81	the 27th at 1 p. m. .	80	the 26th at 1 p. m. .	83
Minimum therm. on		Minimum therm. on		Minimum therm. on	
the 19th at 8 p. m. .	74	the 2d at 8 p. m. . .	72	the 4th at 8 a. m. .	74

*September, 1819.*—From the 1st to 10th, running from the windward coast to Cape Coast Roads, moderate and cloudy weather, with showers of rain occasionally. From the 11th to 16th, at anchor in Cape Coast Roads, fine weather. On the 17th, 18th, 19th, and 20th, between the parallels of 5° and 6° N, cloudy weather. From the 21st to 30th in the Bight of Benin, continued and heavy rain.

*September, 1820.*—From the 1st to 12th, reaching from the Gold Coast to the Bight of Biafra, moderate and cloudy weather. From the 13th to 30th, between the parallels of 2° N and equator, first week cloudy, with rain occasionally; latter part heavy rain.

*September, 1821.*—From the 1st to 9th, running from 0° 30' N, and the meridian towards Sierra Leone, moderate and fine weather. From the 10th to 30th, at anchor in Sierra Leone River, weather extremely unsettled, with a considerable quantity of rain.

1819.		1820.		1821.	
October 1	79°	October 1	78½°	October 1	79½°
2	79	2	78½	2	79½
3	79	3	78½	3	82
4	78½	4	78	4	81½
5	79	5	77½	5	79
6	78½	6	78	6	80½
7	79½	7	78½	7	82
8	78½	8	78½	8	81
9	79½	9	78	9	80½
10	79	10	78½	10	78½
11	79½	11	78	11	80
12	79½	12	78½	12	81½
13	79	13	77½	13	79½
14	80½	14	78	14	79
15	81	15	78	15	81½
16	80½	16	77½	16	81
17	78½	17	77½	17	80
18	80	18	77	18	80½
19	79½	19	77½	19	81½
20	79½	20	77½	20	83½
21	81	21	77½	21	83½
22	81½	22	79	22	82½
23	80½	23	79½	23	82½
24	80½	24	77½	24	83
25	79½	25	80½	25	83½
26	80	26	80½	26	81½
27	80½	27	81½	27	83½
28	81	28	81	28	83½
29	81	29	79½	29	81½
30	81½	30	81½	30	83½
31	81½	31	78½	31	83½
Mean of the month ..	80	Mean of the month ..	78½	Mean of the month ..	81½
Maximum therm. on the 30th at 1 p. m.	82	Maximum therm. on the 30th at 1 p. m.	84	Maximum therm. on the 25th at 1 p. m.	86
Minimum therm. on the 5th at 8 a. m. . .	77	Min. therm. on 24th at 1 p. m. a tornado	76	Minimum therm. on the 10th at 8 a. m.	77

*October, 1819.*—From the 1st to 25th, cruising in front of the Right of Benin, nearly the whole time between the parallels of 4° and 5° N, continued and most heavy rain. The remainder of the month on the Gold Coast, fine weather.

*October, 1820.*—From the 1st to 13th, between the parallels of 1° 20' N and equator, and 9° and 3° 30' E, continued and heavy rain. From 14th to 23d, between the parallels of the equator and 4° N and 3° E and 13° W, cloudy weather. From the 24th to 31st, between the parallels of 5° and 8° 30' N, and 13° and 14° 30' W, continued and heavy rain.

*October, 1821.*—From the 1st to 16th, cruising between the parallels of 8° and 6° N, and 12° and 14° W, extremely unsettled and squally weather, attended by heavy and frequent rain. From the 17th to 31st, cruising between the parallels of 6° 30' and 11° 30' N, and 14° and 19° W, light winds, calms, and fine weather.

1819.		1820.		1821.	
November 1	82°	November 1	80½°	November 1	84°
2	82	2	80	2	84
3	82½	3	80½	3	84½
4	82½	4	80½	4	84
5	82½	5	80½	5	83½
6	82	6	80	6	83½
7	83	7	80½	7	83
8	83½	8	80½	8	82½
9	84	9	81	9	82½
10	82½	10	79½	10	80½
11	83	11	79½	11	80
12	82½	12	80½	12	80
13	82½	13	80½	13	78½
14	83	14	80½	14	78½
15	82½	15	79	15	79½
16	82½	16	82½	16	81½
17	80	17	82	17	81½
18	83½	18	80	18	81½
19	84	19	81½	19	81½
20	83½	20	81	20	82½
21	83	21	82½	21	83
22	82½	22	81	22	84
23	83	23	80½	23	83½
24	81½	24	81	24	84
25	80½	25	80½	25	84½
26	82½	26	81½	26	84½
27	83½	27	82	27	84
28	83½	28	81	28	83½
29	83½	29	81½	29	84½
30	81½	30	82½	30	84
Mean of the month ..	82½	Mean of the month ..	80½	Mean of the month...	82½
Maximum therm. on		Maximum therm. on		Maximum therm. on	
the 19th at 1 p. m.	85	the 16th at 1 p. m.	84	the 29th at 1 p. m.	87
Minimum therm. on		Minimum therm. on		Minimum therm. on	
the 17th at 8 a. m.	79	the 10th at 8 a. m.	77	the 14th at 8 a. m.	76

*November, 1819.*—The whole of this month, between the parallels of 6° and 3° N, and 1° and 6° E, generally fine weather, but a few tornados.

*November, 1820.*—The whole of this month, in the vicinity of Sierra Leone, during the first 16 days a considerable quantity of rain fell. The last fortnight the weather was cloudy, but otherwise fine.

*November, 1821.*—Throughout the whole of this month cruising between the Island of Goree and river Sierra Leone, the weather particularly fine, but frequently attended with considerable haze.

1819.		1820.		1821.	
December 1	81½°	December 1	81½°	December 1	82°
2	81½	2	82	2	81½
3	83½	3	81½	3	80½
4	82½	4	81½	4	80½
5	79½	5	80	5	80½
6	80	6	80	6	81½
7	78½	7	81½	7	81
8	80½	8	81	8	80
9	82½	9	79	9	80
10	79	10	80½	10	80
11	78½	11	81	11	80½
12	79½	12	80½	12	79½
13	78½	13	80½	13	81½
14	79	14	80½	14	80½
15	82	15	80½	15	80½
16	82½	16	81½	16	80
17	82½	17	81½	17	79½
18	83½	18	80½	18	79
19	83	19	81	19	78
20	81½	20	81	20	78
21	80	21	82	21	78
22	81½	22	81	22	77½
23	78	23	80	23	79½
24	80½	24	79½	24	79½
25	82	25	78½	25	79½
26	79½	26	80½	26	80½
27	82½	27	78½	27	80½
28	83	28	79½	28	80½
29	83½	29	79½	29	78½
30	82½	30	79½	30	79½
31	82½	31	80½	31	79
Mean of the month ..	81	Mean of the month ..	80½	Mean of the month ..	79½
Maximum therm. on the 9th at 1 p. m. .	86	Maximum therm. on the 4th at 1 p. m. .	84	Maximum therm. on the 7th at 1 p. m. .	85
Minimum therm. on the 10th at 8 a. m. .	77	Minimum therm. on the 25th at 8 a. m. .	75	Minimum therm. on the 19th at 8 a. m. .	75

*December, 1819.*—From the 1st to 14th, in and near Princes Island, fine weather. From the 15th to 31st, between the parallels of 1° 30' and 4° 57' N, running from Princes Island to Cape Coast Roads, moderate and cloudy weather; between the 19th and 24th, squally at times.

*December, 1820.*—From the 1st to 16th, between Sierra Leone and Cape Palmas, the weather very unsettled and squally with rain. From the 17th to 23d, between Cape Palmas and Cape Coast; and from the 24th to 31st, at anchor at the latter place, weather generally fine and pleasant.

*December, 1821.*—The whole of this month at anchor in Sierra Leone River. From the 1st to 13th, regular sea and land breezes, with fine weather. From the 14th to 23d, a strong harmattan blowing, with thick hazy weather. From the 24th to 31st, light and regular sea and land breezes, with fine weather, but rather hazy.

1820.		1821.		1822.	
January 1	82½°	January 1	80½°	January 1	77½°
2	83	2	80½	2	78½
3	82½	3	80½	3	81
4	82½	4	81½	4	80½
5	83	5	81½	5	80½
6	82½	6	81½	6	80½
7	81½	7	79½	7	81½
8	81½	8	82	8	81
9	81½	9	82½	9	81
10	80½	10	82½	10	81½
11	80	11	83	11	81
12	79½	12	82½	12	81½
13	80½	13	81½	13	81½
14	80½	14	80½	14	82
15	81½	15	81½	15	78
16	82	16	82	16	80½
17	82½	17	82½	17	80½
18	80½	18	82	18	80½
19	80½	19	82	19	81½
20	81½	20	81½	20	82
21	80½	21	81½	21	80½
22	81	22	83	22	79
23	78½	23	81½	23	80
24	80½	24	80	24	79
25	81½	25	81½	25	78½
26	82	26	81	26	79
27	83½	27	82	27	77½
28	82½	28	81	28	78½
29	8½	29	82½	29	78½
30	82½	30	82½	30	80
31	82½	31	81	31	80
Mean of the month...	81½	Mean of the month...	81½	Mean of the month...	80
Maximum therm. on the 27th at 1 p. m.	84	Maximum therm. on the 29th at 1 p. m.	86	Maximum therm. on the 20th at 1 p. m.	85
Minimum therm. on the 23d at 8 a. m.	77	Minimum therm. on the 31st at 8 a. m.	77	Minimum therm. on the 1st at 8 a. m.	75

*January, 1820.*—From the 1st to 18th, on the Gold Coast, moderate and settled weather. From the 19th to 31st, cruising between the parallels of 4° and 5° N, in front of the Bight of Benin; continued fine weather.

*January, 1821.*—From the 1st to 7th, on the Gold Coast, moderate and fine weather. From the 8th to 23d, between the parallels of 5° 30' and 4° N, and 1° 30' and 7° 30' E, light winds and fine weather attended by considerable haze. From the 24th to 31st, near the Island of Fernando Po, and at anchor in the Bay, light winds and fine weather; while at anchor in this Bay, found a greater variation of the temperature between the night and day than any part of the coast I had visited, the range being 9° or 10°.

*January, 1822.*—The whole of this month in Sierra Leone River; the weather in general moderate, but a slight tornado or two towards the middle of it.

1820.		1821.	
February	1 83°	February	1 82°
	2 83½		2 81½
	3 83½		3 81
	4 83½		4 82½
	5 83		5 82½
	6 83½		6 82½
	7 82½		7 82½
	8 84½		8 81½
	9 84½		9 82½
	10 84		10 83½
	11 82½		11 83
	12 84		12 82½
	13 82½		13 83½
	14 83		14 83½
	15 82½		15 82½
	16 83½		16 82½
	17 84		17 83
	18 84		18 83½
	19 83½		19 83½
	20 83½		20 84
	21 84½		21 84
	22 83		22 84
	23 83		23 84
	24 83		24 84
	25 81½		25 84
	26 83		26 83½
	27 83½		27 82
	28 84½		28 83½
	29 84		
Mean of the month ..	83½	Mean of the month ..	83
Maximum therm. on the 9th at 1 p. m. .	86	Maximum therm. on the 1st at 1 p. m. .	86
Minimum therm. on the 15th at 8 a. m. .	81	Minimum therm. on the 1st at 8 a. m. .	78

*February, 1820.*—Throughout this month, cruising between the parallels of 4° and 6° N, and 3° and 5° E, generally fine weather.

*February, 1821.*—Nearly the whole of this month between the parallels of 2° 30' N and the equator, and 8° 30' and 1° 30' E, light winds and cloudy, but fine weather throughout.

1820.			1821.		
March	1	83½°	March	1	83½°
	2	81½		2	83½
	3	81½		3	83½
	4	82½		4	82½
	5	82½		5	83
	6	82		6	83
	7	80½		7	82½
	8	82		8	81
	9	84		9	81½
	10	84		10	82
	11	81½		11	82½
	12	83		12	81½
	13	80½		13	82½
	14	82½		14	82½
	15	84		15	82½
	16	81		16	79½
	17	82½		17	81
	18	82½		18	82
	19	80½		19	82
	20	82		20	81½
	21	83½		21	83½
	22	83		22	83½
	23	83		23	82
	24	81		24	83½
	25	82½		25	83
	26	84½		26	82½
	27	83½		27	83½
	28	82½		28	83½
	29	84½		29	83½
	30	83½		30	82½
	31	81½		31	82½
Mean of the month...		82½	Mean of the month...		82½
Maximum therm. on the 28th at 1 p. m.		86	Maximum therm. on the 1st at 1 p. m.		83
Minimum therm. on the 31st at 5 p. m.		78	Min. therm. on 16th at 1 p. m. a tornado		79

*March, 1820.*—From the 1st to 7th, in and near Princess Island, in general, fine weather, but sometimes squally. From the 8th to 28th, between the parallels of 2° and 5° 30' N, and 7° 30' and 2° E; the remainder of the month on the Gold Coast, generally fine weather; between the first periods had a few tornados.

*March, 1821.*—From the 1st to 7th, between the parallels of 2° N and 1° 21' E, and the Gold Coast, moderate and fine weather. From the 8th to 21st, on the Gold Coast, fine weather. From the 22d to 31st, between the parallels of 3° 30' and 1° 30' N, and 1° and 7° E, moderate and pleasant weather.

1820.			1821.		
April	1	83°	April	1	82½°
	2	83½		2	83½
	3	84		3	83½
	4	85½		4	79½
	5	86½		5	82½
	6	85		6	82½
	7	84		7	80½
	8	84½		8	82½
	9	83½		9	82
	10	84½		10	83
	11	84		11	84
	12	82½		12	83½
	13	83½		13	81
	14	83½		14	82½
	15	83		15	80
	16	79½		16	82
	17	82½		17	80½
	18	81		18	82½
	19	81½		19	83½
	20	80½		20	83
	21	80½		21	83½
	22	80		22	82
	23	80		23	82½
	24	79½		24	83
	25	79½		25	82½
	26	80½		26	82
	27	81		27	83
	28	80½		28	83
	29	81		29	83½
	30	82		30	84½
Mean of the month .. 82½			Mean of the month .. 82½		
Maximum therm. on the 4th at 1 p. m. .. 86			Maximum therm. on the 30th at 1 p. m. .. 82		
Min. therm. on 16th at 1 p. m. a tornado. 77			Minimum therm. on the 4th at 8 p. m. .. 78		

*April, 1820.*—From the 1st to 24th, running from the Gold Coast to the Island of Ascension; between the coast and equator experienced frequent and heavy tornados; and between the parallel of 8° N and the latter had continued and heavy rain. Crossed the meridian and equator on the 16th nearly at the same time, after which had strong southerly and SSE winds, with cloudy weather, all the way to Ascension. The remainder of the month at anchor at Ascension fine weather.

*April, 1821.*—From the 1st to 11th, in the vicinity of Princes Island, experienced several tornados; otherwise had fine weather. From the 12th to 20th, running from Princes Island to the Gold Coast, experienced frequent and heavy tornados. From the 21st to 30th, on the Gold Coast, fine weather, with the exception of a tornado at times.

1820.		1821.	
May 1	82½°	May 1	83°
2	82½	2	84½
3	83	3	83
4	82½	4	81½
5	83½	5	80
6	82½	6	82½
7	81	7	82½
8	81½	8	82½
9	79½	9	80½
10	79½	10	82½
11	79½	11	82½
12	81½	12	82½
13	80½	13	81½
14	80½	14	81½
15	81½	15	81½
16	80½	16	81½
17	80½	17	80½
18	80	18	80½
19	79½	19	80½
20	80	20	80½
21	79	21	80½
22	78½	22	80½
23	77½	23	80½
24	79½	24	80½
25	80½	25	79½
26	80½	26	90
27	81½	27	90½
28	82	28	81½
29	83	29	81½
30	82½	30	80½
31	81	31	80½
Mean of the month ..	81	Mean of the month ..	81½
Maximum therm. on the 4th at 1 p. m. .	85	Maximum therm. on the 2d at 1 p. m. .	86
Min. therm. on 23d at 8 a. m. on equator. .	77	Minimum therm. on the 25th at 8 a. m. .	78

*May, 1820.*—From the 1st to 16th, at anchor at Ascension, fine clear weather. From the 17th to 31st, running from Ascension to Cape Coast Roads. While south of the equator, had strong breezes and cloudy weather. On approaching the coast, experienced unsettled weather, with rain.

*May, 1821.*—From the 1st to 7th, at anchor in Cape Coast Roads, cloudy weather, with rain occasionally. From the 8th to 14th, between the Gold Coast and equator, variable weather, sometimes fine, at others rainy. From the 15th to 26th, between the equator and the Island of Ascension. On the 27th, anchored at the latter; fine weather throughout.

## ARTICLE X.

*On the Height of the Barometer.* By M. P. Moyle, Esq.

(To the Editor of the *Annals of Philosophy*.)

DEAR SIR,

Helston, April 15, 1823.

A PROPER collection of accurate barometrical observations would give us a correct idea of the height of the different places where they are kept above the level of the sea, as well as serving other useful purposes; but unless every necessary correction is made, this cannot be obtained. From the few observations that I have made on those tables which have appeared in the *Annals*, this consideration does not seem to have been properly meditated. It may not, therefore, be unacceptable to such of your correspondents to make a few remarks on this particular point, and thereby prove its necessity.

It is well known that mercury expands by heat, and contracts on the application of cold. Hence the height of the mercurial column in the barometer is affected not only by the pressure of the atmosphere, but by the temperature, and the attention of meteorologists ought to be more fully drawn to this consideration to render their tables of the greatest utility.

The standard temperature for observation is agreed on all hands to be  $32^{\circ}$  of Fahr.; consequently if made at a time when the thermometer stands above or below this point, it must of necessity indicate a higher or lower range respectively; and to prove the extent of error arising from the neglect of this circumstance, let us suppose that the height of 30 inches of mercury is taken when the attached thermometer stands at  $72^{\circ}$ ; this would give an excess above what it would be at  $32^{\circ}$  of more than  $\frac{1}{100}$  of an inch from the mere expansion of the mercury. No wonder then that errors arise in our calculations.

General Roy found that the expansion of one inch of mercury in the barometer tube at  $32^{\circ}$  was  $\cdot 0001127$ : hence to reduce the observed height of the mercury to what it would be at  $32^{\circ}$ , becomes an easy matter; but for the greater facility, I have constructed the following table, which represents the expansion of one inch of mercury for its corresponding temperature. It extends from  $32^{\circ}$  to  $150^{\circ}$  beyond which it is not probable that any observation will be made.

In order to obtain the exact temperature of the mercury, the observation should be made by a thermometer attached to the frame of the barometer, that it may warm and cool along with it.

32°	·0001127	62°	·0032910	92°	·0064020	122°	·0093030
33	·0001127	63	·0033976	93	·0065026	123	·0094276
34	·0002250	64	·0035040	94	·0066030	124	·0095220
35	·0003370	65	·0036102	95	·0067032	125	·0096162
36	·0004492	66	·0037162	96	·0068032	126	·0097102
37	·0005610	67	·0038220	97	·0069030	127	·0098040
38	·0006726	68	·0039276	98	·0070026	128	·0098976
39	·0007840	69	·0040330	99	·0071020	129	·0099910
40	·0008952	70	·0041382	100	·0072012	130	·0100862
41	·0010062	71	·0042432	101	·0073002	131	·0101772
42	·0011170	72	·0043480	102	·0073990	132	·0102700
43	·0012276	73	·0044526	103	·0074976	133	·0103626
44	·0013380	74	·0045570	104	·0075960	134	·0104550
45	·0014482	75	·0046612	105	·0076942	135	·0105472
46	·0015592	76	·0047652	106	·0077922	136	·0106392
47	·0016680	77	·0048690	107	·0078900	137	·0107310
48	·0017776	78	·0049726	108	·0079876	138	·0108226
49	·0018870	79	·0050760	109	·0080850	139	·0109140
50	·0019962	80	·0051792	110	·0081822	140	·0110052
51	·0021052	81	·0052822	111	·0082772	141	·0110962
52	·0022140	82	·0053850	112	·0083760	142	·0111870
53	·0023226	83	·0054376	113	·0084726	143	·0112776
54	·0024310	84	·0055900	114	·0085690	144	·0113680
55	·0025392	85	·0056922	115	·0086652	145	·0114582
56	·0026472	86	·0057942	116	·0087612	146	·0115482
57	·0027550	87	·0058860	117	·0088570	147	·0116380
58	·0028626	88	·0059976	118	·0089526	148	·0117276
59	·0029700	89	·0060990	119	·0090480	149	·0118170
60	·0030772	90	·0062002	120	·0091432	150	·0119062
61	·0031842	91	·0063012	121	·0092380		

There are different ways to calculate the correction from this table; the first, and most accurate, is, that of multiplying the sum in the table corresponding with the observed temperature by 30, which gives the expansion for 30 inches of mercury. Then let the observed height of barometer be 28·420, attached thermometer 72°, and we shall have  $4348 \times 30 = 13044$ . Then  $30 \cdot 13044 : 30 :: 28 \cdot 420 = 28 \cdot 29697$ .

The second, the one I always adopt, as being more expeditious, is, by multiplying the sum corresponding with the temperature by the observed height of mercury, and then taking it from the observed height, viz.  $4348 \times 28 \cdot 42 = 1 \cdot 2357 - 28 \cdot 420 = 28 \cdot 29643$ , differing a mere nothing from the first method.\*

It appears from the *Ann. de Chim. et Phys.* that the meteorological table published monthly in them has the proper correc-

\* The correction obtained by this method must be *added* to or *subtracted* from the observed heights, according as the temperature is below or above the standard temperature, or 32° of Fahr.

tion made, and that it is marked at nine in the morning, at noon, at three in the afternoon, and at nine at night.

From the monthly mean of these heights, it appears that the barometer is highest at nine in the morning, next highest at nine in the evening, lower at noon, and lowest of all at three in the afternoon. The proper hours, therefore, for taking the heights of the barometer is nine in the morning, and at three in the afternoon.

The elevation of the barometer above the level of the sea ought also to be noted when known; and if a correction is made for it in the results given, it should also be remarked, as some meteorologists allow for it, while others are guilty of the omission.

I am, dear Sir, your humble servant,

M. P. MOYLE.

## ARTICLE XI.

*Some Particulars regarding the Ashmolean Catalogue of Extraneous Fossils, published in Latin by Mr. Edward Luid (or Llwyd); and recommending a Translation of the same to be made and printed. By Mr. John Farey.*

(To the Editor of the *Annals of Philosophy*.)

SIR,

Howland-street, March 29, 1823.

IN pursuance of a design which I formed some years ago, of collecting as many as possible of the published *localities* of fossil shells, and of other organic remains, in some instances, I lately made an alphabetical arrangement of the 294 *places*, to which Mr. Edward Luid (or Llwyd) seems to me to refer, in the second edition of his descriptive *Catalogue of the Ashmolean Museum at Oxford*, printed in 1760. This book I borrowed of my friends Messrs. Sowerby, having, after many years inquiry, never been able to meet with a copy of it on sale, from whence I conclude that it is out of print, and become very scarce and dear; which circumstances induce me to request you to give insertion in the *Annals* to a few particulars regarding Mr. Luid's work; with the hope that the same may excite the attention of the collectors of fossil shells, &c. and prove of some use to them, and may lead to the publishing of an English edition of this work, by some gentleman connected with the University; wherein I hope, that the copper plates to Mr. L.'s book are preserved, and might serve for a cheap English edition.

The specimens of organic remains, &c. mentioned, and briefly described, by Mr. Luid, including some sparry substances at the beginning, seem about 1800 in number; 1766 of which specimens are distinguished by a series of numbers, and some others.

are interpolated, and marked *a*, *b*, *c*, &c. in addition to the number.

The local index which I have made shows, that 30 places, which I shall mention presently, furnished more than one-half of the whole collection, or 1065 of the specimens enumerated; consisting of 477 fossil shells, and 588 other bodies, mostly organized; the produce, apparently, of twelve of the assemblages of strata, ascertained by Mr. William Smith, and enumerated in his Geological Table, from the *London clay* downwards to the *lias*.

It hence appears, that 25 of the places, among the most productive of Ashmolean specimens, furnished none to the Smithian collection, of about 1155 specimens of shells, &c. which Mr. S. in June, 1816, deposited in the *British Museum*,\* whose localities, he has partly mentioned in two quarto works, "*Strata Identified*," and a "*Stratigraphical System*" (but which works, unfortunately, remain unfinished); and which localities, as far as they have been published, are enumerated in alphabetical order, in the "*Philosophical Magazine*," vol. 50, p. 271.

For the purpose of ascertaining the strata of the following places, I have consulted Mr. Smith's separate "*Geological County Maps*," except as to Lincoln and Northampton counties, which are not yet published, and as to which, I have consulted his original "*Geological Map of England*," published in September, 1815; which last was accompanied by an interesting "*Memoir*," which has, I think, been strangely overlooked, by almost every subsequent writer.

1. From the *London clay*, on the N shore of Sheppy Island, near Minster, in Kent, 5 shells and, in all, 25 specimens, are described by Mr. Luid.

2. From the *Portland rock*, at Brill, 10 miles W of Aylesbury, Bucks, 11 shells and 2 other specimens:

3. From the *Coral rag* and *pisolite*, at 6 places; which, beginning south-westward in the range of these strata are as follows, viz. Faringdon, 13 miles W of Abingdon, Berks, 29 shells and 89 others; these 118 specimens being the greatest number from any one place; Garford (or Garvord) 4 miles W by S of Abingdon, Berks, 9 shells and 26 others; Marcham, 2½ miles W by S of Abingdon, Berks, 40 shells and 54 others; Bessel's-Leigh (or Basse's L.) 3 miles NW of Abingdon, Berks, 16 shells and 10 others; Chawley, 4½ miles N by W of Abingdon, Berks, 5 shells and 13 others; and Stanton (St. John's) 3 miles NE of Oxford, 5 shells and 11 other specimens.

4. From the *Woburn sand*; at 3 places, viz. Cumner, 4½ miles N by W of Abingdon, Berks, 6 shells and 10 others; Bullington, 1½ mile SE of Oxford, 28 shells and 26 others; and Stafford Grove, 2½ miles ENE of Oxford, 2 shells and 11 other specimens.

\* See vol. xi. p. 364 of the First Series of the *Annals*, also the *Phil. Mag.* vol. 4. p. 180. The places common to both collections are, Kelloways B., Marsham, Sheppy 11; Stonefield; and Towcester.

5. From the *Clunch clay*, at 2 places, viz. Oxford city, Sunk Fences or Walks, &c. 19 shells and 7 others; and Cowley,  $\frac{1}{2}$  mile SSE of Oxford, 41 shells and 27 other specimens.

6. From *Kelloway's stone*, at Kelloway's Bridge (or Calloway B.), 2 miles NE of Chippenham, Wilts, 13 shells.

7. From the *Cornbrash*, at 5 places, viz. Witney, 4 miles NNE of Bampton, Oxfordshire, 46 shells and 63 others; Kidlington,  $2\frac{3}{4}$  miles SE of Woodstock, Oxfordshire, 21 shells and 13 others; Islip, 4 miles SE of Woodstock, Oxfordshire, 13 shells and 13 others; Charlton, 3 miles S of Bicester, Oxfordshire, 5 shells and 24 others; and Raunds (or Rance),  $3\frac{1}{2}$  miles S of Thrapston, Northamptonshire, 10 shells and 10 other specimens.

8. From the *Forest Marble* at Stonesfield (or Stunsfield),  $2\frac{1}{2}$  miles W of Woodstock, Oxfordshire, 6 shells and 51 other specimens.

9. From the *Upper Oolite*, at 3 places, viz. Towcester (or Tocester), 8 miles SSW of Northampton, 9 shells and 6 others; Wellingborough,  $6\frac{1}{2}$  miles S of Kettering, Northampton, 7 shells and 6 others; and Desborough,  $4\frac{1}{2}$  miles NW of Kettering, Northamptonshire, 8 shells and 16 other specimens.

10. From the *Fuller's-earth*, at Marston Trussel (or Merston T.),  $11\frac{1}{2}$  miles WNW of Kettering, Northamptonshire, 13 shells and 11 other specimens.

11. From the *Under Oolite*, at 4 places, viz. Birlip Hill, 5 miles ESE of Gloucester, 9 shells and 4 others; Barrington (Great ?) 5 miles E of Northleach, Gloucestershire, 22 shells and 12 others; Upton,  $\frac{3}{4}$  mile W of Burford, Oxfordshire, 7 shells and 9 others; and Byfield,  $6\frac{1}{2}$  miles SW of Daventry, Northamptonshire, 21 shells and 3 other specimens.

12. From the *Liass*, at 2 places, viz. Parton Passage (or Pyrtan P. on the W shore of Severn River), 4 miles SSW of Newnham, Gloucestershire, 23 shells and 28 others; and Whitton (on S shore of Humber River), 10 miles W of Barton, Lincolnshire, 28 shells and 11 other specimens are described, and part of them figured, by Mr. Luid; 56 of which latter are shells.

Five out of the above 30 places, the least production of shells, &c. have furnished Mr. L. with 13 specimens each; all the other 264 places mentioned in his work, gave less numbers than 12 specimens each to the Ashmolean collection, except, perhaps, some of those unnamed places, included under the respective county names.

In a new edition of Luid, I beg to suggest that the several specimens figured, should be pointed out by a reference to the number of the plate or *table*. The want of these references, and the apparently random placing of the figures in the plates, are at present very perplexing; and, lastly, I request, that an index to the several *localities* may accompany such edition, towards the preparing of which, I would gladly lend assistance; and am,

Yours, &c.

JOHN FAREY.

## ARTICLE XII.

*A Description of the Crystalline Form of some new Minerals.*

By H. J. Brooke, Esq. FRS. FLS. &amp;c.

(To the Editor of the *Annals of Philosophy*.)

DEAR SIR,

April 19, 1822.

HAVING lately been engaged in an examination of the crystalline forms of minerals, and in a few instances of their chemical characters, preparatory to a list I have proposed to add to an elementary introduction to crystallography, I have observed a few new results, which form the substance of the following brief notices :

*Arfwedsonite*.—The benefits which mineralogy has derived from the labours of Mr. Arfwedson have induced me to associate his name with this mineral, which is from Greenland, and is black and foliated, and has been hitherto called ferriferous hornblende. It differs, however, from hornblende in its angles, its specific gravity, and its hardness.

I am not aware of its occurrence in determinable crystalline forms. It has cleavages parallel to the lateral planes, and to both the diagonals of a rhombic prism of  $123^{\circ} 55'$ , but there is no transverse cleavage to determine whether this prism is right or oblique.

Its colour is black without a shade of green. Its cleavage planes, and its cross fracture, have a greater lustre than those of amphibole, and it is scratched by amphibole.

Specific gravity 3.44.

It sometimes accompanies the sodalite from Greenland.

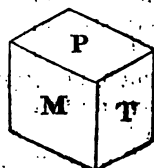
*Cleavelandite*.—The *albite*, and the *siliceous spar* of Haussman, which accompanies the green and red tourmaline from Chesterfield, in Massachusetts, are varieties of the same mineral. Two different names having been given to this substance, it becomes necessary either to adopt one of these to the exclusion of the other, or to assign a new one to the species.

As the *albite* is generally *blue*, and sometimes *red*,\* its name is consequently bad, and *siliceous spar* might be applied with equal propriety to other substances ; I have, therefore, preferred adopting the term *Cleavelandite* to denote the species, out of respect to the Professor of Natural Philosophy in Bowdoin College, United States.

This species has cleavages in three directions, parallel to the planes of a doubly oblique prism.

\* The specimen which first enabled me to determine the form is bright blue. It came from Labrador, and was given me by the Rev. C. I. Latrobe. M. Nordenskiöld has favoured me with a specimen which is red.

P on M.....	119° 30'
P on T.....	115 0
M on T.....	93 30



*Zoizite* is crystallographically, as well as chemically, a distinct species of mineral. It has been classed by the Abbé Haüy under epidote, a mistake into which he has probably fallen from observing crystals of epidote apparently forming part of the mass of the specimens of zoizite from Carinthia.

Some specimens of it have been sent to this country from the Tyrol under the name of spodumene, and resembling that mineral in colour, and it probably accompanies the true spodumene from that locality.

A mineral called zoizite from the Oural Mountains, which has been examined chemically by Mr. Children, and found to agree in its characters with zoizite, has afforded the most accurate measurements of the prism. And Mr. Heuland has kindly supplied me with other specimens of this mineral, by means of which I have been enabled to ascertain that the angles of the prism are the same in specimens from all the different localities in which the mineral occurs.

Its form is a rhombic prism, probably oblique from an obtuse edge, the lateral planes measuring  $116^{\circ} 30'$ , with a bright cleavage plane parallel to the short diagonal of the prism.

*Arseniferous Phosphate of Lead.*—This substance occurs at Iohangeorgenstadt in yellow hexagonal prisms, the terminal edges of which are replaced by single planes.

It presents crystalline faces after fusion by the blowpipe, as phosphate of lead does, and it exhales an abundance of arsenical fumes when fused on charcoal. It appears, therefore, to contain both arsenic and phosphoric acids, but I cannot find any analysis of it published.

I have taken this notice of it for the purpose of observing that the same species is found also at Beeralston in small yellow hexagonal prisms, and has been called arseniate of lead; from which, however, it may be readily distinguished by its crystalline character after fusion.

*Carbonate of Magnesia and Iron.*—On measuring the angles of different specimens of the substances which have been called bitter spar, or magnesian carbonate of lime, I have found one variety differing in its angle from all the others. This is the yellow variety from the Tyrol, which occurs in single crystals imbedded in talc or chlorite.

Its form is an obtuse rhomboid, measuring  $107^{\circ} 80'$ , the corresponding angle of the true bitter spar being  $106^{\circ} 15'$ .

On observing this difference in the angle, I dissolved a portion of the yellow crystals in dilute sulphuric acid, and obtained from

the solution crystals of sulphate of magnesia, tasting, however, very strongly of iron.

There was no residuum except a small quantity of the talc which penetrated the fragment I examined,\* and the solution gave no trace of lime; the mineral appears, therefore, to be a carbonate of magnesia and iron.

Ten grains kept for some time at a red heat lost 4·82, and the colour became a snuff-brown.

Ten grains dissolved in dilute muriatic acid left a small insoluble residuum, apparently of the talc in which the mineral is imbedded. A few drops of nitric acid being added to peroxidate the iron, a precipitate was obtained by succinate of ammonia, which, when washed, and heated to redness, to destroy the succinic acid, weighed 1 gr.

From this experiment, the mineral might consist of 1 atom of carbonate of iron, and 9 atoms of carbonate of magnesia.

For if to the apparent loss by heating ..... 4·820 we add the increase occasioned by the peroxidation of the iron, we shall have the true weight of the matter driven off.

1 gr. peroxide of iron, equivalent to ·815 protoxide,	
difference .....	0·185
	<hr/> 5·005
0·815 protox. of iron requires of carbonic acid for saturation	0·500
0·500 carbonic acid	
1·315 carb. iron	If this be carbonic acid ..... 4·505
8·605 carb. mag.	it would require of magnesia for its
	saturation ..... 4·100
9·920	<hr/> Giving carbonate of magnesia. .... 8·605

If we consider 7·25 as the equivalent for carbonate of iron, and 5·25 as that of carbonate of magnesia, we should have

$$7·25 : 5·25 :: 1·315 : 0·95 \text{ nearly}$$

$$\text{And } 0·95 : 8·605 :: 1 : 9 \text{ nearly.}$$

But a more accurate analysis might possibly vary these proportions.

*Latrobite.*—It is to the Rev. C. I. Latrobe that I am indebted for specimens of the mineral to which I have given this designation. And mineralogy is also indebted to him for the researches he has himself made, and caused to be made by others, in remote districts seldom visited by Europeans.

The mineral in question came from Amitok Island near the coast of Labrador; it is accompanied by mica and carbonate of

\* The true bitter spat leaves a pulpy residuum of sulphate of lime when dissolved in dilute sulphuric acid.

lime, and imbedded in a greyish-coloured substance which I suppose to be also new to mineralogy.

The colour of latrobite approaches to pink like some of the deep coloured varieties of lepidolite.

Its specific gravity is about 2.8.

It scratches glass, and is scratched by felspar.

It has cleavages in three directions, parallel to the lateral and terminal planes of a doubly oblique prism, P on M,  $98^{\circ} 30'$ ; P on T,  $91^{\circ}$ ; M on T,  $93^{\circ} 30'$ . (See the figure already given.) The plane parallel to P is very dull, and the measurement obtained from it not to be confidently relied on; those parallel to M and T afford good reflections, but one of these is brighter than the other.

I send herewith specimens of the arfwedsonite and latrobite, which you will, perhaps, take the trouble to analyze at your leisure; and I remain, yours truly,

H. J. BROOKE.

### ARTICLE XIII.

#### *On the Discovery of Acids in Mineral Substances.*

By James Smithson, Esq. FRS.

(To the Editor of the *Annals of Philosophy*.)

SIR,

April 12, 1823.

ACIDS, it is well known, have been repeatedly overlooked in mineral substances, and hence dubiousness still hovers over the constitution of many, although they have formed the subjects of analysis to some of the greatest modern chemists.

To be able to dissipate all doubts—to ascertain with certainty whether an acid does or does not exist, and, if one is present, its species, and this with such facility that the trial may be indefinitely renewed at pleasure, and made by all, so that none need believe but on the testimony of his own experiments, is the degree of analytical power which it would be desirable to possess.

So far as I have gone in these respects, I here impart.

As the carbonates of soda and of potash precipitate all the solutions of earths and metals in acids, so do they decompose all their salts by fusion with them. Fusion with carbonate of soda or potash affords there a general method of separating acids from all other matters.

Lead forms an insoluble compound with all the mineral acids except the nitric. It may consequently be immediately known whether a mineral does or does not contain an acid element by

the carbonate of soda or potash, with which it has been fused after saturation by acetous acid, forming or not forming a precipitate with a solution of lead.

If the production of a precipitate proves the presence of an acid, the determination of its species will present no great difficulty.

1. *Sulphuric Acid*.—If the alkali which has received it from the mineral is fused on charcoal, and then laid in a drop of water placed on silver, a spot of sulphuret of silver will be produced, as I have stated on a former occasion.\* Bright copper will likewise serve for this purpose.

Fusion in the blue flame will often be sufficient to deoxidate the sulphur.

It is needless to observe that the alkali used in this trial must itself be perfectly free from sulphuric acid. When such is not possessed, its place may be supplied by Rochellesalt, or by cream of tartar.

2. *Muriatic Acid*.—I have likewise discovered a test of chlorine, and consequently of muriatic acid, of delicacy equal to the foregoing. If any matter containing chlorine or muriatic acid is laid on silver in a drop of solution of yellow sulphate of iron, or of common sulphate of copper, a spot of a black chloride of silver, whose colour is independent of light, and which has not been attended to by chemists, is produced. The chlorine in a tear, in saliva, even in milk, may be thus made evident. When the quantity of chlorine in a liquor is very small, a bit of sulphate of copper placed in it on the silver is preferable to a solution. To find chlorine in milk, I put some sulphate of copper to it, and placed a small piece of bright silver in the mixture.

3. *Phosphoric Acid*.—The alkali containing it, after saturation by acetous acid, gives a sulphur-yellow precipitate with nitrate of silver, which no other acid does. The precipitate obtained with lead crystallizes on the blowpipe. M. Berzelius's elegant method of detecting phosphoric acid is universally known.

4. *Boracic Acid*.—Its presence in carbonate of magnesia, and in some other of its compounds, is indicated by the green colour they give, during their fusion, to the flame of the lamp.

M. Gay-Lussac has observed that a solution of boracic acid in an acid changes the colour of turmeric paper to red, like an alkali.† Borax, to which sulphuric acid has been put, does so, and the same is of course the case with a bead of soda containing boracic acid.

The most certain test of boracic acid in a soda bead, &c. is to add sulphuric acid to it and then spirit of wine, whose flame is coloured green, if boracic acid is present.

\* *Annals of Philosophy* for July, 1820.

† *Annales de Chimie et de Physique*, tome xvi. p. 75.

5. *Arsenical Acid*.—Alkali containing it produces a brick-red precipitate with nitrate of silver.\*

6. *Chromic Acid*.—Chromate of soda and its solution are yellow, and so is the precipitate with lead. That with silver is red.

Chromate of soda or potash fused on a plate of clay leaves green oxide of chromium.

Chromate of lead fused on a plate of clay produces a very dark-green mass, which is probably chromate of lead; with an addition of lead, it forms a fine red, or orange glass.

Lead added to the green oxide left by chromate of soda on the clay plate, dissolves it, and forms an orange-coloured glass.

The green oxide of chromium sometimes acts the part of an acid. I have seen a combination of it with oxide of lead found in Siberia, in regular hexagonal prisms, having the six edges of the terminal face truncated (Hailly, pl. lxviii. fig. 63); melted with lead on the clay plate this would undoubtedly produce the orange glass; and fused with nitrate of potash it would form chromate of potash.

7. *Molybdic Acid*.—If molybdate of soda or potash, or, I apprehend, any other molybdate, is heated in a drop of sulphuric acid, the mixture becomes of a most beautiful blue colour, either immediately, or on cooling.

The solution of molybdate of soda in sulphuric acid affords with martial prussiate of potash, a precipitate of the same colour that copper does. Tincture of galls gives with this acid solution a green precipitate; but with an alkaline solution of molybdic acid galls produce a fine orange precipitate. If an alkali is put to the green precipitate, it becomes orange; and if an acid to the orange precipitate, it becomes green.

8. *Tungstic Acid*.—If tungstate of soda is heated with sulphuric acid, the granules of precipitated tungstic acid become blue, but not the solution; and the phenomena cannot be confounded with those presented by molybdate of soda. Martial prussiate of potash has no effect on this acid liquor.

Tincture of galls put to the solution of tungstate of soda in water does not affect it. On the addition of an acid to this mixture, a brown precipitate forms.

If tungstate of soda is heated to dryness with a drop of muriatic acid, a yellow mass is left. On extracting the saline matter by water, yellow acid of tungsten remains. It is readily soluble in carbonate of soda. If taken wet on the blade of a knife, it soon becomes blue. This is made very evident by wiping the blade of the knife with a bit of white paper. Possibly a small remainder of muriatic or sulphuric acid among it is required for this effect.

9. *Nitric Acid*.—Nitrate of ammonia produces no deflagration when filtering paper, wetted with a solution of it and dried, is

burned; the salt volatilizing before ignition, most, or all, the other nitrates deflagrate.

If metallic copper is put into the solution of a nitrate, sulphuric acid added, and heat applied, the copper dissolves with effervescence.

10. *Carbonic Acid*.—It is to be discovered in the mineral itself. The application of heat is, in some cases, required to render the effervescence sensible. It has been sometimes overlooked in bodies from want of attention to this circumstance.

11. *Silica*.—A simple and sufficient test of it is the formation of a jelly, when its combination with soda is put into an acid.

It has evidently not been intended to enumerate all the means by which the presence of each acid in the soda bead could be perceived or established. Little has been said beyond what appeared required and sufficient.

Mention has been made above of small plates of clay.

They are formed by extending a white refractory clay by blows with the hammer, between the fold of a piece of paper, like gold between skins. The clay and paper, and then cut together with scissars into pieces about 4-10ths of an inch long, and 2-10ths of an inch wide, and hardened in the fire in a tobacco-pipe.

They are very useful additions to the blowpipe apparatus. They admit the use of a new test, oxide of lead. They show to great advantage the colours of matters melted with borax, &c. Quantities of matter too minute to be tried on the coal, or on the platina foil, or wire, may be examined on them alone, or with fluxes. Copper may be instantly found in gold or silver by fusing the slightest scrapings of them with a little lead, &c. &c.

Cut into very small, very acute triangles, clay affords a substitute for Saussure's sappare.

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## ARTICLE XIV.

### ANALYSES OF BOOKS.

*Narrative of a Journey to the Shores of the Polar Sea, in the Years 1819, 1820, 1821, and 1822.* By John Franklin, Capt. R.N. FRS. and Commander of the Expedition. *With an Appendix on various Subjects relating to Science and Natural History. Illustrated by numerous Plates and Maps. Published by Authority of the Right Honourable the Earl Bathurst.*

THE late period of the month at which this most interesting work has been submitted to our attention, in conjunction with

other circumstances, compels us to postpone, for the present, the regular analysis of it which we purpose to give; and to content ourselves with extracting from the copious Appendix, the following observations on the Aurora Borealis, to which subject Capt. Franklin's attention was expressly directed in his Instructions.

*Aurora Borealis.*

"*General Remarks.*—So few observations of the Aurora Borealis in high northern latitudes have been recorded, that I trust a minute account of the various appearances it exhibits, will not be thought superfluous or uninteresting. The remarks of the late Lieut. Hood are copied verbatim from his journal. They speak sufficiently for themselves, to render any eulogium of mine unnecessary. To this excellent and lamented young officer, the merit is due of having been, I believe, the first who ascertained by his observations at Basquiau-Hill (combined with those of Dr. Richardson at Cumberland House), that the altitude of the Aurora upon these occasions was far inferior to that which had been assigned to it by any former observer. He also, by a skilful adaptation of a vernier to the graduated circle of a Kater's compass, enabled himself to read off small deviations of the needle, and was the first who satisfactorily proved, by his observations at Cumberland House, the important fact of the action of the Aurora upon the compass-needle. By his ingenious electrometer invented at Fort Enterprise, he seems also to have proved the Aurora to be an electrical phenomenon, or at least that it induces a certain unusual state of electricity in the atmosphere."

"The observations of Dr. Richardson, independent of their merit in other respects, point peculiarly to the Aurora being formed at no great elevation, and that it is dependent upon certain other atmospheric phenomena, such as the formation of one or other of the various modifications of cirro-stratus."

"With respect to my own observations, they were principally directed to the effects of the Aurora upon the magnetic needle, and the connexion of the amount, &c. of this effect, with the position and appearance of the Aurora. I have been anxious to confine myself to a mere detail of facts, without venturing upon any theory. My notes upon the appearances of the Aurora coincide with those of Dr. Richardson, in proving, that that phenomenon is frequently seated within the region of the clouds, and that it is dependent, in some degree, upon the cloudy state of the atmosphere."

"The manner in which the needle was affected by the Aurora will need some description. The motion communicated to it was neither sudden nor vibratory. Sometimes it was simultaneous with the formation of arches, prolongation of beams, or certain other changes of form, or of activity of the Aurora; but

generally the effect of these phenomena upon the needle was not visible immediately, but in about half an hour or an hour, the needle had attained its maximum of deviation. From this, its return to its former position was very gradual, seldom regaining it before the following morning, and frequently not until the afternoon, unless it was expedited by another arch of the Aurora operating in a direction different from the former one."

"The bearings of the terminations of the arches are to be taken with considerable allowance. They were estimated by the position of the Aurora, with respect to the sides of the house, the angles of which had been previously determined. The bearings given in the whole of my observations refer to the magnetic meridian, and are reckoned from the magnetic north, towards the east round the whole circle, which, it is conceived, will afford a means of more readily computing the horizontal extent of the arches."

"It is to be noticed, that the bearings given by Dr. Richardson and Lieut. Hood are true, and not magnetic."

"*On the Aurora Borealis, Cumberland House. Extracted from the Journal of Lieut. Robert Hood, R.N.*—The most material information we had obtained at this period regarded the height of the Aurora from the earth. The following is the result of the observations that were made at the Basquiau Hill, and at the same time by Dr. Richardson at Cumberland House. The instruments used for the purpose were two small wooden quadrants, revolving on pivots, and furnished with plummets. Our chronometers were previously regulated, though great accuracy was not necessary in this particular, as the arches of the Aurora are sometimes stationary for many minutes. On the 2d of April, the altitude of a brilliant beam was  $10^{\circ} 0' 0''$ , at  $10^{\text{h}} 1^{\text{m}} 0^{\text{s}}$ , p. m. at Cumberland House. Fifty-five miles SSW it was not visible. As the trees at the latter station rose about  $5^{\circ}$  above the horizon, it may be estimated that the beam was not more than seven miles from the earth, and twenty-seven from Cumberland House. On the 6th of April, the Aurora was, for some hours, in the zenith at that place, forming a confused mass of flashes and beams; and in lat.  $53^{\circ} 22' 48''$  N, long.  $103^{\circ} 7' 17''$  W, it appeared in the form of an arch, stationary about  $9^{\circ}$  high, and bearing N by E. It was, therefore, seven miles from the earth. On the 7th of April, the Aurora was again in the zenith before 10, p. m. at Cumberland House, and in lat.  $53^{\circ} 36' 40''$  N, and long.  $102^{\circ} 31' 41''$ ; the altitude of the highest of two concentric arches at  $9^{\text{h}}$  p. m. was  $9^{\circ}$ ; at  $9^{\text{h}} 30^{\text{m}}$ , it was  $11^{\circ} 30'$ ; and at  $10^{\text{h}} 0^{\text{m}} 0^{\text{s}}$ , p. m.  $15^{\circ} 0' 0''$ , its centre always bearing N by E. During this time, it was between six and seven miles from the earth. After  $10^{\text{h}}$ , p. m. it covered the sky at Cumberland House, and passed the zenith at the other place."

"These observations are opposed to the general opinion of

meteorologists; they are nevertheless facts. We have sometimes seen an attenuated Aurora flashing across  $100^{\circ}$  of the sky in a single second; a quickness of motion inconsistent with the height of sixty or seventy miles, the least of which has hitherto been ascribed to it. This kind of Aurora is not brighter than the milky way, and resembles sheet-lightning in its motions."

"For the sake of perspicuity, I shall describe the several parts of the Aurora, which I term beams, flashes, and arches. The beams are little conical pencils of light, ranged in parallel lines, with their pointed extremities towards the earth, generally in the direction of the dipping needle. The flashes seem to be scattered beams approaching nearer to the earth, because they are similarly shaped, and infinitely larger. I have called them flashes, because their appearance is sudden, and seldom continues long. When the Aurora first becomes visible, it is formed like a rainbow, the light of which is faint, and the motion of the beams undistinguishable. It is then in the horizon. As it approaches the zenith, it resolves itself at intervals, into beams, which, by a quick undulating motion, project themselves into wreaths, afterwards fading away, and again brightening, without any visible expansion or concentration of matter. Numerous flashes attend in different parts of the sky. That this mass, from its short distance above the earth, would appear like an arch to a person situated at the horizon, may be demonstrated by the rules of perspective, supposing its parts to be nearly equidistant from the earth. An undeniable proof of it, however, is afforded by the observations of the 6th and 7th of April, when the Aurora which filled the sky at Cumberland House, from the northern horizon to the zenith, with wreaths and flashes, assumed the shape of arches at some distance to the southward."

"But the Aurora does not always make its first appearance as an arch. It sometimes rises from a confused mass of light in the east or west, and crosses the sky towards the opposite point, exhibiting wreaths of beams, or coronæ boreales in its way. An arch, also, which is pale and uniform at the horizon, passes the zenith without displaying any irregularity or additional brilliancy; and we have seen three arches together, very near the northern horizon, one of which exhibited beams and even colours, but the other two were faint and uniform."

"On the 7th of April, an arch was visible to the southward, exactly similar to that in the north, and it disappeared in fifteen minutes. It had probably passed the zenith before sunset. The motion of the whole body of Aurora is from the northward to the southward, at angles not more than  $20^{\circ}$  from the magnetic meridian. The centres of the arches were as often in the magnetic as in the true meridian."

"The colours do not seem to depend on the presence of any luminary, but to be generated by the motion of the beams, and then only when that motion is rapid, and the light brilliant.

The lower extremities quiver with a fiery red colour, and the upper with orange. We once saw violet in the former. The number of Auroræ visible in September was two; in October, three; in November, three; in December, five; in January, five; in February, seven; in March, sixteen; in April, fifteen; and in May, eleven. Calm and clear weather was the most favourable for observation; but it is discernible in cloudy weather, and through mists. We could not perceive that it affected the weather. The magnetic needle, in the open air, was disturbed by the Aurora, whenever it approached the zenith. Its motion was not vibratory, as observed by Mr. Dalton; and this was, perhaps, owing to the weight of the card attached to it. It moved slowly to the E or W of the magnetic meridian, and seldom recovered its original direction in less than eight or nine hours. The greatest extent of its aberration was 45'."

"A delicate electrometer, suspended at the height of fifty feet from the ground, was never perceptibly affected by the Aurora, nor could we distinguish its rustling noise, of which, however, such strong testimony has been given to us, that no doubt can remain of the fact. The conclusions to be drawn from the above will be found in the observations for the winter of 1820."

(*To be continued.*)

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## ARTICLE XV.

### *Proceedings of Philosophical Societies.*

#### ROYAL SOCIETY.

\* \* We endeavoured, last month, to give a full report of the important paper communicated by the President to the Royal Society, on the 5th of March; but writing only from memory, we have made two errors, one with respect to the rotation of the mercury not being stopped, but produced, by the approximation of the magnet; the other in the historical paragraph in the conclusion, which, as we have stated it, is unjust to Mr. Faraday, and does not at all convey the sense of the author. We wish, therefore, to refer our readers forward to the original paper, when it shall be published, for the correction of these mistakes.—*Edit.*

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*March 20.*—At this meeting the reading of the following paper, which had been commenced on the 13th, was resumed and concluded.

Of the Motions of the Eye, in Illustration of the Uses of the Muscles of the Orbit. By Charles Bell, Esq. (Communicated by the President.)

In this paper the author enters upon an examination of the motions of the eye, and the classification of the muscles according to their offices. This is done for the purpose of explaining the uses of the six nerves which enter into the orbit, and in pursuance of his former papers on the nervous system.

His object in this paper is to show, in the first place, that there are motions performed by the eye not hitherto noticed. Every time the eyelids descend to cover the transparent part of the eye, the eyeball ascends, or suffers a revolving motion. If this were not the case, the surface of the eye would not be moistened, nor freed from offensive particles. He proves, in the next place, that when we fall asleep, the eyeball is turned up, and the cornea lodges secure and moistened by the tears, under cover of the upper eyelid. He shows that these motions are rapid and insensible, and that they are provided for the safeguard of the eye. The other motions are voluntary, and for the purpose of directing the eye to objects. He then proceeds to the examination of the muscles of the eyeball, distinguishing them, as usual, into the straight and oblique muscles. It has been supposed, hitherto, that both these classes of muscles were voluntary; some describing the oblique muscles as coadjutors of the recti, and others as opponents to the recti; but Mr. Bell maintains that the obliqui are provided for the insensible motions of the eyeball, and the recti for those motions which are directed by the will, and of which we are conscious.

He proceeds to show, that the consciousness of the action of the recti muscles, gives us the conception of the place or relation of objects. He then proves by observation and experiment, that the actions of the straight muscles are inseparably connected with the activity of the retina; that is, with the enjoyment of the sense of vision: but that the moment the vision is unexercised, the eyeball is given up to the operation of the oblique muscles, and the pupil is consequently drawn up under the eyelid. Hence, the eyes are elevated in sleep, in faintness, and on the approach of death; and that distortion which we compassionate as the expression of agony, is the consequence merely of approaching insensibility.

Having examined the different motions of the eye and eyelids, and the muscles which are appropriated to them, the author promises, in the second part of the paper, to explain on this foundation, the distinction in the uses of the nerves of the orbit.

The Society then adjourned, in consequence of the approaching fast and festival, to meet again on

April 10, when the following paper was read: *An Account of an Apparatus on a peculiar Construction, for the Exhibition of Electromagnetic Experiments.* By W. H. Pepys, Esq. FRS.

This apparatus, constructed at the London Institution, under the direction of the author, consists of two plates, the one of copper, and the other of zinc, each two feet wide, and 50 feet

long; giving a total surface of 200 square feet. These plates are wrapped or coiled round a common centre, and are prevented from contact with each other by the interposition of three cords of hair line, and also of notched slips of wood placed at intervals. Two conductors of copper wire, nearly three-fourths of an inch in diameter, are attached, one to the zinc, and the other to the copper plate. In order that so large a mass may be readily employed for experiment, the apparatus is suspended by means of pulleys and a counterpoise, and so let down into a tub of acid, or, when not in use, into one of water. It requires 55 gallons of fluid.

This instrument exhibits very powerful magnetic effects: when the contact was made, a change in the direction of compass needles was produced, at the distance of five feet; steel bars enclosed in cylinders of glass, with a spiral of wire round them, were rendered magnetic, and several were suspended together; when the contact was broken, the bars fell, but one of them was immediately taken up again on restoring the contact, though it weighed above 270 grains. The electric intensity of the apparatus is very slight; it has not any decomposing action, and will not make a spark with charcoal, nor will it deflagrate the metals.

A paper was also read, On the Condensation of several Gases into Liquids. By M. Faraday, Chemical Assistant in the Royal Institution. (Communicated by the President.)

In this paper, Mr. Faraday described the results obtained by the application of the mode of condensation by which he had succeeded in liquefying chlorine, and Sir H. Davy muriatic acid gas, to several other aeriform bodies.

A portion of sulphuric acid being heated with mercury at one end of a sealed glass tube, while the other was kept cool by moistened bibulous paper, the *sulphurous acid gas*, which was evolved, condensed into a liquid in the cool end: the same result was obtained by forcing the dry gas into an exhausted tube with a condensing syringe, until its pressure became equal to three or four atmospheres. When the sealed tube was broken, the liquid expanded into pure sulphurous acid gas. The refractive power of *liquid sulphurous acid* is nearly that of water; the pressure exerted by its vapour in the tube was determined, by means of a mercurial guage, to be equal to two atmospheres. *Liquid sulphuretted hydrogen* was produced in the following manner: The small and closed leg of a bent tube was filled with muriatic acid; a piece of platinum foil, crumpled up, was next introduced; and then some fragments of sulphuret of iron; the platinum foil being interposed in order to prevent the two substances from contact until the tube was sealed, which operation would otherwise have been rendered ineffectual by the pressure of the evolved gas. When this had been done, the acid was made to

flow upon the sulphuret, and, in space of 24 hours, protomuriate of iron, and *liquid sulphuretted hydrogen*, were formed. When the tube was broken, under water, a portion of the gas which arose was collected, and found to be pure sulphuretted hydrogen, with which, also, the water was found to be impregnated. Sulphuric ether, when compared with this liquid, appeared adhesive and oily; the pressure which its vapour exerts, in the tube, is equal to 13 atmospheres, at 32° F. *Liquid carbonic acid* was produced in a similar manner, by means of sulphuric acid and carbonate of ammonia; but the strongest tubes were required for its formation, and tubes which had contained it for several weeks, often exploded with great violence upon a slight change of temperature. It was necessary to use a glass mask, goggles, &c. in the whole of these experiments; and some of them were attended with much risk to the author. The refractive power of liquid carbonic acid is much less than that of water; the pressure exerted by its vapour is equal to 40 atmospheres at about 45°. *Euchlorine* was liquefied, by its evolution, in a sealed tube, from chlorate of potash and sulphuric acid; in this state, it is of a deep-yellow colour, and quite transparent.

Some nitrate of ammonia, previously rendered as dry as possible by being heated to partial decomposition, was heated in a closed tube; and the results were, *liquid nitrous oxide* and water: the two fluids did not mix, or but in a slight degree. The refractive power of liquid nitrous oxide is lower than that of any of these fluids, and lower, indeed, than that of any other known liquid. Its vapour exerts a pressure equal to 48 atmospheres at 50°. *Liquid cyanogen* was formed by heating cyanuret of mercury; when the tube was broken, it became pure cyanogen gas.

The liquefaction of *ammoniacal gas* was effected by heating a portion of chloride of silver which had absorbed a large quantity of it, according to a property of this and of other chlorides formerly ascertained by the author. In this experiment, a curious combination of effects took place; as the tube cooled, the chloride began to re-absorb the ammonia, by the solidification of which, heat was liberated; while, at the distance only of a few inches, at the opposite end of the tube, cold was produced by the consequent evaporation of the liquid. At 60°, the whole of the ammonia became re-absorbed. The refractive power of *liquid ammonia* exceeds that of any other liquid described in this paper, and is even greater than that of water. *Liquid muriatic acid*, when the substances from which it is prepared are pure, is colourless, as Sir H. Davy had anticipated: its refractive power is nearly that of liquid carbonic acid.

All these liquids, with the exception of chlorine and euchlorine, are colourless; all are perfectly transparent, and highly fluid, and remain so at all temperatures to which they have been subjected; none of them exhibiting the least tendency to adhe-

tiveness at  $0^{\circ}$ . Experiments had been made, with a view to the liquefaction of oxygen, hydrogen, phosphuretted hydrogen, fluosilicic, and fluo-boracic gases, but these substances had hitherto resisted all powers of condensation that the author had been able to apply to them. With respect to the latter gas, this seemed to arise from its great affinity for sulphuric acid, as discovered by Dr. J. Davy, which is so great, that it even carries up that acid with it, in the form of vapour. Mr. Faraday intimated, however, that he should proceed with these experiments.

#### GEOLOGICAL SOCIETY.

*April 4.*—Two notices were read on a Recent Ligneous Petrification. By the Rev. J. J. Conybeare, MGS.

A notice was also read, respecting a Mass of Quartzose Ferruginous Sandstone, occurring in the Limestone near Bristol. By George Cumberland, Esq. Hon. MGS.

*April 18.*—A letter was read, containing "A Description of Two New Species of Encrinurus found in the Mountain Limestone near Bristol." By the same.

A letter was also read, "On the Geology of Pulo Nias, an Island on the Western Side of Sumatra." By Dr. Jack. (Communicated by H. T. Colebrooke, Esq. MGS.)

A paper was read, "On the Geology and Geography of Sumatra, and some of the adjacent Islands." By Dr. Jack. (Communicated by H. T. Colebrooke, Esq. MGS.)

#### ASTRONOMICAL SOCIETY.

*April 11.*—A letter was read from M. Pastorff to the late President, on a Photosphere observed at Buckholts, in Germany, round Venus, Jupiter, and Saturn.

At the same meeting was read, an Extract of a Letter from M. Littrow, Director of the Imperial Observatory at Vienna, to the Foreign Secretary, relative to the Cause of certain Discrepancies in Astronomical Observations; on the Construction of Instruments, and on Correction for Refraction.

### ARTICLE XVI.

#### SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS CONNECTED WITH SCIENCE.

##### I. *Alkanet as a Test.*

Dr. Robert Hare, of Philadelphia, finds that the colour of alkanet may be used instead of litmus, producing the same phenomena, but in a reversed order; for the alkanet infusion is made blue by an alkali, and restored by an acid, instead of being, as in the case of litmus, reddened by an acid, and restored by an alkali. Thus as the one is indi-

rectly a test for alkalies, so is the other for acids. When infusion of alkali is made blue for the latter purpose, the smallest quantity of alkali should be used that is adequate to the effect, in order to preserve the delicacy of the test.—(American Journal.)

## II. *Analysis of the Mineral Waters of Carlsbad.* By M. Berzelius.

These waters deposit a calcareous tufa, of a striated crystalline texture, which possesses all the characters of arragonite. Prof. Stromeyer's discovery, that arragonite always contains strontian, induced M. Berzelius to search for that earth in this tufa, and in the waters by which it is deposited; and he has succeeded in finding a small quantity of it in both. The following is the general result of his analysis of the waters of the principal spring, called the *Sprudel*.

In 1000 parts of the water, there are of

Sulphate of soda . . . . .	2.58714
Carbonate of ditto . . . . .	1.25200
Muriate of ditto . . . . .	1.04893
Carbonate of lime . . . . .	0.31219
Fluate of ditto . . . . .	0.00331
Phosphate of ditto . . . . .	0.00019
Carbonate of strontian . . . . .	0.00097
Carbonate of magnesia . . . . .	0.18221
Phosphate of alumina . . . . .	0.00034
Carbonate of iron . . . . .	0.00424
Silica . . . . .	0.07504
	<hr/>
	5.46656

With traces of carbonate of manganese.—(Ann. de Chim. et de Phys. xxi. 246.)

## III. *Fothergillian Prize Medal to be given by the Medical Society of London, Bolt-court, Fleet-street.*

In conformity with the will of the late Dr. Anthony Fothergill, the Society resolve to give annually to the author of the best dissertation on a subject proposed by them, a gold medal, value 20 guineas, called the "Fothergillian medal," for which the learned of all countries are invited as candidates.

1. Each dissertation offered for this prize must be delivered to the Registrar in the Latin or English language, on or before the 31st day of December.

2. With it must be delivered a sealed packet, with some motto or device on the outside; and within, the author's name and designation; and the same motto or device must be put on the dissertation, that the Society may know how to address the successful candidate.

3. No paper in the hand-writing of the author, or with his name affixed can be received; and if the author of any paper shall discover himself to the Committee of Papers, or to any member thereof, such paper will be excluded from all competition for the medal.

4. The prize essay will be read before the Society, at the meeting preceding the Anniversary Meeting of the Society, in March, 1824.

5. The prize medal will be presented to the successful candidate, or his substitute, at the Anniversary Meeting of the Society.

6. All the dissertations, the successful one excepted, will, if desired, be returned with the sealed packets unopened.

One dissertation only on the subject "Dropsy," proposed by the Society for the Fothergillian medal, to have been adjudged in March, 1823, having been presented, the Society thinking it probable that from the recent establishment of the prize, it had not been sufficiently made known to the medical faculty, have deferred the adjudication of the prize for the best dissertation on the subject of "Dropsy," to another year.

The subject of the Essay for the gold prize of the ensuing year is "Diseases of the Spine."

## ARTICLE XVII.

### NEW SCIENTIFIC BOOKS.

#### PREPARING FOR PUBLICATION.

Mr. Goldsworthy Gurney is about to publish a Series of Lectures on the Elements of Chemical Science, lately delivered at the Surrey Institution.

Mr. Oliver has in the press, Popular Observations on Muscular Contraction, with a Mode of Treatment of Diseases of Limbs associated therewith.

In the press, a Translation of De Humboldt's Geognostical Essay on the Superposition of Rocks.

Dr. Antommarchi, formerly Professor of Anatomy in the University of Pisa, and Surgeon to the late Ex-Emperor at St. Helena, is publishing an Anatomical Work, to be comprised in 80 Plates, representing the whole Structure of the Human Body, except the Integuments, in Figures of the natural Size. An explanatory and descriptive Sketch will be added to every Plate in which the multiplicity of figures might otherwise create confusion. The Work is edited by M. le Comte de Lasteyrie.

Mrs. Holderness is employed on a Work on New Russia, being some Account of the Colonization of that Country, and of the Manners and Customs of the Colonists.

#### JUST PUBLISHED.

A Journal of a Voyage to the Northern Whale Fishery, including Researches and Discoveries on the Eastern Coast of West Greenland, made in the Summer of 1822, in the Ship *Baffin*, of Liverpool. By W. Scoresby, Jun. FRSE. Commander. With 8 Plates. 8vo. 16s.

Dendrologia Britannica, or Trees and Shrubs that will live in the open Air of Britain throughout the Year. By P. W. Watson. Royal 8vo. Parts I. to IV. 4s. 6d. each. Each Number contains 8 coloured Engravings.

Hortus Cantabrigiensis, or an Accented Catalogue of Plants, Indigenous and Exotic, cultivated in the Cambridge Botanic Garden. By the late J. Donn, FLS. and FHS. Tenth Edition, with numerous Corrections and Additions, by John Lindley, FLS. &c. 8vo. 10s. 6d.

**Elements of Experimental Chemistry.** By William Henry, MD. FRS. &c. The Ninth Edition, greatly enlarged and recomposed throughout. Illustrated with 10 Plates by Lowry, and numerous Wood-cuts. In 2 Vols. 8vo. 1*l.* 14*s.*

**Narrative of a Journey from the Shores of Hudson's Bay to the Mouth of the Copper Mine River, and thence along the Coast of the Polar Sea, &c.** By Capt. John Franklin, RN. Commander of the Expedition. With an Appendix containing Subjects of Natural History, &c. by J. Richardson, MD. Surgeon to the Expedition, and others. Illustrated by 4 Maps, 8 Plates of Natural History, and 24 Engravings by Finden, from Drawings by Lieuts. Back and Hood. 4to. 4*l.* 4*s.*

Among the new productions at the present Leipzig Fair, is the first Volume of MM. Martin and Spix's Travels in Brazil, during the Years 1817, 1818, 1819, and 1820; with an Atlas, in imperial Folio, of 15 Lithographic Plates of Portraits, Views, Geological and Botanical Charts, &c. This Volume contains their Travels through Rio Janeiro, St. Paul, Minas Geraes, Goyaz, Bahia, &c.

Also:—*Animalia Nova quæ in Itinere jussu et auspiciis Max. Jos. Bav. Regis per Brazilium suscepto observavit, et depingi curavit Dr. Joannes de Spix.* 39 Plates.

*Plantæ Novæ quæ in Itinere jussu et auspiciis Max. Jos. Bav. Regis suscepto observavit, et depingi curavit, Dr. Carolus de Martin.* 100 Plates.

**The Naturalist's Repository, or Monthly Miscellany of Exotic Natural History,** consisting of elegantly coloured Plates, with appropriate scientific and general Descriptions of the most curious, scarce, and beautiful Productions of Nature that have been recently discovered in various Parts of the World. By E. Donovan, FLS. &c.

## ARTICLE XVIII.

### NEW PATENTS.

J. Taylor, of Raven-row, Mile End, Middlesex, master mariner, for a new method of constructing the bottoms of merchant-ships, and placing the pumps so as to prevent damage to the cargoes by the bilgewater.—Jan. 16.

J. Smith, of Old Broad-street, merchant, for certain improvements on a machine for washing, cleansing, and whitening cotton, linen, silk, and woollen garments, or piece goods.—Jan. 20.

W. Glossage, of Leamington Priors, Warwickshire, chemist and druggist, for a portable alarum, to be attached to, and detached from, clocks and watches, and which may be regulated to take effect at any given period of time.—Feb. 11.

N. Partridge, of Bowbridge, near Stroud, Gloucestershire, dyer, for improvements in the setting or fixing of steam-boilers or other coppers, by which a considerable saving of fuel will be effected, and the smoke more effectually consumed.—Feb. 14.

Fuller, of Bath, Somersetshire, coach-builder, for an improvement in the construction of shafts, and the mode of attaching them to two-ages.—Feb. 18.

## ARTICLE XIX.

## METEOROLOGICAL TABLE.

1883.	Wind.		BAROMETER.		THERMOMETER.		Evap.	Rain.	Daniell's hyg. at noon.
			Max.	Min.	Max.	Min.			
3d Mon.									
March 1	N	W	30.12	30.03	45	31	—		
2	W		30.11	29.83	48	30	—	04	
3	W		29.83	29.45	51	38	—	04	
4	N	W	29.55	29.44	48	37	—		
5	N	W	29.88	29.55	45	33	—		
6	N	W	29.88	29.57	40	24	—		
7	S	E	29.57	29.19	39	31	—	—	
8	N	W	29.27	29.19	43	30	—		
9	N		29.88	29.27	44	26	—		
10	S	E	29.84	29.77	44	31	—	26	
11	N	W	30.19	29.84	48	32	—		
12	N	W	30.35	30.19	48	30	—		
13	S	W	30.35	30.33	52	32	—		
14	S	W	30.50	30.33	52	38	.95	06	
15	N		30.55	30.50	50	30	—		
16	N		30.55	30.25	48	32	—	06	
17	N	W	30.25	29.97	52	38	—	—	
18	N	W	29.97	29.82	50	30	—	16	
19	N	W	29.93	29.89	40	28	—	—	
20	S	W	29.89	29.49	50	36	—	40	
21	S	W	29.49	29.20	52	40	—	07	
22	S	W	29.69	29.20	55	40	—	01	
23	N	W	30.28	29.69	48	35	—	—	
24	E		30.39	30.28	55	33	—		
25	E		30.28	30.22	46	35	—		
26	N	E	30.22	30.15	44	39	—		
27	N		30.15	30.14	44	32	—		
28	E		30.17	30.14	52	38	—		
29	E		30.14	30.12	51	39	—		
30	E		30.22	30.12	63	38	—	07	
31	N	W	30.22	30.16	58	47	.80		
			30.55	29.19	63	24	1.75	1.17	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

## REMARKS.

*Third Month.*—1. Fine. 2. Cloudy. 3. Drizzling rain: a furious gale of wind from the NW all night. 4. The wind continued to blow with great violence all day. 5. Fine. 6. Cloudy: bleak. 7. Snowy. 8. Cloudy. 9. Fine. 10. Rain. 11, 12. Fine. 13—15. Cloudy. 16. Fine. 17. Cloudy. 18. Drizzly. 19. Snowy morning: very cold wind NE in the night. 20. Snow in the morning: afternoon rainy: a lunar halo in the evening. 21. Drizzly. 22. Overcast. 23—26. Cloudy. 27. Cloudy: night foggy. 28. Cloudy and fine. 29. Cloudy. 30, 31, Fine.

## RESULTS.

Winds: N, 4; NE, 1; E, 5; SE, 2; SW, 5; W, 2; NW, 12.

## Barometer: Mean height

For the month..... 29.952 inches.

For the lunar period, ending the 4th. .... 29.676

For 15 days, ending the 12th (moon south)..... 29.618

For 15 days, ending the 25th (moon north) ..... 30.049

## Thermometer: Mean height

For the month..... 41° 25.9°

For the lunar period..... 38.033

For 29 days, the sun in Pisces..... 39.433

Evaporation..... 1.75 in.

Rain..... 1.17

*Laboratory, Stratford, Fourth Month, 28, 1823.*

E. HOWARD.

ANNALS  
OF  
PHILOSOPHY.

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JUNE, 1823.

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ARTICLE I.

*A new and easy Method of ascertaining the Degree of Temperature at which Water is at its Maximum Density.* By Mr. James Crichton.

(To the Editor of the *Annals of Philosophy*.)

SIR,

Glasgow, May 2, 1823.

HAVING lately been much engaged in determining the specific gravities of certain fluids, by means of adjusted balls of glass, and being satisfied that for simplicity and accuracy, no method whatever is nearly so good; I was led to think, that another important point could thereby with greater certainty be ascertained, than by any mode yet adopted. This is to determine the temperature at which water attains its maximum density.

Of all who have hitherto attempted to decide this question, whether British or foreign philosophers, no one seems to speak with the precision which might be desirable, of the degree at which the phenomenon takes place. The French say it is between 4 and 5 of Celsius, thus admitting an uncertainty of about 2° of Fahrenheit; some in our own country think it is at 39, while others place it at 40.

Any person who is aware of the many sources of error, and of the vague nature of the requisite compensations, will not wonder at this indecision; the difficulty alone, of maintaining an uniform temperature, throughout a large or deep body of water is very considerable; hence the bulkiness of the solid used by

the French for this purpose, having been a cylinder nine inches in diameter, and of the same height, must have rendered it a matter of uncommon difficulty in the quantity of water necessary. Whether this uniformity existed, at the moment of its greatest apparent gravity, may admit of some doubt, however carefully and constantly the thermometer may have been observed; besides, air-bubbles, which it would be almost impossible to see or remove, might have considerably increased the buoyancy of the suspended solid. To estimate the compensations for expansion, in the above-mentioned method, is perplexing, and for the mode by the weighing bottle is still more so; but to ascertain the quantity of hygrometric humidity, which profusely and rapidly fixes on the exterior surface of a bottle, at so low a temperature as 40, is perhaps from several causes impracticable. A hope of being able to assist in obviating these embarrassments, induces me to present a new method of determining this point.

Having frequently observed that a very small alteration of temperature in a fluid, destroyed the precise poise of a solid in that fluid, and that an extremely minute increase or diminution of gravity in the solid, has a similar effect; it was easy to perceive, that if water is of a certain gravity just above freezing, and that if it become heavier, with an increase of temperature, before it reach, say for example, 50, then it is manifest, that at *some* included degree, water must of necessity poise, or sustain, a ball or solid of greater specific gravity, than it will do at any other point in the supposed interval.

My first attempt to ascertain this point, evinced, that a ball which was just poised, at about 33, had the same property near 51; this gave 42 for the point of greatest density, taking the half of the intervening degrees as additive to 33, or the reverse from 51, since all authorities seem to agree, that the expansion is the same for equal intervals of temperature, on both sides of the maximum.

It may be supposed, that to adapt a ball of the greatest possible specific gravity which water can sustain at its greatest gravity, would be the next endeavour; it was, but so infinitely little is the variation of the gravity of water, for about a half degree on either side of the maximum point, that although I have, more than once, diminished the gravity of balls which were too heavy, by a quantity so minute, as not to amount to the 6000th part of a grain, or just as little as I could by any means grind off, still, on trial at the proper range of temperatures, it was found that the mark had always been overshot. This then was relinquished as a hopeless task.

As it had not however escaped notice in the course of these experiments, that the further the temperature of water was removed from that of the greatest gravity, the ball rose, or fell, *with celerity just commensurate to the number of degrees which*

the existing temperature was above, or below, that of the desiderated degree: this, therefore, affords some idea of the approach to, or retrocession from, the temperature in question; but there is a better, and perhaps conclusive proof of its place in the scale, which I shall now describe.

I took a glass jar, 2 inches in diameter, and  $3\frac{1}{4}$  in depth; into this was put distilled water to the depth of  $2\frac{1}{4}$  inches, and cooled down to near the freezing point, but carefully prevented from congealing, as the disengagement of air-bubbles from the ice, when fluidity took place, would have frustrated the experiment; into this was put a ball, previously well wiped with a silken\* cloth, and immediately, by means of a clean hook of glass, lifted, but not rapidly, twice or thrice out of the water; this cleared it of any air-bubbles, which though imperceptible, might have been adhering to its surface. The ball now fell to the bottom of the jar, which as usual was convex, but had a small flat surface on the summit, to which the ball was led, and there it rested. In the water there were suspended two very accurate and sensible thermometers, the bulbs being at the middle of the water as to depth, and just so far removed from its diametrical centre, as not to be in the path of the ball when it rose.

In these circumstances, the lower end of the ball was carefully watched with a large reading glass, and at the moment of its quitting the bottom of the jar, the thermometers were examined, and the degree noted; when the ball had risen about one-fourth of an inch a small rod was cautiously let down, and without agitating the water, gently made to touch the ball; it of course descended, but instantly rose; this is a very delicate part of the experiment, and if overdone loses its effect. It was frequently repeated, and the ball always reascended with accelerated velocity.

The thermometers indicating an increasing temperature, the ball finally became stationary at the surface of the water; from time to time it was slightly touched as before, but in proportion as the temperature rose beyond a certain point, the tendency of the ball to ascend, after these strokes, obviously diminished, judging by the velocity with which it did so; its upper extremity, when examined with the magnifier, plainly seemed to press as it were more and more feebly on the surface of the water, till at last, a fine thread of separation became visible; the degree by the thermometers was again marked, and as they continued slowly to rise, the ball gradually fell to the bottom of the jar.

From many similar experiments I have concluded that 42 is extremely near the true point of the greatest density of water; my most satisfactory trials never gave 3-10ths of a degree less

\* In an experiment of such delicacy, this must be attended to, as linen never fails to leave fibres on whatever is wiped with it; these will detain air enough to render the efforts of the experimenter in this case abortive.

nor more, but at present, I am rather inclined to place it a very little above 42; a trial I made in very favourable circumstances a few days ago, gave for the first appearance of the rising of a ball 37·5, and for that of its sinking 46·3, these make the point in question 41·9; the local temperature was 46·8, but the barometer having been at only 29·4, the above 41·9 may be held perhaps too low. These experiments were made with balls adapted to all the intervals from 33—51 to 39—45, yielding however great uniformity of results.

As I cannot anticipate what objections, or if any, can be made to this method of ascertaining a curious and not unimportant point, I shall allude to one only; that is error from expansion of the ball, and consequent increase of its volume; but as the whole range required does not exceed  $4^{\circ}$  or  $5^{\circ}$ , on either side of a starting point, and though it were granted that the expansion of glass is the same for  $4^{\circ}$  about temperature 42, as it is for  $180^{\circ}$ , that is from freezing to boiling of water, as determined by M. de Luc and others, the expansion for these  $4^{\circ}$  must be so extremely little, as not by any means to affect the decision in any considerable part of a degree.

But were the expansion of glass in the above range even ten times what it is, still it must in effect be cancelled, for taking 42 as the point where this expansion in the present case must be assumed as incipient, and granting that at say 33, a ball just held in poise has become less, that is *heavier* specifically, some degree above 33, for example 34, where water is denser, must really be what the ball virtually indicates; again, if at 51 the same ball poises, then by a parity of reasoning the ball is now said to be increased in volume beyond what it was at 42, or it is too *light*, therefore it must indicate too high a degree, or it *really* shows that the ball, supposing it inextensible, would have stood at a *lower* degree or denser medium, which call for instance 50; so that by the one extreme thus correcting the other, the conclusion to be drawn is the same as in the case of altogether neglecting the expansion.

The low temperature of the atmosphere when these experiments were made, gave confidence that no current upward or downward moved the water; besides, a few very minute particles of dust, just visible in different parts of it, remained entirely motionless during the whole operation.

My first trials on this subject were made with spherical balls, half an inch in diameter, having a depending stalk of about an equal length; but to obviate the possibility of error from dissimilarity of the extremities, I latterly used solids resembling in shape a buoy or parabolic spindle, sharp at the ends, of about an inch in length and 4-10ths in diameter. This shape gave another apparent advantage, that is of meeting less resistance than a sphere when moving in a fluid, and in order to ensure perpendicularity of the axis, before such a ball was hermetically sealed, a

small globule of mercury was introduced, which perfectly answers that purpose.

As the momentum of an ascending ball is very apt to cause its upper extremity to rise above the water, and however free of any thing unctuous, it will there remain too long, a slight tap or blow by a small hammer, on the under side of the table, will obviate this incident.

In cooling water for such experiments, it ought to be kept as still as possible; agitation to procure uniformity of temperature has a bad effect by charging it with air; bubbles may settle on the ball during the experiment, and must be closely watched for, as their effect may be apprehended, if detected occasionally rising through the water. Knowing the degrees at which a ball might be expected to rise or fall, I have frequently lifted it to the surface of the water a short time before, in order to free it of any thing which though imperceptible might have affected its gravity. The thermometers were sometimes placed one at the top, and the other at the bottom of the vessel, in order to ascertain beyond doubt the temperatures at the initial points, or the extremes of the above-mentioned intervals.

When it was considered how uncertain the indications might have been, had I succeeded in adjusting a ball to seeming equilibrium at the maximum gravity, owing to the minute variations near that point, there was little cause for regretting my failure, especially when the method by varied extents of intervals seems so satisfactory; still, since writing the above, another effort was made, when the following appearances took place.

Water in the jar being near 42, and the ball as seen by the naked eye in apparent equilibrio, it was observed with the reading glass as seen over a slight scratch on the side of the jar; it was then very slowly descending; having two or three times breathed on the part of the jar nearest the ball, the consequent dimness was removed by a camel hair brush, but before this could be done and the eye-glass applied, the ball had decidedly begun to ascend, which it continued to do for a few seconds, and after a momentary pause again began to fall. This was repeated several times, the thermometer meanwhile ranging from 42 to 42.6; from this and other circumstances, I with due deference incline to think, that 42.3 is very near the true point in the scale of temperature, where the maximum density of water takes place. In this last trial it may not be improper to mention, that the increment of weight producing the approximating effect, was a mere speck of leaf gold, attached to the side of the ball by means of spirit-varnish, and fixed by applying a moderate heat.

Having thus given an explicit account of these experiments, apology on my part for having too minutely done so, will be deemed quite unnecessary by any one who repeats them. I shall only add, that the thermometers having been made pur-

posely for the experiment, I have perfect reliance on their indications. The smallness of the apparatus and its extreme simplicity, render the determination of the point wherever it is to be placed, a very plain matter. This I submit to those who are capable of availing themselves of the means it affords, and who are qualified for appreciating its powers.

JAMES CRICHTON.

## ARTICLE II.

*On a Salt composed of Sulphuric Acid, Peroxide of Iron, and Ammonia.* By Dr. Forchhammer.

(To the Editor of the *Annals of Philosophy*.)

DEAR SIR,

*Copenhagen, April 8, 1823.*

HAVING prepared a solution of gold by means of nitric acid and muriate of ammonia, and precipitated the gold by protosulphate of iron, in order to determine whether any further portion of gold might be obtained by evaporation, the solution was concentrated to the consistence of a syrup, and suffered to remain for a month, when beautiful octohedral crystals of a wine yellow colour were formed on the sides of the vessel. It first occurred to me that this salt might be a peculiar compound of sulphuric acid and peroxide of iron; but I soon discovered that it also contained ammonia, and that it was an alum in which peroxide of iron supplied the place of alumina.

This salt is soluble in about three times its weight of water at 60°, and, by repeated crystallization, it may be obtained perfectly colourless; the regular octohedron is the most usual form of this salt, and although the cubo-octohedron sometimes occurs, I never obtained any perfect cubes.

Fifty parts of this salt were dissolved in water, and precipitated by nitrate of barytes, 50·33 of sulphate of barytes were obtained, containing 17·298 of sulphuric acid = 34·596 per cent.\*

One hundred parts of the salt were dissolved in water, and decomposed by ammonia, the precipitate was digested during 24 hours in a solution of soda, and this, upon the addition of muriate of ammonia, deposited slight traces of alumina, which increased by ebullition, and weighed, when dry, 0·26 part. In another experiment, from 60 parts of the salt, 0·19 of alumina was obtained. The oxide of iron after being redissolved in muriatic

\* In this and the following calculations, the proportions are given from Berzelius's tables (Stockholm, 1818). If the more simple numbers of Dr. Thomson are adopted, the result differs very little from that deduced from Berzelius's numbers.

acid and precipitated by ammonia weighed in the first experiment 16·37. From 50 parts of the salt in two subsequent experiments, I obtained 8·32 and 8·2, the mean of which is  $8·233 = 16·47$  per cent. Fifty parts of the salt were exposed for three days to a temperature near that of boiling water; when weighed, they had lost  $21·74 = 43·48$  per cent.; the remaining powder was readily dissolved in water, excepting a very small quantity of a brownish-red substance.

I found great difficulty in ascertaining the exact quantity of ammonia contained in this salt. I dissolved 100 parts in water, added caustic potash to the solution, and subjected it to distillation, so as to pass the vapour into a solution of nitrate of lead; although the subnitrate thus precipitated by ammonia is nearly insoluble in a solution of neutral nitrate of lead; it is, however, soluble in water, and when I washed the filter on which it was collected, it almost entirely disappeared. I attempted to expose these solutions of the subnitrate to the action of carbonic acid, which decomposes the subnitrate into carbonate and neutral nitrate, and then the quantity of carbonate of lead would be proportional to the quantity of ammonia passed through a neutral solution of the nitrate; this method did not, however, succeed much better, nor was nitrate of mercury employed with much greater advantage. At length on comparing the numbers for sulphuric acid and oxide of iron, I found that if I considered the persulphate to be composed, according to the proportions indicated by Berzelius as constituting what he terms *sulphas ferricum*, one-fourth of the sulphuric acid remained uncombined with oxide, and, I conceive, this quantity to be combined with ammonia.

One hundred and fifty parts of the salt were dissolved in water in a flask, and mixed with potash sufficient to decompose the whole; a glass tube was adapted to the flask, and immersed into a solution of 50 parts of the salt in question; the mixture in the flask was boiled for half an hour, and every precaution was taken to prevent the escape of any ammonia. At the close of the experiment, the solution through which the ammonia had passed was colourless; it slightly restored the blue colour of reddened litmus paper, but this effect did not take place unless the paper remained for some time in the solution; the precipitate had a yellowish colour indicating an admixture of subsulphate of iron with the peroxide; the solution and precipitate were heated in a close vessel for 24 hours to a temperature of about  $100^{\circ}$ ; the yellow colour of the precipitate had now disappeared, and it seemed to be pure peroxide of iron; the alkali of the solution was also so perfectly expelled that it did not produce any effect upon reddened litmus paper; but when mixed with a reddened solution of litmus, a very slight trace of alkali was discoverable.

The oxide of iron was washed until the water passing through the filter ceased to affect the solution of nitrate of barytes; the

oxide was afterwards dissolved in muriatic acid, and again tried with nitrate of barytes, which occasioned a degree of turbidness that indicated rather more sulphuric acid than would have been required to saturate the very slight trace of alkali.

It was thus proved by direct experiments that three parts of sulphuric acid are combined with peroxide of iron and one part with ammonia; it is extremely probable that the excess of sulphuric acid in the last experiment is derived from some persulphate of iron uncombined with ammonia, and upon this also depends the yellow colour, and this, as already mentioned, may be removed by repeated crystallization, a little subsulphate being deposited. The salt which I have now described is composed of

Persulphate of iron	41·807	comp. of	25·337	acid +	16·470	oxide
Sulph. of ammonia	12·366			8·649	acid +	3·717 amm.
Sulphate of alumina	0·870			0·610	acid +	00·260 alum.

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55·043

Although the quantity of water contained in the salt appeared, as already stated, to be 43·48 per cent. by direct experiment, yet it is well known to be difficult to deprive a salt of all the water, unless it be heated to redness, which the nature of this salt would not admit of. If we assume the ammonia or the sulphuric acid combined with it to be one atom, 23 atoms of water will supply the loss in the analysis, for it would amount to 44·947, and the loss is 44·457. It is, however, to be observed, that the estimate of 23 atoms of water would agree with the analysis of the salt, upon the supposition that the whole quantity of sulphuric acid belongs to it; but this is not the case; for it contains some persulphate of iron which is not combined with ammonia, and which is not united with the same proportion of water as the triple salt.

On closely examining the crystals, I observed that they effloresce slightly, and become brown on the surface. In order to decide the question, I employed Dr. Thomson's method of calculating the exact quantity of nitrate of barytes necessary to decompose the whole of the sulphate of ammonia and iron. On the supposition that it contains 24 atoms of water, 11·83 parts of sulphate of iron and ammonia would exactly decompose 13·06 of nitrate of barytes, and if only 23 atoms of water, then 11·61 of the triple salt would be required to decompose the same quantity of the nitrate. The crystals employed in this experiment were rendered perfectly colourless by repeated crystallization, and when decomposing such a portion of them by means of that quantity of nitrate of barytes which indicates the presence of 24 atoms of water, I found so little sulphuric acid remaining unacted upon, that when more nitrate of barytes was added no turbidness was occasioned for several minutes. This slight

excess of acid was, perhaps, owing to the presence of the small quantity of sulphate of alumina, for this salt contains more sulphuric acid than the triple salt, and no allowance was made in the calculation for the excess.

If, therefore, we neglect the alumina, the composition of the salt in question will be

Persulphate of iron .....	41.95
Sulphate of ammonia .....	12.11
Water. ....	45.94

100.00

I conceive that the salt which Mr. Cooper obtained by dissolving the oxide of iron precipitated from the permanganate by ammonia in sulphuric acid, is similar to that of which I have now given the analysis. The quantities of sulphuric acid and peroxide of iron are nearly in similar proportions; the greater quantity of water which he assigns may be accounted for by the ammonia having been included with it.

As the results deducible from this analysis seem to agree so well with M. Mitscherlich's idea, that peroxide of iron and alumina are isomorphous, and afford additional proof of the correctness of his views, I was more particularly anxious to determine the quantity of water, with as much accuracy as possible. For although the crystals of alum belong to the *formes limitées* of Haüy, which may occur in crystals of very different substances; yet if in two such compound salts there exists a close analogy between their component parts and their form, it deserves attention; and on this account I subjected alum formed of sulphate of alumina, sulphate of ammonia, and water, to analysis. One hundred parts of this ammoniacal alum were dissolved in water, and decomposed by nitrate of barytes; the sulphate obtained weighed  $104.46 = 35.90$  of sulphuric acid; a like quantity of the same alum decomposed by ammonia gave 11.24 of alumina. From the known composition of sulphate of alumina, it is evident that one-fourth of this sulphuric acid is not combined with alumina, and it must, therefore, be united with the ammonia, and on this supposition, the composition of ammoniacal alum will be:

Sulphuric acid .....	35.90
Alumina. ....	11.50
Ammonia .....	3.86

51.26

Water and loss .....	48.74
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100.00

Making sulphuric acid the basis of the calculation, and on account of the great weight of the sulphate of barytes the errors of experiment are diminished, we find that 24 atoms of water

are equal to 48.68, agreeing with the above result as nearly as could be expected. This proportion of water was confirmed by mixing 11.16 parts of the alum with a solution of 13.06 of nitrate of barytes, which are the proportions required by calculation on the supposition that the salt contains 24 atoms of water; there was a slight excess of sulphuric acid, but the addition of nitrate of barytes did not occasion any turbidness for some minutes.

This alum is, therefore, composed of three atoms of sulphate of alumina, one atom of sulphate of ammonia, and 24 atoms of water; and the triple salt now described consists of 3 atoms of persulphate of iron, 1 atom of sulphate of ammonia, and 24 atoms of water; or if we adopt the number assigned by Dr. Thomson to peroxide of iron, 1 atom of tripersulphate of iron, 1 of sulphate of ammonia, and 24 atoms of water.

### ARTICLE III.

*On Clay Slate for Road-making.* By Mr. W. Stokes.

(To the Editor of the *Annals of Philosophy*.)

SIR,

*Harcourt-street, Dublin.*

IN many parts of Ireland is found a species of clay slate which answers remarkably well for road-making. This clay slate may be described as follows:

It occurs massive; colour of the fresh fracture, a dark bluish-grey. The colour of that which has been exposed to the air is a brownish-red. Lustre, in some places, greasy; in some exhibiting brilliant points: a few places have a blistered appearance. The fracture undulating, slaty; yields readily to the knife; it feels slightly greasy; opaque; specific gravity, 2.6. Before the blowpipe, it melts into a greenish slag.

The advantages which this stone possesses as a road material are remarkable, and might seem inconsistent were they not verified by experience.

Although easily broken it does not form light dust so as to rise with the wind; it is not readily worked into mud by carriage wheels or the feet of cattle, although roads made of other species of clay slate are very liable to this defect. Roads formed of this peculiar clay slate have an agreeable smoothness under the horse's feet, and seldom exhibit projecting stones; neither do they throw out loose stones, which is a common defect of our limestone roads. They preserve their evenness in decay remarkably, continuing smooth when only an inch of gravel remains on the surface; they are so porous that even though they have lost their central elevation, water does not rest on them.

The material is not favourable to the growth of grass, which is another advantage; roads of common clay slate and of limestone are often edged by a grassy bank, which, if it be not frequently removed, confines the water, and so forms a water-course on the road. Garden walks of this material are durable and convenient, being but little overrun with weeds, but it must be confessed they have a sombre appearance.

The districts in which this material is employed are in the county of Down, between Downpatrick and Ballynahinch, Ballynahinch and Clough Banbridge and Tandragee, and between Belfast and Bangor; in the county of Armagh, Market Hill and Armagh, and from Armagh to Castle Blaney, in the county of Monaghan. In the south of Ireland, between Waterford and Ross.

The stone from Dunmeniss, near Ballynahinch, being submitted to analysis in the usual manner, viz. by fusing it with an alkali, yielded the following constituents :

Silica .....	59·4
Alumina.....	17·4
Lime.....	2·1
Magnesia.....	2·2
Oxide of iron.....	11·6
Water.....	6·4
Charcoal and manganese.....	Trace
Loss.....	0·9
	<hr/>
	100·0

WILLIAM STOKES.

#### ARTICLE IV.

*Observations on Gas Light Establishments, with an Account of some Experiments made to determine the comparative explosive Force of Carburetted Hydrogen Gas and Gunpowder.* By Sir William Congreve, Bart. FRS. &c.\*

THE first observation that arises on taking a general view of the gas works in the metropolis, is the immense extent to which they have now been carried since the year 1814, when there was only one gasometer of 14,000 cubic feet in existence in Peter-street, Westminster, belonging to the chartered company, as they were termed; the only company then established. Whereas, at present, there are four great public companies established, having, altogether, 47 gasometers at work, capable of contain-

\* From Reports on Gas Light Establishments. Ordered to be printed by the House of Commons, March 26, 1823.

ing, in the whole, 917,940 cubic feet of gas, supplied by 1,315 retorts; these retorts consuming upwards of 33,000 chaldrons of coal in the year, and producing upwards of 41,000 chaldrons of coke. The whole quantity of gas generated annually being upwards of 397,000,000 cubic feet, by which 61,203 private, and 7,268 public or street lamps are now lighted in the metropolis. In addition to these great companies, who are the subject of this Report, as being placed by legislative enactment under the control of the Secretary of State, there are in the metropolis several private companies, whose operations are not included in the foregoing statements; but on which I shall feel it right to make some remarks before I conclude.

In Table, No. 8,\* will be seen the details of all the above-mentioned totals; the following consideration of which will, I think, point out the importance of thus having collected these particulars into a single point of view, as tending to regulate the just price which ought to be charged to the public for the gas they consume; and, at the same time, by the general comparative view thus obtained of the whole system, as tending to induce every company to adopt the best mode of conducting its works. Presuming, and believing, as I do, in the correctness of the particulars of information given me by the different companies, as to the quantity of coal consumed in their retorts, and the average workings of those retorts during the year; it appears, by a review of these totals, either that the average consumption of gas per lamp, allowing for waste, is not so great as is generally stated; or that the quantity of gas generated from a chaldron of coals, is greater; or that the average times which these lamps are allowed to remain lighted, is not so great as reported; for if the consumption or waste of gas were equal to what is generally believed, or the quantity produced were not greater, I find that the number of lamps returned could not have been lighted for the times stated.

The general statement made by the companies of the consumption of gas for each lamp, is five cubic feet per hour; and that of the produce of gas per chaldron, something less than 12,000 cubic feet per chaldron; now allowing full 12,000 cubic feet per chaldron to be generated, it appears that the average consumption of gas per lamp in the hour by the London Company, does not exceed four cubic feet and a half; nor is the average time of burning, winter and summer, for private lamps, more than four hours; or for public lamps, more than nine hours, which is below the supposed average.

By Table, No. 8, it also appears that in the City of London Gas Works, the consumption of gas per lamp, allowing the same time of burning, is six cubic feet and a half per hour, instead of four, as above stated. It is evident, therefore, that there exists

\* For this table, as well as other interesting particulars, we must refer the reader to the original report.—*Edit.*

a waste of gas in these works, which is avoided by the London Company; and, indeed, this waste is admitted at the works, and even stated at a higher rate than it thus appears to be.

The various and important checks, therefore, that may thus be obtained, show the necessity of these general investigations, and of the calculations arising out of them. I shall, therefore, in future Reports, cause a still more detailed survey of them to be made than has in the commencement of the present system of inspection been hitherto practicable.

By thus determining the actual consumption and waste of gas to each lamp, together with the expense of generating it, allowing for wear and tear and outlay of capital, which must also form the subject of future surveys, more in detail, a just estimate, not only of the actual cost of the gas to each company, but of its real value, supposing the best system to be adopted, may thus be made with a view to a fair regulation of prices, and to such other measures as the legislature may think fit to enact for the further proceedings of this great public concern.

It might have been imagined that the enormous quantity of coal thus shown to be consumed in the generation of gas, would have greatly enhanced the price and increased the quantity of coals brought into the port of London since the introduction of the gas-light system; this, however, has not been the case; on the contrary, the increase in either of these is inconsiderable, owing to the compensation arising from the quantity of the surplus of coke produced.

The quantity of coke generated, in the first instance, is at the rate of 120 or 130 chaldrons for every 100 chaldrons of coal carbonized; of this quantity, about 20 per cent. is consumed in the furnaces under the retorts, in the process of carbonization; the whole of that operation being now effected by the coke produced on the spot, without any addition of coal whatever; deducting, therefore, 20 per cent. from the increased measurement of coke produced, there remains, in measurement, nearly, or perhaps rather more than the same quantity of surplus disposable coke to be applied as fuel in the metropolis, as of coal used in the process; so that the coke seems nearly to amount to a compensation for the coal as fuel. But were the enhanced price of coal greater than it is, it could scarcely be doubted, that, as a question of economy, there would be a very considerable public saving arising out of this system, when the value of the light thus produced were set against this increased price; and certain it is, that some parts of the population of this metropolis are peculiarly benefitted by it. The poor have, by this means, access to a very serviceable fuel, at a comparatively moderate price; the coke being now sold by the companies at from 20s. to 30s. per chaldron; and the fire produced from it being, in all respects, adequate to the necessities of the lower orders.

The small rise in the price of coals, and the great fall in that of oil, may indeed be considered as a sort of measure of the saving to the public that has arisen from this application of coal; nevertheless there may be many very important considerations both of external and internal policy, which may render it very desirable that the government should give every possible encouragement to the use of the gas produced from oil. Not only our distant fisheries seem to point to the expediency of this policy, viewed as a principal nursery for seamen (which from the foregoing statement of the little increase in the consumption of coal, evidently receives no aid from the use of the coal gas) but the encouragement of our coast fisheries, as an additional means of subsistence for an increased population, renders it expedient that the utmost encouragement should be given to this mode of augmenting the consumption of oil; nor can it be doubted, that if such enhanced demand could be created for the description of oil that would thus be required, it would form a most powerful stimulus to the coast fisheries.

Thus far very little progress has been made in the extension of the use of oil gas in the metropolis since my last Report; I shall, however, state some particulars respecting this part of the subject at the conclusion of the present Report.

It is satisfactory to observe, that although considerable extensions have been made in this mode of lighting the town during the last year, no serious accident has happened: the only one indeed which deserves mentioning is that which occurred in the vaults under the Opera House, of which I transmitted a particular account at the time.

It is also satisfactory to observe, that very considerable improvements have been made in some of the processes in the different works, as well in the mode of setting the retorts as in that of charging them; and that some of the probable causes of mischief stated in my first Report have been removed; among which I may state, that the practice of making use of the coal tar in the tanks, for floating the gasometers, has been entirely discontinued.

Most of the new gasometers which have been lately erected are uncovered; by which means the probability of accident by the leakage of the gas into the gasometer houses is obviated; and the only objection that existed to making this a general practice (namely, the effect of the wind or snow in disturbing or increasing the pressure of the gasometer when exposed) is proposed to be got rid of by the addition of a small regulating gasometer, from which the mains are immediately fed.

Another very great improvement in the mode of conducting these works has been introduced in the evaporation under the retorts of the lime water; by which the gas is purified. By this means a great nuisance has been obviated which used to exist

in allowing this lime water to run into the sewers, by which a very offensive smell was produced in the neighbourhood, and a very deleterious matter conveyed into the Thames.

This mode of disposing of the lime water was first introduced by the superintendant of the Brick-lane station, and will I doubt not be very generally adopted, as this water is prohibited from passing into the sewers after the 15th February, in consequence of a prosecution carried on by the City of London against the Dorset-street Company.

Another mode of getting rid of this lime water has been experimented; viz. the purification of the gas by dry lime, which is about to be adopted in Dorset-street, in preference to the common mode of passing it through lime water.

Considerable improvements have also been made during this year in the quantity of gas produced from a given quantity of coal, which, as far as it tends to reduce the cost of the gas, not only renders a benefit to the public, but an increased advantage to the companies, in whose well-doing the public cannot but be interested, since this mode of lighting the town has now become so general, and is unquestionably attended with so many advantages to the metropolis, not only as relates to the convenience of the inhabitants, but to their security as a matter of police. This increased produce of gas has been accomplished by various means, such as the improvements in the mode of setting the retorts, in the use of a better coal, in the application of a certain quantity of the coal tar mixed with the coal in the retort.

A new description of retort, called the Graftonian retort, from the name of its inventor, has also been introduced, with the hope of further economy by the increased production of gas. This retort is made of fire brick instead of iron, and it is supposed that it will be found much more durable than the iron retort, and capable also of sustaining a greater intensity of heat, so as to render the distillation of the coal more perfect.

Other experiments have been made with a view to this same object, by the engineer of the Imperial Company, who has invented a retort, which is to be gradually fed with the coal from a hopper, so as to make the destruction of the material to be carbonized still more complete. Mr. Clegg seems to anticipate, from the experiments which he has tried, that he should be enabled by this means to destroy both the tar and ammoniacal liquor produced in the ordinary modes. He also calculates, that by these means the quantity of gas produced will be increased to 16,000 or 18,000 cubic feet per chaldron, and that it will moreover be of a superior quality, so as to give considerably more light in proportion to the quantity burned. It would certainly be very desirable to get rid, as much as possible, of the different products resulting in the distillation, excepting the coke, as some of them do not pay for their removal, and are liable, there-

fore, to be left for some time on the premises, so as to become more or less nuisances to the public.

At present the system is far from perfect in any respect. It is hoped, therefore, that every possible encouragement will be given by the different companies to that description of investigation and experiment calculated to promote improvement, although no immediate profit may result to the companies.

It is also very desirable, that when any improvement is realized by any particular company, that it should be generally adopted by the rest; and by the due exertion of this spirit, I have but little doubt that a great degree of perfection, both as to convenience and security, may be looked for at no distant period.

Great care, however, is unquestionably requisite, and there are certain points, the observance of which by the different companies is so essential, that any indifference or neglect as to them must be vigilantly prevented; for it must be obvious that the vast accumulation of inflammable gas necessary to carry on this mode of illumination, in its present extended state, must be attended with more or less risk, according to the conduct of those employed. To explain, therefore, somewhat more definitely than has hitherto been done, the nature and extent of this risk, I have caused the following experiments to be tried at Woolwich since my last Report, with a view to bring the explosive forces of different mixtures of carburetted hydrogen and atmospheric air into a comparison with gunpowder.

The results are as follow:—The following mixture was fired from a cylinder constructed for this purpose, to project a ball weighing 7 lbs. 2 oz.; the cylinder being suspended by a pendulum, to measure the degrees of recoil.

First experiment.		Feet.	Average recoil of cylinder.
Carburetted hydrogen, 288 cubic inches, and 1,440 cubic inches of common air, being in the proportion of one-sixth of hydrogen to five-sixths of atmospheric air, were fired.	Ranged the ball, on an average of three rounds.	94	64°
Gunpowder (8 drachms) was found to produce nearly the same effect.			
	Average range of the same ball.	77	64

It appears, therefore, that mixed in this proportion, viz. one-sixth of carburetted hydrogen to five-sixths of common air, one cubic foot of this gas would be equal to three ounces of gunpowder; that 480 cubic feet are, therefore, equal to one barrel; and that the contents of one gasometer of 15,000 cubic feet are

equal to 31 barrels. Fifteen such gasometers, therefore (the number in Peter-street), would, with this mixture, produce a force equal to that of 465 barrels of gunpowder.

A second experiment was subsequently made with a greater proportion of the carburetted hydrogen.

Second experiment.		Feet.	Recoil of cylinder.
346 cubic inches of carburetted hydrogen, and 1,382 cubic inches of common air, being one-fifth to four-fifths were fired.	Average of three rounds with the same ball.	158	76°
16 drachms of gunpowder were found to produce nearly the same effects.		113	74

One cubic foot, therefore, of gas, mixed in this proportion, would give an explosive force equal to five ounces of gunpowder, or 288 cubic feet equal to one barrel; one gasometer, therefore, of 15,000 cubic feet so mixed, would be equal to 52 barrels and a quarter, and 15 such gasometers to 783 barrels and three-quarters of gunpowder.

For the mere purpose of determining the risk of large accumulations of carburetted hydrogen gas, it was evidently unnecessary to try any further experiments, since it was thus proved, that if by any accident the contents of one of the gasometers in Peter-street were to be supposed to have escaped into the gasometer-house, so as to form a mixture with the surrounding atmosphere, in the proportion of one-fifth of gas, an explosive mixture would be created equal to upwards of 50 barrels of gunpowder; and that if the contents of the whole of the gasometers at this station could be supposed to have escaped in like manner, an explosive force equal at least to 700 barrels might be created; and that on the same principle, the entire leakage of one of the large gasometers of 31,000 cubic feet in Dorset-street, might produce an explosive mixture equal to about 135 barrels of gunpowder.

It is true, that in the ordinary condition of the carburetted hydrogen contained in the gasometers, it is not explosive; it wants the admixture with common atmospheric air to make it so; but then it must not be forgotten, that this requisite ingredient is the circumambient atmospheric air, that surrounds the gasometers, and is in constant contact with every part of them, so that the union of the two ingredients in certain dangerous proportions may always be effected, either internally or externally, by unforeseen accidents; as the destruction of a gasometer by lightning, producing both the mixture and the ignition at the same moment; the same by the burning of the gasometer-

house; by the oversetting of a gasometer, from the breaking of its chain of suspension, an accident which lately happened at Brighton, and which has happened also by the bursting of the tank in which the gasometer floats. The constant leakage of an old gasometer is also a probable cause of explosive mixture. At all events, these are evidently probabilities of accident quite sufficient to show the absolute necessity of the utmost vigilance and precaution.

But even supposing this mixture not to take place, I apprehend that serious consequences might attach to the mere inflammation and combustion of a large deposit of inflammable air, even in an unexplosive state.

It appears to me, that such a combustion, by lightning or otherwise, would produce very destructive effects to the neighbouring buildings, and gasometers, merely by the sudden rarification and expansion of the air caused by it, and would, in fact, produce a violent detonation and concussion, without supposing any combination of oxygen and hydrogen, but merely by the simple conflagration of the hydrogen; under these circumstances, and especially with reference to the experiments made at Woolwich, by which it appears, that the explosive force is greater than has usually been assigned,\* I shall feel it right in concluding this Report, distinctly to point out such precautions, in this and in other respects, as I think ought to be enforced for the security and convenience of the public.

In viewing the probable causes of disappointment to the public which might occur from the want of the due management of this now greatly extended concern, I have been induced to make very particular inquiries as to the probability of the stoppage of the pipes, by which the gas is conveyed to the different parts of the metropolis. The result is certainly satisfactory, as I find that in the cast iron pipes or mains not the smallest deposit or corrosion takes place; there is, however, a very considerable tendency to stoppage in the service pipes, which are made of wrought iron, insomuch, that many of these pipes have been found completely choked up in four or five years use.

The pipes, also, in the interior of the shops and houses, and for the immediate conveyance of the gas to the lamps, which pipes are generally made of copper, are liable to stoppage by internal corrosion. The wrought iron service pipes may, however, be conveniently replaced by lead, which is not subject to this deposit, and the copper pipes by those of block tin, both of which have been so used with good effect.

It is satisfactory, therefore, to observe, that that which would be a general and extensive evil (if there existed any liability of stoppage in the mains), has no existence at all, while that which is comparatively a less evil, being merely partial, may be easily

\* Probably from the experiment having been made on a larger scale than any before tried.

remedied, by the substitution of one material for another. These are, therefore, points to which the attention of all the companies should be directed.

It is further also to be observed, that as the deposit by which these pipes are liable to be stopped, is a sulphate either of iron or copper, this evil may be greatly diminished by the greater purity of the gas.

This, therefore, becomes an additional reason for urging the utmost attention to the process of perfectly purifying the gas previously to its entering the pipes, independent of many other advantages, such as the diminution of its offensive odour in every part of its course and application, and of the mischief which occurs to gilding, furniture, &c. in the rooms in which impure gas is burned.

*Regulations which it appears important to enforce.*

Enough has already been stated on the probability of accident and the extent of danger, to prove the necessity of enforcing certain restrictions as to the quantity of gas suffered to be accumulated within a given space; by which restrictions not only the size of the gasometers hereafter to be constructed, or the quantity of gas to be allowed to be admitted into those already constructed, shall be limited, but by which also the proximity of these gasometers to each other shall be regulated, as well as their distance from any inhabited buildings. I shall now, therefore, proceed to the consideration of these restrictions.

Much unwarrantable and inconsiderate extension has proceeded in all these respects since the first Report of the Royal Society on this subject in 1814; and it may not now be practicable, without exceedingly embarrassing the companies, and, consequently, inconveniencing the public, both as to the price and as to the supply of gas, to make any immediate reform; but I have no hesitation in saying, that the gasometers now generally in use contain a much greater quantity of gas than can be accumulated with perfect safety; and that they are placed much too near to each other, and too near to some of the most populous parts of the town.

The recommendation contained in the Report of the Royal Society in 1814, limited the size of the gasometers to 6000 cubic feet; the expense, however, attending this limitation, from their contents being so small in proportion to their surfaces, is such, that I conceive it might be expedient to seek for an equivalent security by other precautions, and to allow a greater latitude as to the magnitudes of the gasometers. Taking, therefore, ample space around each gasometer, I do not know that there would be much to apprehend from the use of gasometers measuring about 20,000 cubic feet, and working between 15,000 and 20,000 of these feet; but with such quantities in each, I am decidedly of opinion, that these gasometers should not be placed nearer

to each other than 40 feet, or about a diameter of one of the gasometers ; and that if they are nearer, a strong brick traverse should be immediately erected between them, similar to those used in the powder works at Waltham Abbey. I am decidedly of opinion also, that these gasometers should not be suffered to exist nearer than from 50 to 100 yards to any dwelling-house, without being similarly traversed.

But under such restrictions, I should consider the safety of the town as well provided for in respect of the gas works, as by that regulation relative to the quantity of gunpowder allowed to be stored in any warehouse, which limits that quantity to two barrels.

In carrying this restriction, as to the size of the gasometers, into effect with as little inconvenience as possible to the companies, I should recommend, as the readiest and cheapest mode, that where larger gasometers exist, their altitudes should be diminished, or, at least, that they should not be allowed to work these gasometers above a certain altitude ; and that if the expence of reducing their depths were desired to be dispensed with, some permanent stop should be applied to them to prevent their rising above a certain height, so as to contain more than the limited quantity of gas. I should recommend also, that where these gasometers are, as in many of the establishments is the case, nearly close to each other, every other one should be taken out and removed to a distance ; nor need this involve any necessary increase of ground *on the spot* where the gasometers are now placed (the purchase of which, in some cases, might be impracticable), but merely a removal of these gasometers to a distance ; the communication with the retorts, &c. being, as it may be, without difficulty, effected (and as, in fact, it now is at the South London Works), at a distance by pipes under ground. I should also recommend, that a general regulation should be enforced to prevent any gasometer being placed within 50 yards of any retort or other part of the works in which fire is used. Such appear to me to be the principal regulations necessary as to the size and situation of the gasometers ; and it was on these principles, in a reference lately made to me by the Lords Commissioners of the Treasury, of an application from the Imperial Gas Light and Coke Company, for permission to purchase an additional quantity of land, that I recommended that this permission should be granted, although it had, in passing the Acts of Parliament, been considered desirable to limit the grants of lands, that the works might not be erected on too large a scale.

My view of this subject is, that it may be quite right to limit the quantity of gas generated in particular works ; but that I certainly would not limit the quantity of ground on which the apparatus, for its generation, is to be erected.

With reference to the particular construction of the gasometers and gasometer houses, from all that I have been able to

ascertain, I should recommend that the gasometers should be erected in the open air, or, at all events, with the slightest and most ventilated covering possible, seeing that there is much greater chance of explosion in the gasometer house, where a very small leakage of gas from the gasometer, may produce the explosive mixture, than in the gasometers themselves, into which large portions of atmospheric air must find their way to create danger; and this I am the more induced to recommend, since I find that a remedy has been devised for all the inconveniences that might otherwise be apprehended by the intervention of a small regulating gasometer between the principal gasometers and the mains, as already stated; an arrangement which, it appears to me, ought to be generally adopted, as it would also be attended with other good effects, in addition to those above-mentioned; among which may be noticed the prevention of accidents from the irregular flaming of the lamps in shops, and especially that very serious cause of accidents in such situations, which was noticed in my first Report as being likely to occur from the falling of a gasometer at work, supposing its chain to break, by which, in the present state of the works, the flame of the lamps connected with that gasometer would, on a sudden, be made to blaze up to so great a height as could not but be attended with the most serious consequences in a great variety of situations; all of which accidents would be entirely prevented by the regulation in question.

In the foregoing general observations, I have stated the liabilities to stoppage by corrosion, which exists in the wrought iron service pipes in the streets, and also in the copper feeders in the inside of the houses; and I have there mentioned the facility which, fortunately for the perfection of this system, exists, of getting rid of this inconvenience by the substitution of leaden and block tin pipes; I shall now add, as an important regulation for the general adoption of which, I conceive, the government should exert its authority; that all the gas pipes, whether mains or services, ought to be laid in a bed of clay, firmly rammed round the pipe. The necessity of this regulation not only manifests itself constantly in walking through the streets of the metropolis, from the frequent recurrence of offensive smell arising from the leakage of the pipes; but was clearly proved by the accident which happened in the wine vaults under the arcade of the Opera House, into which the gas had found its way from a leak in a service pipe passing over the crown of this vault, though no gas was laid on in any part of the premises. The recurrence of much accident, and the general nuisance above stated, would be effectually guarded against by this coating of clay round the pipes; in addition to which, a saving of gas now lost by leakage, would ultimately accrue to the companies. The expense, in the first instance, might be considerable, and, it is to be regretted, that it was not originally attended to, because it

would not certainly be now advisable, generally, to break up the streets for that purpose; but I should distinctly recommend, that whenever new pipes are laid, or old ones exposed for repair or otherwise, this measure should be universally enforced.

There is another subject to which I feel it right to call the serious attention of the government, conceiving that some general regulation in this respect would be a material benefit to the public, as well as to the companies.

I allude to the limitation of particular districts to particular companies; at present, as may be seen by the accompanying plan, the mains of more than one of the first-formed companies, principally the London and City of London Gas Light Companies, are carried into the same district, and indeed in many places into the same streets, lanes, and even alleys, so that adjoining houses, in the most confined situations, are frequently supplied by different companies, the pipes of these different companies intersecting and mixing in a very complicated manner.

From this it frequently happens, that in case of the nuisance of leakage, it is unknown from what pipes this nuisance proceeds. A hesitation is thus, therefore, constantly manifested as to which company is to open the ground; disputes arise, and much delay and inconvenience is the consequence, not only from the nuisance being thus prolonged, but from the more frequent breaking up of the pavement, naturally arising from a double set of mains and services than if only one set were allowed in the same district.

The only argument, as it appears to me, that could be adduced against this limitation, would be as to any supposed effect that might arise from the encouragement of the competition of different companies in keeping down prices; but this objection I apprehend may be settled, by regulating the general prices with less difficulty than exists in the present state of the supply. And this regulation of prices, as I have already stated, is one of the advantages that may be fairly calculated upon as an early result from the due exertion of the control of the government.

Some of the companies, from the complaints, disputes, and other inconveniences, attending the present arrangement, are very desirous of entering into a compromise for the limitation of their districts, and have offered an exchange of mains, &c. value for value; and I must confess that it appears to me advisable, that a general regulation on this subject should be adopted as soon as possible. The latter charters indeed have proceeded upon this principle. The Imperial Company, for instance, is thus limited to particular districts, and those districts are exclusively granted to them.

I shall now only state one more regulation, which I think it would be very important to have enforced, that not only should accurate plans of the present actual state of all the gas works

in the metropolis be transmitted to the person charged with the general inspection of them, but that no new work, nor any extension of old works, should be proceeded upon until regular plans of them shall have been laid before the inspector, and until the sanction of the Secretary of State shall have been procured by him for their proceeding.

It is but right that I should take this opportunity of stating, that I have received every facility and attention on the part of the directors of the different companies to my requests for information; but I have not yet been favoured with plans, such as those above-mentioned, without which it is impossible to enter with that accuracy into all the details of such a survey, which the magnitude, risk, and general importance of these works demand. It is indeed evidently the interest of the companies concerned, to furnish as early as possible, before they undertake new works, plans of their intended operations, inasmuch as prevention is better than remedy, and that it is easier to make alterations in a drawing than in a building; nor can I refrain from repeating, that it is much to be regretted that such an arrangement was not long since carried into effect, as the thorough security of the town and the perfect organization of the system might have been insured, at a much less expense to the companies than will now be ultimately required.

On this part of the subject it remains only that I should state, that having thus far, at the request of the government, devoted myself to the consideration of this important public concern, I am ready to proceed in giving my best assistance in seeing that those preventions, which I have suggested for the safety of the metropolis, are properly carried into effect, in the performance of which I should feel it incumbent upon me, equally upon public grounds, to consult the interests and accommodation of the companies as far as it could consistently be accomplished with reference to the primary object, the convenience and security of the public.

On the subject of the coal gas works, it remains only to be observed, that there are several private gasometers erected for the lighting of manufactories and other buildings, where the operation is confined entirely to the particular premises, such as the Royalty Theatre, Messrs. Ward and Ainger's, south side of Blackfriars-bridge, and many others.

There are other coal gas works also formed, not only for the purpose of lighting particular premises, but which extend their operations by the sale of the gas for lighting the neighbouring buildings also.

And there are other private establishments created entirely for the sale of coal gas to the public. The mains of some of these works extend to very considerable distances. The Ratcliffe Company, for instance, have three gasometers, and have brought their mains nearly up to the Tower of London. App-

ther of these factories is formed at Poplar, and another in Cromer-street, Brunswick-square.

It does not, however, seem necessary that I should now go into any further details respecting these works, though it is important that I should state that they have been established *entirely without Act of Parliament or other licence*, and that it appears to me, as these works are liable to accident and nuisance, as well as those carried on upon a larger scale, though not, perhaps, the same extent, that they ought also to become the object of such rules and regulations as the government may deem it expedient to determine upon for the conduct of the principal works—and that no gas works whatever should be allowed to be formed without a licence in the first instance, and without being subjected to subsequent inspection and control.

#### *Oil Gas Works.*

I ought not to close this Report without adding a few words more on the subject of oil gas, although there is no part of the application of this modification of the system, which at present regularly falls within the scope of this Report.

I am sorry to say, that but little extension in this branch of the gas lights has taken place in the metropolis since my last Report, although it has been adopted by some of the largest provincial towns, namely, Liverpool, Plymouth, Cambridge, and Taunton, and, as I understand, Dublin also.

In London one public company\* only has been established, which is situated near Old Ford; these works consist of one gasometer, 30 feet in diameter and 12 feet deep, supplied by 12 retorts, of which, however, not more than three or four are at present kept in action, as the quantity of gas consumed does not exceed 6,000 cubic feet daily.

These retorts are charged only once a day, a continued small stream of oil being kept constantly dropping into them.

One gallon of oil is calculated to produce 100 cubic feet of gas, the illuminating power of which is stated to be equal to more than 300 cubic feet of coal gas.

The oil gas requires only to be passed through a cistern of oil for its purification. The oil undergoes no change in the process, so as to deteriorate it, or render it offensive, like the lime water used in the purification of coal gas, neither is there any residuum left in the distillation by which a nuisance can be created.

Oil of any inferior description is capable of producing good gas, though some of the vegetable oils are the best.

About seven miles of main are attached to these works, running from the London Hospital, Whitechapel, to the four mile-stone, Stratford.

\* The capital granted to this company by Act of Parliament is 10,000*l.* of which 6000*l.* has been already raised.

Each house served with this gas is furnished with a small gasometer, to measure the quantity of gas consumed, for which the consumer pays at the rate of 5s. for 100 feet.

There can be no doubt that the light thus produced by oil gas, taking the different intensities into the account, is much cheaper than that produced by the direct burning of oil; nor indeed is the expense of oil gas greater than that of coal gas, if we admit the intensity of its light to be three times that of an equal quantity of coal gas, or that one burner supplied with oil gas is equal to three with coal gas.

The charge for 1000 cubic feet of coal gas is 15s.; and the expense of 3000 feet of coal gas, therefore, is 45s. which is stated to be *scarcely* equal to 1000 cubic feet of oil gas costing 50s.; now if the values in proportion to the light obtained thus nearly approximate, it certainly appears to me, that the oil gas must be nearly, if not quite as cheap, taking into the account the great reduction in the first outlay for the apparatus required for generating the oil gas, compared to that required for the coal gas, and reckoning also the difference in the required size of the mains. And indeed the extensive introduction of this system into the provincial towns now going on, is to my mind a convincing proof of the correctness of the foregoing statement, and makes it only matter of greater surprise to me, that so little has yet been done as to the public use of oil gas in the metropolis, the more especially when its greater security, from the reduced quantity necessary for a given light, its greater purity, and less offensive smell, are taken into consideration.

I must not, however, omit to say, that an oil gas company, called The Portable Oil Gas Company, is about to be formed in London. The principal object of this company is to supply detached houses with this gas, in situations where no mains are laid down. For this purpose strong copper vessels are prepared of different dimensions, in which the oil gas is deposited by compression; so that a vessel of one cubic foot has been made to contain 16 cubic feet of this gas; and if each cubic foot of this gas is supposed to be equal to three cubic feet of coal gas, there would such a vessel contain oil gas sufficient to supply one lamp for nine hours.

The idea is in some cases to attach a vessel containing a considerable supply on this principle to small feeding pipes leading to fixed lamps in different parts of the building; and in some cases, to have the reservoir containing the condensed gas, made part of a moveable or portable lamp.

The first of these modes seems to me most likely to be useful, limited, however, as above-mentioned, to situations where no mains are laid on, and where it is desired to avoid the trouble of generating the gas on the spot.

The latter seems to be still more limited in its application, as few moveable lamps could be conveniently constructed, which

would allow of a reservoir being attached to them sufficiently capacious for general uses.

These plans are certainly very ingenious, though it cannot be denied that there must be considerable inconveniences and expenses attaching to the constant removal of the reservoirs between the factory where the gas is to be generated and compressed, and the house where it is to be consumed; nevertheless, I am informed, that a considerable capital has been already subscribed to carry the project into effect.

To this spirit of enterprise it must be left to overcome the difficulties of the undertaking; I have only to consider the danger; and to this, as the scheme advances, it will be my duty, or the duty of whoever undertakes this inspection, to look seriously and to call the attention of the government; for here unquestionably we find introduced a new description of danger altogether; in the highly compressed state into which it is found necessary for the sake of portability to condense the gas. There is evidently *some risk* in forcing 16 atmospheres into a light copper vessel, when *quite new*; but when such vessel has been long in use and long strained by the constant efforts of the confined gas, the danger of its bursting must be considerably enhanced.

Precautions may it is true be taken to render an explosion as little mischievous as possible, and the vessels may be calculated of ample strength in the first instance; yet it is difficult to ascertain the probable effects of accidental explosion, or the constant strength of materials, and more especially where the compression first given to the gas is liable to subsequent increase by heat.

With respect to the oil gas it remains only to be stated, that though so little extension of it in public works has taken place in the metropolis, its use has been considerably advanced in private establishments; and I shall conclude by offering it to consideration, how far all these applications of this gas, as well as of the coal gas, ought to be subjected to some sort of licence and regulation.

WILLIAM CONGREVE.

## ARTICLE V.

*On the Development of Electromagnetism by Heat.* By the Rev. J. Cumming, MA. FRS. and Professor of Chemistry in the University of Cambridge.

(To the Editor of the *Annals of Philosophy*.)

DEAR SIR,

Cambridge, April 30, 1823.

THE following, selected from some experiments which were the substance of a paper read at the last meeting of the Cambridge Philosophical Society, as, I believe, they are new, will, I hope, be found interesting to your readers.

All the metals, not excepting fluid mercury, produced a deviation on the magnetic needle of a galvanoscope, provided the extremities of the metallic bars employed were at *different* temperatures.

It was indifferent whether the connecting wires were united to the bars by soldering, rivetting, or simple contact.

Bismuth and antimony (the metals which seemed most efficacious) gave with copper wires, deviations in opposite directions.

A bar of bismuth four and a half inches long, half an inch broad, and one-eighth thick, gave a *positive* deviation of  $21^{\circ}$  on a compass needle, four and a half inches long, at the melting point of bismuth; at  $180^{\circ}$  and  $100^{\circ}$ , the deviations were respectively  $12^{\circ}$  and  $5^{\circ}$ , the cooler end being constantly at  $60^{\circ}$ .

A similar bar of antimony, with the utmost heat of a spirit-lamp, gave a *negative* deviation of  $19^{\circ}$ .

When a few drops of ether were made to evaporate from one extremity of these bars, the other being kept at a constant temperature of  $60^{\circ}$ , deviation was produced, but, of course, in the opposite direction.

A slip of palladium, weighing 35 grains, having silver wires rivetted into it, when made red-hot at one extremity, gave with a small compass needle a deviation of  $70^{\circ}$  *positive*; with platina wires  $10^{\circ}$  *negative*.

A similar slip of platina with silver wires gave a *positive* deviation of  $65^{\circ}$ ; with platina wires  $4^{\circ}$  *negative*.

A bar of zinc cast upon copper wires gave a *negative* deviation of  $45^{\circ}$ ; the same bar with zinc wires gave a deviation of  $2^{\circ}$ ; with silver wires a deviation of  $2^{\circ}$ ; and with iron wires a deviation of  $3^{\circ}$ , all *positive*; but with platina wires a deviation of  $50^{\circ}$  *negative*.

A copper bar with zinc wires gave a deviation of  $20^{\circ}$ ; with copper wires a deviation of  $10^{\circ}$ ; and with silver of  $30^{\circ}$ , all *positive*; but with platina wires a deviation of  $18^{\circ}$  *negative*.

A small silver bar with silver wires gave a *positive* deviation of  $20^{\circ}$ ; but with platina wires  $50^{\circ}$  *negative*, both at a red heat.

A brass bar with platina wire gave  $10^{\circ}$  negative deviation; with silver wire  $20^{\circ}$  positive; with brass wire  $15^{\circ}$ , and with zinc wire  $25^{\circ}$ , both positive.

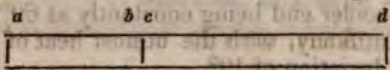
An iron bar with brazed copper wires, and the heat of two lamps, gave  $45^{\circ}$  of negative deviation; it likewise deviated negatively with wires of platina.

Two connecting wires, each composed of a platina and a silver wire soldered together at one of their extremities, were rivetted into a copper bar: when the silver ends were rivetted to the bar, the deviation was *positive*; when the platina, *negative*; but when the platina ends were shortened to half an inch, the deviation again became *positive*.

Quicksilver inclosed in a glass tube, eight inches long, and half an inch in diameter, with copper wires, gave at the temperatures  $170^{\circ}$  and  $115^{\circ}$  the corresponding deviations  $8^{\circ}$  and  $3^{\circ}$  with the small compass.

A double bar, eight inches long, composed by soldering together at the middle a bar of bismuth and another of antimony, when heated at *both* extremities, and kept at a temperature of  $60^{\circ}$  in the middle, gave a deviation of  $36^{\circ}$  with the large compass at the melting point of bismuth.

An alloy of bismuth and antimony gave with the large compass a deviation of  $3^{\circ}$  *negative*; on continuing the heat it returned to zero, and at the melting point of the bar, it became  $4^{\circ}$  *positive*.

A bar of bismuth, six inches long, was broken into two parts;  , *a b* and *c d* of two

inches and four inches in length, and soldered together again with a thin plate of copper interposed. When heated at *a* or *c*, the deviation was *positive*; and when at *b* or *d*, was in the opposite direction; but when the bar was unbroken, the deviations had been in the same direction, whether the heat were applied at *a*, *b*, or *c*.

A rod of bismuth having wires soldered to it in different parts of its length, the included portions being alternately hot and cold, exhibited as many poles as there were wires.

Two bars, whose deviations were respectively  $20^{\circ}$  and  $16^{\circ}$ , when connected in sequence gave a deviation of  $23^{\circ}$ ; the deviation was not so great when the hot and cold ends were connected respectively.

A battery of eight bars produced some, but not a considerable increase of power.

A bar which produced a deviation of  $20^{\circ}$  with four feet of copper wire, of 1.21 of an inch diameter, gave with 8, 16, and 32 feet of the same wire, corresponding deviations of  $16\frac{1}{4}^{\circ}$ ,  $10^{\circ}$ , and  $7^{\circ}$ .

With eight feet of copper wire, of 1.37th inch, the deviation

was  $6\frac{1}{4}^{\circ}$ ; with the same length of platina wire of 1-100th inch, not more than  $\frac{1}{4}^{\circ}$ .

With four feet of copper wire of 1-12th inch, the deviation was  $21^{\circ}$ , and was not increased by using two such wires.

In all these experiments, the deviation was in the same direction throughout every part, both of the bars and of the connecting wires.

No difference of effect was observed, whether the bars were after fusion cooled slowly or rapidly.

The effects were not materially increased by increasing the dimensions of the bars, or of the surfaces in contact.

A tourmaline, which by heat exhibited the opposite electric states very strongly, produced no deviation on the magnetic needle, when its ends were connected with the galvanoscope by silver wires coiled round them.

For a comparison between the electromagnetic effects as produced by heat and by the usual process, it was observed that rods of zinc and copper, each 7-10ths of an inch in diameter, and distant 4-10ths, when excited by equal parts of muriatic acid and water, produced a deviation of  $27^{\circ}$  on the large compass; and rods of 3-10ths distant 3-10ths inch, gave  $40^{\circ}$  with the smaller.

Believe me, dear Sir,

Very sincerely yours,

J. CUMMING.

## ARTICLE VI.

*Experiments to determine the Practicability of an Operation for Phthisis Pulmonalis, proposed by Dr. Carson. By David Williams, MD.*

(To the Editor of the *Annals of Philosophy*.)

SIR,

Liverpool, May 4, 1823.

THE following experiments were instituted from a desire to examine a very important physiological hypothesis, advanced by Dr. Carson. That gentleman has published a pamphlet,\* consisting of three essays: one, on the Elasticity of the Lungs; another, on the Vacuity of the Arteries after Death; and the third, on Lesions of the Lungs. In the second, or that on the Vacuity of the Arteries after Death, he relates two methods that he adopted for killing animals, by admitting air through different parts of the parietes of the chest into both its cavities simultaneously, thereby collapsing the lungs. One method was by making an opening of about an inch in length between a pair of the ribs on each side of the thorax. The other was by making

\* *Essays, Physiological and Practical.* By James Carson, MD.

an opening fit to admit his two fingers from within the abdomen, through the muscular part of the diaphragm on each side: instantaneous death was the result of the latter method in all his trials. In the former, death was not less certain; though a little more tedious; the life of one animal only (a large dog) was protracted so long as twenty minutes. In his third essay, or that on Lesions of the Lungs, Dr. Carson details an experiment performed on three rabbits as follows: An incision was made between two of the ribs on one side into the cavity of the chest, the air was freely admitted, and he concluded that the lung must have been reduced to a state of collapse. The wound was then allowed to close and heal. At the distance of five days, a similar opening was made into the cavity on the opposite side, when one out of the three rabbits soon expired after the second operation. Dr. Carson inferred from the results, that it is possible to collapse one of the lungs, and to retain it in that state, *ad libitum*, by keeping open the communication between the cavity of the chest and the external air; and further, that upon allowing the opening to close, the lung in a given time will recover its wonted function, thereby rendering it practicable, when conceived necessary, to place the opposite lung under the like discipline. Dr. Carson has very ingeniously proposed the application of his views to the cure of one of the most frequent and fatal of diseases, phthisis pulmonalis; and in a single instance, he has reduced his hypothesis to practice, by sanctioning the operation upon a patient in this town.

It will be, perhaps, rather difficult to reconcile with the above, the results of the following experiments: indeed had I not been indulged with the presence of medical gentlemen at several of them, I should have felt some scruple in giving them publicity. All the subjects of my experiments were dogs. The first four noticed had their os humeri secured to the rings of 56 pound weights; their hind legs were well extended, and fastened to similar weights; so that they were retained nearly in their natural standing position, which preserved to them, in a great measure, the use of their auxiliary respiratory powers.

*Exper. 1.*—An opening to the full extent of an inch was made into the left cavity of the chest, between the sixth and seventh ribs, midspace, betwixt the spine and sternum. At each inspiration, the lung was partially inflated, and was distinctly seen beneath the orifice, and at a little distance from it. At each expiration, it evidently contracted, and its lower thin margin was thrust outward through the aperture, with great force and a peculiar noise, caused by the protrusion, and an exit of a portion of the air. The breathing was distressed when the lung protruded, but as soon as the wound was allowed to contract, it became more tranquil. The right cavity was in like manner opened, and the same phenomena were observed. When both apertures were kept wide open, the breathing was very laborious

and distressing, which did not arise altogether from the presence of air in the cavities, but principally from the violent protrusion of the inferior margins of the lungs, because the instant the openings were permitted to contract, so as to prevent that accident, respiration became more easy. About ten minutes after, the second opening was made, before the lips of the wounds were brought together. The dog, when let loose, walked, was weak, but breathed easy; would taste nothing until the next morning, when he appeared lively, took his food, and regained his strength daily. The ninth day after the operation, he was hanged, and the trachea tied, to prevent the collapse of the lungs, on opening the chest. Dissection: no morbid appearance in the cavities of the chest; the wounds were healed internally, but not quite externally. The lungs did not reach by two inches the lowermost part of their cavities, nor did their bulk appear equal to the filling of the other parts of their cavities.

*Exper. 2.*—Dr. Traill, Mr. Christian, and Mr. Dawson, surgeons, were present. The operation was performed in the same manner as the first; the openings were better than an inch in length: both lungs wounded in the operation in consequence of the dog struggling (his hind legs not being sufficiently extended). The lungs did not protrude, but the wounded part of the one that was most seriously injured by the knife was observed to present itself at each expiration opposite the aperture. Will a wounded lung protrude under every circumstance that a sound one will? The above medical gentlemen were convinced that the lungs only partially contracted, and that they were not in a quiescent state, but that respiration more or less oppressed, according as the openings were expanded, continued uninterruptedly during the experiment. A silver catheter introduced into the chest was moved about by the alternate contraction and distension of the lung. The dog was detained with both apertures wide open for the space of five minutes; then untied, without bringing the edges of the wounds together. He was rather lively, shook his tail, drew himself upon his belly along the floor, then lay down on one side, and contracted himself, as if to close the openings; whether it was instinct or accident that induced him to choose that position, it certainly had that effect, thereby enabling him to breathe easier; though his breathing, after he was let loose, did not appear much distressed, his strength was considerably exhausted. About an hour after, the edges of the wounds were brought together; during the intermediate hour, the external air communicated with the cavities. He refused his food until next day, when he recovered his appetite, and regained his strength apace. The fifth day after the operation, the dog was hanged. Dissection: the internal edges of the wounds healed; the left lung had a deep plum-coloured circular spot, as large as the circumference of a crown piece,

surrounding the wounded part, which was quite healed. On making an incision into the discoloured portion, it was found purged with blood, without any apparent disorganization, and appeared in a fair way of regaining its natural state. A similar spot surrounding the wounded part was observed on the right lung, but smaller; that wound was also healed, no adhesion or extravasation in either cavity. Absorption must have been very rapid, as a considerable effusion of blood into the left cavity had taken place during the operation.

*Exper. 3.*—In the presence of Drs. Jeffreys, Jardine, Nicholson, and Messrs. Blackburr and Jones. An opening of an inch and a half in length was made between the sixth and seventh ribs on each side of the thorax, of a middle-sized cur. The air had free ingress during inspiration; and at each expiration, it rushed out, the lungs frequently protruding, when not prevented by the examinations of the above medical gentlemen. The breathing was much distressed; the animal was kept on the table for ten minutes, after both the openings were made; when he was taken down, he walked about, apparently but little affected, the air passing in and out of the cavities. At the expiration of ten minutes from the time he was liberated, the apertures were fully closed, and retained so by applying and pressing a finger at the extremity of each of them, which effectually prevented the ribs from approaching each other; in fact, in my opinion, they were fixed. The death of the animal took place in less than two minutes. To what extent the auxiliary respiratory organs were impeded in their action by the two ribs on each side being separated asunder, I cannot say; but if they were impeded, and I think it is evident that they were, the death of the animal naturally must have been accelerated.

*Exper. 4.*—In the presence of the same gentlemen as witnessed the latter. A small opening was made into each cavity of the chest of a bull-dog in the intercostal spaces, and a full-sized clyster-pipe introduced into both apertures, which were retained for half an hour in an oblique position, their internal ends pointing towards the upper part of the thorax; during which time air freely passed inwards and outwards. The animal was occasionally distressed in his breathing, when he would struggle and contract so as to force out as much as possible the air in the bags of the pleuro; then he would make a quick and full inspiration, by which he was considerably relieved. After regaining his liberty, he seemed but little injured by the operations.

*Exper. 5.*—An opening five inches in length was made between the sternum and symphysis pubis into the cavity of the abdomen. The diaphragm appeared tense, and the action of its fibres were visible, but confined, from the very great pressure required to prevent the protrusion of the intestines, &c. The

Violent thrusting outward of the contents of the abdomen was principally, if not entirely, occasioned by the action of the diaphragm, for the force of the protrusion diminished as soon as its action was obstructed on one side of the mediastinum, and ceased when it took place on both sides. With a little difficulty an opening about an inch in length was made into the left cavity of the chest through the muscular part of the diaphragm, three inches below its attachment to the ribs. When the air rushed in, the diaphragm on that side became relaxed, and its action not only obstructed, but its irritability to all appearance suspended. A portion of the diaphragm jutted out towards the abdomen, and formed a pouch, with the aperture in its centre. A similar opening was made into the right cavity of the chest, followed by the same effects. An ivory tube,  $\frac{3}{10}$ ths of an inch in diameter, was repeatedly introduced into each aperture. The diaphragm seemed to be altogether guided by the action of the contiguous parts, and by the pressure and passage of the air modified by respiration; at each inspiration it was drawn towards the chest, and expanded by the dilatation of the thorax, but its expansion was not sufficient to obliterate the pouches before noticed, which was a strong proof, together with the quiescent state, compared with the previous violent thrusting outwards of the contents of the abdomen, of its paralyzed condition. Is it not likely that in many wounds penetrating into the cavity of the abdomen, unaccompanied with thrusting outwards of its contents, that the diaphragm is wounded? At each expiration, the diaphragm was pushed towards the abdomen, by the pressure and passage of the air from within the cavities of the chest, and the pouches enlarged. The breathing was oppressed, but not so distressing as in the first three experiments, when the apertures were wide open. The external wound was brought together by suture. When the dog was removed from the table, he walked, but tottered a little; he breathed easy, and three hours after lapped some milk, which was rather surprising, as the first two operated upon would accept of nothing until the following morning. He recovered his strength and liveliness amazingly fast. On the third day after the experiment, he was hanged, together with another dog not operated upon, for the purpose of comparing the appearances of their respective lungs. Each had his trachea secured immediately on being cut down, before the suspending cord was slackened, to prevent collapse of the lungs on opening the cavity of the chest. Dissection: in the presence of Dr. Traill, to whom I am greatly indebted for many suggestions in conducting these experiments. No morbid appearance in the abdomen, with the exception of a few adhesions between the liver and peritoneum; the apertures in the diaphragm were closed by a tender film, which very readily gave way to the probe. On opening the chest, the heart was once observed to contract, and the pericardium was perfectly trans-

parent, as remarked by Prof. Richerand.\* We could perceive very distinctly the ramifications of the coronary vessels; the diaphragm was in its natural position; no adhesions; the lungs on first exposing them appeared perfectly smooth and glossy; in a little time they were slightly corrugated by the effect of the cold in condensing the enclosed air: the examination was conducted in the open air. Not the slightest difference could be perceived either in the expansion or general appearance of the lungs of the two dogs. Their lungs did not extend by two inches to the inferior part of their respective cavities, and their bulk was insufficient to fill the remaining part of the bags of the pleura. Pray what proof is there that the lungs fill the bags of the pleura? I must confess that I am quite sceptical upon that point, and, on the contrary, believe that in a healthy state they never fill them. To prove that the measure we had recourse to was effectual in preventing the air escaping out of the lungs, when the chest was opened at the conclusion, we divided the windpipe of each below the knots, when they instantaneously collapsed.

*Exper. 6.*—The description supposes the animal to be standing upright on his hind legs. The right cavity of the chest was transixed with a sharp-pointed penknife, by thrusting it transversely with a slight degree of obliquity downwards, through the intercostal space, immediately above the upper edge of the eleventh rib, mid-space, between its head and anterior extremity. The animal was afterwards put to death. Dissection: the instrument had pierced the diaphragm, and had slightly wounded the liver; the lung uninjured; the knife had passed as near as possible through the centre of the cavity at that part, and but little short of two inches above its inferior termination. Had the lung extended so low down, it must have been wounded, the knife being sharp-pointed, and pushed in with force.

*Exper. 7.*—To remove every doubt, with respect to the state of the lung, when deprived entirely of the influence of the auxiliary respiratory organs, the following experiment was made in the open air. The cartilages of all the true ribs were divided, with the exception of the superior one, close to their juncture, with the osseous structure, and the left cavity of the chest laid open its whole length; the diaphragm was punctured; the ribs, and their cartilaginous ends, attached to the sternum were separated and retained asunder, so as to expose and to deprive the lung of every assistance from the auxiliary respiratory organs. The lung rested upon the ribs and side of the vertebrae; on exposure, it shrunk considerably, but did not collapse. Its motion, for it was not at rest, might be compared to that of a leech when it draws up, and again recedes its body, without making any progression; indeed, with both ends fixed, the motion was not

that of pulsation, neither was it synchronous with the action of the heart, but it was a slow undulating movement. On closing the gash, and keeping it so for four or five inspirations, then opening it quickly, the lung was found to have increased in volume; on exposure, it again diminished. To close the scene, the knife was plunged into the heart with a determination never to perform another experiment on a living animal, which I had been induced in the present case to do, by my anxiety to solve an important practical problem.

I believe no one has ever appreciated the power of the lungs and their auxiliaries in respiration during life in resisting and diminishing the pressure of the atmosphere, when admitted into the cavities of the thorax through apertures in its parietes. That they do possess a considerable power is beyond a doubt. The oppression of the breathing and the expansion of the lungs are in an inverse ratio to one another, and can be regulated by adjusting the dimensions of the openings into the cavities of the chest. As the apertures increase in their size, the power of expansion of the lungs is diminished, while the oppression of the breathing is augmented, until at last life is extinguished. What quantity of air is sufficient to produce death as long as all the respiratory organs are not restrained, I have not ascertained. The doctrine of the lung collapsing, while the function of the opposite one is unimpaired, on exposure of its external surface to the atmosphere, taught from time immemorial in the schools, I must now consider as erroneous, and feel somewhat surprised that a notion so groundless should have existed for so many ages.

From the foregoing experiments, it appears,

1. That a lung will not collapse from exposure to the atmosphere as long as respiration is carried on by the opposite one, and the auxiliary respiratory powers are not restrained.
2. That a lung possesses for a time, independently of the influence of the diaphragm and intercostal muscles, if respiration is carried on by the opposite lung, a peculiar motive power, the source of which I do not pretend to explain.
3. That a sound lung soon regains its full power of expansion, when the pressure of the exterior air is removed.
4. That air freely and uninterruptedly admitted into both cavities of the chest simultaneously, through tubes of a certain calibre, will not collapse the lungs, if the auxiliary respiratory organs are unrestrained.
5. That air admitted into both the cavities of the chest (of a middle-sized dog) simultaneously through apertures of an inch and better in length in the intercostal spaces, will not collapse the lungs, provided the animal is allowed unconfined the use of his respiratory organs.
6. That a sound lung never fills the bag of the pleura.

If the last physiological inference is correct, it is highly interesting in a pathological point of view. It enables us to explain how hydrops thoracis, or that species of it called hydrops pleurae, may exist to a certain extent, without being attended with any symptoms indicating the presence of the disease, as related by numerous medical authors. We can also more satisfactorily account how the lung so frequently escapes being wounded when weapons penetrate the cavity of the thorax; and how the extravasation which follows, if not considerable, produces but little derangement. It may also have a practical utility, for it informs the surgeon that the lung descends to a certain point only, so that he need not be afraid wounding it should an operation be required below that position.

Dr. Carson retained open the apertures into the chests of his animals with his fingers; whereas mine were kept open in the manner described, which accounts for the different results of our experiments. Dr. Carson, in his *Essays*, does not allude to this circumstance, but since my investigations he has mentioned it. When air is admitted into the cavities of the chest, the animal requires the aid of all his respiratory powers.

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## ARTICLE VII.

*On M. Schweigger's Electromagnetic Multiplier, with an Account of some Experiments made with it.* By Prof. Oersted. (Communicated by the Author.)

IMMEDIATELY after the discovery of electromagnetism, Prof. Schweigger, of Halle, invented an extremely useful instrument for the purpose of discovering very weak electrical currents by means of the magnetic needle. The effect of this multiplier is founded upon the equal action which every part of a conducting wire when it transmits a current exerts upon the magnetic needle. When a part of this wire is curved as in A B C, fig. 1 (Pl. XX), so that the two branches A B and B C are in a vertical plane, and a magnetic needle D E is properly suspended in the same plane, it will be readily conceived that the needle receives an impulse double that which it would receive from one only of the branches. The impulse given by each branch has also the same direction, since it is in fact the same side of the wire which in both branches is opposite the needle. The effect is still further increased when the conducting wire makes several circumvolutions round the needle, as in fig. 2, and thus an elec-

Fig. 1.

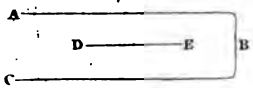


Fig. 2.

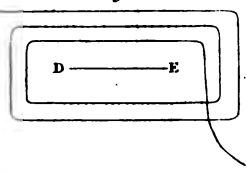


Fig. 3.

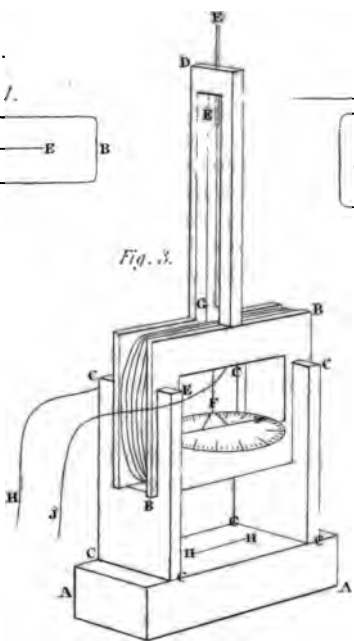


Fig. 4.

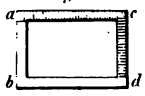


Fig. 5.

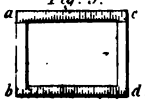


Fig. 8.

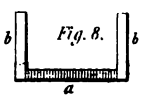


Fig. 6.

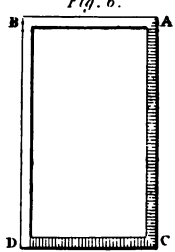


Fig. 7.

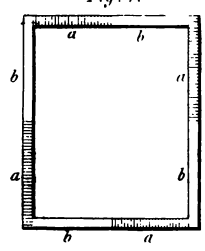


Fig. 10. p. 457.

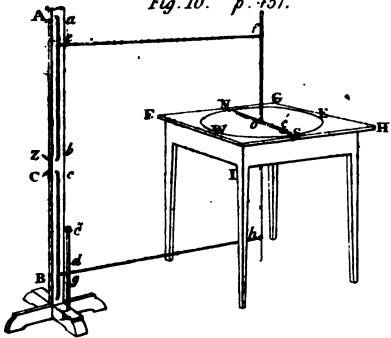
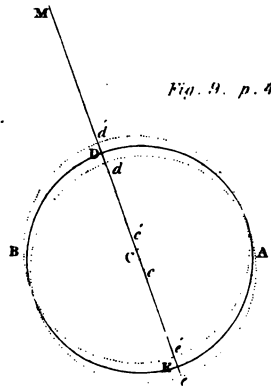


Fig. 9. p. 453.



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tromagnetic multiplier is formed; fig. 3 represents an apparatus according to my construction, which differs, however, from M. Schweigger's in no essential respects. A A, fig. 3, is the foot of the apparatus. C C, C C, are two stands which support a frame B B, which has a groove on the edge to receive the multiplying wire. D D is a stand to support the wire from which the magnetic needle is suspended. E E is a metallic wire passed tightly through a hole made in the upper part of the stand D D. To this metallic wire there is attached by a little wax a thread of raw silk E F, suspending a double triangular loop of paper, in which the magnetic needle is placed. E G is a tube which allows the suspension wire a free passage, and prevents the multiplying wire from touching it. Below the magnetic needle a divided circle is placed to measure the deviations. The multiplying wire is of plated copper, and a quarter of a millimetre, or about  $\frac{1}{16}$  of an inch in diameter. It is covered with silk thread, which prevents any communication between the different parts of the multiplying wire; H and J are the two ends of this wire. The use of this apparatus will be understood almost without any explanation. In order to multiply the effect produced by a galvanic arrangement upon the needle, it is requisite only to effect a communication so as to make the multiplying wire a part of the circuit. The effect of a disk of copper and of zinc with pure water as a liquid conductor, was rendered perfectly sensible by this apparatus, and it is even possible by its means to render those galvanic actions sensible, which are too weak to produce a marked effect upon the prepared muscles of a frog. When it is required to discover an action which is so extremely weak as to occasion a scarcely visible deviation, the circuit is interrupted immediately after it has been completed, but it is again effected at each time that the needle is at the point of terminating the preceding oscillation; the apparatus may be rendered still more sensible by putting a small magnetic needle in H H in the situation required to diminish the force with which the suspended needle tends to preserve its direction.

When the multiplier is employed for moderately strong electromagnetic action, thicker conducting wires must be used. If this precaution be neglected, the effect may be diminished instead of increased, owing to the imperfection of the conductor. M. Seebeck has made some very satisfactory researches on this subject, in his memoir on electromagnetism, published two years since in the memoirs of the Berlin Academy.

M. Poggendorf, of Berlin, a distinguished young philosopher, constructed an electromagnetic multiplier very soon after M. Schweigger, and made some striking experiments with it. The experiments of M. Poggendorf having been cited in a work upon electromagnetic phenomena by the celebrated M. Erman, published soon after the discovery of these phenomena, were

known to several philosophers before those of M. Schweigger, which circumstance has given rise to different names for the same apparatus. M. Poggendorf has made a very useful application of this apparatus by employing it for the purpose of examining the order of the conductors in the galvanic series. An account of his labours, is contained in the German Journal, the *Isis*, for the year 1821. M. Avogadro, in Italy, has used the same plan, but without experimenting on so great a number of different bodies, his memoir contains some other observations which are worthy of being known. By the indications of the electromagnetic multiplier, he discovered that some metals at the first moment of their immersion in concentrated nitric acid, produce an effect contrary to that which is observed in a few seconds afterwards; but this alteration does not occur in dilute nitric acid. The metals which have exhibited this property are lead and bismuth, lead and tin, iron and bismuth, cobalt and antimony. M. Avogadro states, that the first effect which occurs in a concentrated acid is similar to that which happens in a diluted acid, and that it is afterwards that the contrary effect is perceived. I have repeated these experiments with lead and bismuth, and I have confirmed them by other means, excepting only that I have always had at the end of the experiment with concentrated acid, the same effect as that constantly produced by the dilute acid. I have also found that the bars of lead and of bismuth which have been acted upon by concentrated acid, gave in repeated experiments constantly the same effects as by dilute acid, unless fresh surfaces were given to them before they were again immersed in the acid; this renewal of the surfaces may be effected not only by mechanical means, but also by diluted nitric acid. It also frequently happened that the bars which had been in diluted acid, and which had been only slightly wiped, gave at first in the concentrated acid a momentary deviation in the same direction as in the diluted acid, very probably on account of the fluid which remained on their surface; they then gave for some seconds the contrary deviation; that is to say, the same as that observed when the experiment is made with bars well cleaned. At length the deviation became such as it would have been, if diluted acid had been employed as a fluid conductor. It is to be remarked that concentrated nitric acid acts much more strongly upon bismuth than upon lead; and, on the contrary, that the diluted acid acts strongly upon the lead, and scarcely at all upon the bismuth. Thus it follows, that the lead acts as the more positive metal in the dilute acid, but as the negative in the concentrated acid.

It remains only to explain why the deviation produced by the concentrated acid does not continue the same during the whole of the experiment. As I am travelling, I have not time to treat of this, or the analogous experiments related by M. Avogadro thoroughly, but I shall content myself with having contributed

to call the attention of philosophers to this class of experiments which are equally interesting as regards the theory of solution and that of the excitation of the electric current. M. Avogadro mentions also that arsenic acts with respect to antimony as a positive metal in concentrated nitric acid and as a negative in dilute acid. This phenomenon appears interesting in relation to the chemical effect of this acid upon the two metals in its different degrees of concentration.

Among the experiments to which the electromagnetic multiplier gives rise, it may be stated that by its use, we may show, that when two pieces of the same metal are immersed in an acid capable of acting upon them, that which is first immersed acts towards the other as the most positive metal; this experiment is extremely well performed with two bars of zinc and diluted sulphuric or muriatic acid. It would be extremely interesting to examine the electromagnetic changes which take place during every period of the action of acids and alkalies upon the metals, and nothing affords greater facility for this purpose than the electromagnetic multiplier.

—

*Notice read at the Academy of Sciences of some new Thermoelectric Experiments made by M. Le Baron Fourier and M. Oersted.*

I have had the honour of exhibiting to this illustrious Academy the remarkable experiments by which M. Seebeck has shown that an electrical current may be produced in a circuit formed of solid conductors only by disturbing the equilibrium of the caloric. We are therefore in possession of a new kind of electric circuits, which may be called thermoelectric circuits, thus distinguishing them from galvanic circuits, which may in future be denominated hydroelectric. On this subject an interesting question arises respecting electromagnetism, and which relates also to the theory of the motion of heat in solid bodies; the question is to examine whether the thermoelectric effects may be increased by the alternate repetition of bars of different matters, and how it will be necessary to proceed to obtain the sum of these effects. It does not appear that the author of the discovery of the thermoelectric circuit has as yet directed his researches to this point. But M. Le Baron Fourier and I agreed to examine this question together experimentally.

The apparatus with which we performed our first experiments is formed of three bars of bismuth, and three other of antimony, alternately soldered together; so that they form an hexagon, and thus constitute a complex thermoelectric circuit, consisting of three elements. The bars are about 4.7 inches long, 0.6 of an inch wide, and nearly 0.16 of an inch thick. We placed this circuit upon two supports, in an horizontal position, taking care to give to one of the sides of the hexagon the direction of the magnetic needle, and we placed a compass as nearly as

possible to and beneath this side. By heating one of the soldered parts with a taper, we produced a very sensible effect upon the needle. By heating two soldered places which were not contiguous, the deviation was considerably increased; lastly, when the temperature was raised at the three alternating solderings, a still greater effect was produced. We also employed an inverse process, reducing by means of melting ice the temperature of one or more of the solderings of the circuit to the freezing point. It will be readily conceived that in this process the solderings which are not cooled are to be considered as heated with respect to those that are. This manner of making the experiment admits of ascertaining by different processes the requisite comparisons for discovering the laws of the power investigated.

Employing the action of ice and that of flame at the same time; that is to say, by heating the three solderings which were not cooled, we produced a very considerable effect; the deviation amounted to 60 degrees.

We afterwards continued these experiments with a stronger apparatus, composed of 22 bars of bismuth and 22 of antimony, much thicker than those of the hexagon, and we satisfied ourselves that each element contributes towards the total effect. In order to make some other experiments, we interrupted the circuit in one place, and soldered at the extremities of the separated bars, small brass cups which we filled with mercury, in order to have a ready mode of forming a perfect communication between these two points by means of metallic wires. A copper wire nearly four inches long, and 0.04 thick, was nearly sufficient to establish an entire communication; and two similar wires, one by the side of the other, effected a most perfect communication; a similar wire, about 40 inches in length, also effected a pretty good communication, but a platina wire, 0.01968 of an inch in diameter, and nearly 16 inches in length, occasioned so imperfect a communication that the variation amounted to only one degree. A slip of paper moistened with a saturated solution of soda, completely interrupted the communication. There was no chemical action; nor did we observe any sensible ignition as might have been expected in an apparatus capable of producing so great an electromagnetic effect. We may also add, that the sum of the effect of all the elements of the complex electromagnetic circuit, is much less than the sum of the isolated effects, which may be produced by employing the same elements to form simple circuits.

I shall now give a detailed account of the experiments referred to in the above communication, accompanied with some further observations.

The bars which were employed in the following experiments were parallelepipeds, the transverse section of which was square, each side being nearly 0.6 of an inch in length.

*Exper. 1.*—We formed a rectangular circuit:  $a b c d$ , fig. 4, one-half of which was antimony, and the other of bismuth;  $a c d$  and  $a b d$  soldered together, so that the two contiguous were of antimony, and the other two of bismuth. One of the sides was nearly four and a half inches long, and the other three inches; the circuit was placed horizontally upon stands, with two of its sides in the direction of the magnetic needle, and the compass was placed upon one of them. Having left the circuit for a time sufficient to regain the equilibrium of temperature, which might have been disturbed during the placing of it, ice was put upon one of the two solderings,  $a$  or  $d$ , which unite the two heterogeneous metals. The compass showed a deviation of 22 or 23 degrees; the temperature of the air was  $57^{\circ}$  of Fahr.; at a temperature of  $68^{\circ}$ , the deviation was observed to be 30 degrees. We neglected to note the temperature of the atmosphere at the commencement of the experiment. We shall, therefore, only compare the results of experiments made at the same period.

*Exper. 2.*—Another circuit, fig. 5, was formed of the same length as the former; but having the opposite sides of the same metal,  $a b$  and  $c d$  being bismuth, and  $a c$  and  $b d$  of antimony so that the circuit was composed of two thermoelectric elements rendered active by ice placed upon two opposite angles. This circuit produced a deviation of 30 to 31 degrees, under the same circumstances in which the simple circuit produced a deviation of 22 to 23 degrees. The temperature in this circuit has its equilibrium soon restored, so that the thermoelectric effect appears weaker than it would do without this circumstance.

*Exper. 3.*—A circuit  $A B C D$ , fig. 6, the circumference of which was double that used in the first experiment, was put in action by ice placed upon one of the solderings. The deviation was only from  $13^{\circ}$  to  $15^{\circ}$ , under the same circumstances which, with the circuit, fig. 4, gave 22 or 23 degrees.

*Exper. 4.*—Another circuit, fig. 7, was formed, of the same length as the preceding, but it had four alternations, or four thermoelectric elements  $a b$  ( $a$  being the antimony, and  $b$  the bismuth). This circuit was put in action by placing ice upon every other soldering. The deviation of the needle amounted to  $31\frac{1}{2}$  degrees, under the same circumstances in which the simple circuit of equal length produced a deviation of only 13 to 15 degrees, but the circuit used in *Exper. 2*, fig. 5, which had only half its circumference, and half the number of elements, produced nearly the same effect as that obtained in this experiment. Thus it appears, which will be confirmed in the sequel, that the deviations of the needle produced by the thermoelectric circuit increase with the number of the elements when the length of the circuit remains the same, but that they become weaker in proportion as the length is increased. It is also evident, and it will be rendered still more so in the sequel, that these two effects balance each other; so that the effect of a circuit is not

altered, when the length of the circumference increases in the same proportion as the number of the elements; or, in other words, elements of equal length form circuits which produce equal deviations, whatever may be the number of the elements. We confirmed these results by comparing the effects of two, three, four, six, thirteen, and twenty-two elements.

In order to form complex circuits capable of producing a great effect upon the magnetic needle, it will be necessary to employ very short elementary bars; and to avoid the inconvenience which follows from the restoration of the equilibrium of temperature which happens too rapidly in such small circuits, the solderings must be placed alternately in contact with continued sources of heat and cold. There is still another increase of effect in the complex thermoelectric circuit, which is not thus limited by the length of the circumference; but before it is mentioned we shall show the relation which exists between the different elements of the complex circuit.

*Exper. 5.*—We examined the effects of the circuits by cooling first one, then two, afterwards three, &c. of the solderings which were rendered active; and after several experiments, we found the mean numbers to be as follow: In a circuit of three elements, the first gave a deviation of  $15\frac{1}{2}^{\circ}$ ; the first two  $25\frac{1}{2}^{\circ}$ ; the three together  $31^{\circ}$ . In a circuit of four elements, the one placed upon one soldering gave a deviation of  $13\frac{1}{2}^{\circ}$ ; upon two  $19^{\circ}$ ; three  $25^{\circ}$ ; four  $31\frac{1}{2}^{\circ}$ . In a circuit of six elements, the first gave a deviation of  $9^{\circ}$ ; the first two  $13\frac{1}{2}^{\circ}$ ; the first three  $18\frac{1}{2}^{\circ}$ ; the first four  $22^{\circ}$ ; the first five  $25\frac{1}{2}^{\circ}$ ; the six together  $28\frac{1}{2}^{\circ}$ .

It will be observed that the deviation produced by the first cooled soldering, is nearly represented by double the quotient obtained by dividing the total deviations produced by the circuit, when all its elements are put in activity, by the number of elements plus one. It is also evident that the other numbers nearly approach the value of the simple quotient; but still they appear to form a decreasing series. We are now alluding to the deviations measured by the angles, and not of the real extent of the effects. If it were not necessary to regard the different distances of all the points which act upon each other in the different positions of the needle, and even to consider what may be the reciprocal situation more or less oblique of the edges of the conductor and of the needle, the effects might be represented by the tangents of the deviations. It is, however, remarkable, that the experiments which we have made indicate a constant relation between the deviations. If such experiments as we have hitherto had an opportunity of performing were susceptible of greater exactness, consequences interesting to the theory would undoubtedly arise from them.

*Exper. 6.*—Thermoelectric action may be rendered sensible by means of the electromagnetic multiplier. In order to produce

this effect; one of the two pieces of metal, *a*, fig. 8, is combined with two pieces of *b*, the other, so that this arrangement constitutes a broken circuit, the two ends of which are of the same metal. After having put some ice upon one of the solderings, a communication is established between the two pieces *b* by means of the multiplying wire.

The effect of this is sensible upon the needle of the instrument, but yet it is very weak; weaker, for example, than the effect of a piece of copper and silver with water as a fluid conductor. The effect is rendered more evident by communicating a fresh impulse to the needle, at the end of each return after a former impulse.

The extraordinary weakness of this action is very remarkable. We learn from this result that the same thermoelectric elements which produce a great effect upon the magnetic needle of the compass, when their communication is made by a short and thick conductor, act but very little even upon a much more sensible needle, when the communication is made by a thin conductor of considerable length. A hydroelectric current excited by a piece of zinc and silver, with water as a fluid conductor, produces an effect upon the needle perhaps a hundred times greater than that of the thermoelectric current; nevertheless the effect produced by the former upon the needle of the compass, even when the communication is made between the elements by the best conductors is scarcely sensible; while the effect of the latter upon the compass is not only sensible but considerable. All this marks a very important property of the thermoelectric current, which indeed might have been foreseen by theory, but which experience should confirm; that is to say, the thermoelectric circuit contains the electric powers in much greater quantity than the hydroelectric circuit of equal size; but, on the other hand, the intensity of force in the former is much weaker than in the latter. Since the first electromagnetic experiments, it has been clearly seen that the deviation of the needle produced by the electrical current would be regulated according to the quantity of electric power, and not by its intensity. Thus the considerable deviation which the thermoelectric current produces is an indication of the great quantity of power which it contains. As to the intensity; it is universally acknowledged, that an electric current pervades conductors so much the more readily as it is more intense: the hydroelectric current which more easily pervades the wire of the multiplier than the thermoelectric current does, must, therefore, be more intense. The much greater quantity of electric power which must be admitted to exist in the thermoelectric current, will form no objection to this reasoning; for it is perfectly evident that in the case in which a current *A*, of intensity equal to that of another current *B*, but greater in quantity, is presented to a conductor sufficient to transmit the quantity of *B* only; this

conductor must be capable of transmitting a part of the current A equal to the current B; and if we suppose A to possess a stronger intensity than B, the transmission of the former will be still greater.

*Exper. 7.*—We tried the effect of the complex circuit upon the needle of the multiplier, and we found it considerably augmented, by increasing the number of the elements of the circuit, even in cases in which the number did not increase the effect upon the compass. We obtained this result by experiments with 6, 13, and 22 elements. It appears then that the intensity of the power increases in the circuit with the number of the elements, which is perfectly conformable to what happens in Volta's pile. The circuit had no sensible effect upon the compass when the communication was made by the multiplying wire.

*Exper. 8.*—A platina wire, about 0.004 of an inch in diameter, was not ignited by a thermoelectric circuit composed of 13 elements, but which was nevertheless capable of causing the compass to deviate 28 degrees; yet a hydroelectric circuit producing an equal effect upon the compass, was quite sufficient to ignite the same wire. This difference is derived from the too weak transmission of the thermoelectric current by the platina wire. During the communication effected by this wire, the needle of the compass indicated only 2 or 3 degrees of deviation. An iron wire, about 0.008 of an inch in diameter, was not ignited. The communication effected by this wire produced a greater deviation than the platina wire, but only by 5 degrees. We must wait for the current produced by a thermoelectric apparatus composed of several hundred elements, before we shall be able to ignite a metallic wire.

*Exper. 9.*—We were unable to produce any sensible chemical action by the thermoelectric circuit; those fluids which have the greatest conducting power resisted its action; for instance, nitric acid, solution of soda, and many metallic solutions. We shall mention only one of these experiments, which, frequently repeated, appeared to produce some chemical effect. We placed a piece of blotting paper moistened with solution of sulphate of copper between two perfectly new five franc pieces; the precaution was taken to put the two pieces in contact with the paper on the sides which had similar impressions, and the thermoelectric current was passed through the two pieces of metal and the moistened paper. In a quarter of an hour some parts of the silver were slightly covered with copper. But as this trace of metallic precipitation did not resist washing accompanied with slight friction, we are disposed to consider this experiment as too questionable. During the time that the two pieces of silver with the paper formed part of the circuit, not the slightest effect was produced upon the compass, so that this small piece of moistened paper may be said to have

entirely interrupted the thermoelectric current. In a state of such perfect isolation, no sensible chemical effect could be expected. From the slight intensity indicated by the multiplier, there is reason to think that it would require an electric circuit of many hundred elements to pervade a fluid equally well as a Volta's pile formed of four or five elements; but it is very probable that such an apparatus will produce effects similar to those which may be expected from hydroelectric piles, the metallic elements of which are enormously large.

*Exper. 10.*—The action of electrical currents upon animal bodies is one of the most remarkable which it exerts. The thermoelectric circuit excited no sensible taste, when it was made to act upon the tongue; but upon a prepared frog, it produced effects of two slightly different metals; this result evinced that the nerves of a frog are excellent conductors.

*Exper. 11.*—A thermoelectric circuit of 13 elements produced no effect upon the most delicate electrometers; nor did Volta's condenser unequivocally indicate signs of electricity in this circuit. But we acknowledge that we did not repeat this experiment so often as it deserves.

*Exper. 12.*—The experiments which we have related are sufficient to prove how weak the thermoelectric current is with relation to the conducting power even of the best conductors. Another experiment produced similar results under other forms. The great circuit consisting of a rectangle, the length of which was nearly four times its width, was placed in such a manner that the two short sides were parallel to the needle of the compass; the compass was placed on one of these sides, and the two adjacent elements were rendered active. After having observed the deviation of the needle, a communication was effected between the active parts furthest from the compass by means of a copper wire, so that all the active parts might form a separate circuit. After this diminution of the circumference of the circuit, the needle indicated a stronger action; this effect would not have been very evident, if the transmission of the thermoelectric current were not so difficult even in the metal, that a difference of passage of two or three feet could produce so considerable a change in the effect. It must be observed that the same copper wire employed to effect the communication, when some part of the whole circuit was interrupted, would produce scarcely the same effect as the immediate junction. When the part of the circuit furthest from the compass was rendered active, and a similar communication was effected, the deviation of the needle diminished. However, this difficulty of transmission is unattended with any thing that ought to occasion surprise. For the electricity in a circuit of conductors, in consequence of their contact, must flow in proportion as it acquires the intensity requisite to clear the passage in these conductors; therefore this electricity never acquires sufficient intensity to

pervade the conductor with facility, but it will constitute a current as soon as the circuit does not oppose the obstacle of very considerable isolation. It is easy to perceive that the quantity of electricity developed by this continual excitation which exists in the circuit, ought to be so much the greater as the circuit is a more perfect conductor. Thus the thermoelectric circuit supplies an incomparably greater quantity of electricity than any other circuit which has as yet been invented. If by other circuits water, the acids, and the alkalies, have been successively decomposed, it is not beyond the limits of probability, that by means of a new circuit, we shall be able to decompose even the metals; and thus complete that great change in chemistry which commenced with the pile of Volta.

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## ARTICLE VIII.

### *Analysis of the Native Sulphate of Iron and Alumina.*

By R. Phillips, FRS. L. and E. &c.

UNTIL after I had completed the analysis of this substance, I was not aware that it had been noticed in any work on mineralogy: I find, however, that it has been described in the 24th number, p. 97, of Mr. Sowerby's *Exotic Mineralogy*; the specimen mentioned and figured in this work under the name of sulphate of iron and argilla, is stated to be from Bacherstolln, in Schmolnir. Mr. Sowerby mentions that it seems to have been mistaken for native alum, but he justly observes that it differs from alum in containing no alkali, and that the solution yields it upon the addition of potash.

The salt which I subjected to examination originates from the decomposition of iron pyrites in slate-clay. It was presented to me by Charles Macintosh, Esq. and is plentifully met with in the slate clay of the deserted coal mines of Hurlet and Campsie, which as well known is employed for the double purpose of preparing alum and sulphate of iron.

The sulphate of iron and alumina exists in the state of soft delicate fibres, easily separable from each other; it is colourless, and its lustre is silky, and it resembles asbestos in appearance. It is so extremely light that 100 grains of the crystals occupy a space equal to that of an ounce and a quarter of water. By exposure to moist air, the iron is converted into peroxide, and the crystals become yellowish-brown. It is readily soluble in water, and the solution, as above stated, readily yields crystals of alum on the addition of the salts of potash or ammonia. By spontaneous evaporation, crystals of common sulphate of iron are obtained, and the sulphate of alumina remains in solution; this

circumstance renders it probable that the salt in question is not producible artificially; but this I have not tried. Crystals of common sulphate of iron are sometimes mixed with it. When the usual tests of the presence of iron are added to the solution, the common evidence of the existence of that metal is obtained; prussiate of potash gives a very light blue precipitate, showing that the iron is principally in the state of protoxide.

One hundred grains of this double salt, cleared as much as possible from the small fragments of slate-clay, were dissolved in distilled water; the solution was filtered, and four grains of earthy matter remaining undissolved, the deficiency was supplied by an equal quantity of the pure salt. The solution was heated with a little nitric acid to convert the iron into peroxide, and nitrate of barytes was added to it as long as precipitation ensued; the sulphate of barytes washed and dried amounted to 91.25 grains, which are equivalent to 30.9 of sulphuric acid. The slight excess of nitrate of barytes being removed from the solution by means of sulphuric acid, the solution was decomposed by ammonia added in excess, and the peroxide of iron and the alumina were of course precipitated together.

The ammoniacal solution was examined in order to discover whether any minute portion of lime or magnesia was contained in the salt, but none was detected; the precipitate was boiled with a solution of soda to separate and dissolve the alumina, and the peroxide of iron left being washed, dried, and ignited, weighed 23 grains; but the iron exists in the state of protoxide; and as 40 of peroxide consist of 36 protoxide and 4 oxygen, 23 are equivalent to 20.7 of protoxide, which is of course the quantity contained in 100 grains of this double salt.

The alkaline solution which contained the alumina was supersaturated with muriatic acid, and the alumina precipitated by carbonate of soda. When washed and ignited, it weighed 5.2 grains.

I made an attempt to ascertain the quantity of water of crystallization by direct means, but it failed; indeed the nature of the salt is such as to render it scarcely practicable; for it is, I think, more than questionable, whether anhydrous sulphate of iron or of alumina can exist; and, at any rate, there is great danger either of not expelling the whole of the water by heat, or of driving off some of the acid with it.

From the experiments above stated, it appears that this salt consists of

Sulphuric acid. ....	30.9
Protoxide of iron. ....	20.7
Alumina. ....	5.2
Leaving for water. ....	43.3
	<hr/> 100.0

I repeated these experiments with a fresh quantity of the salt, and the results agreed almost precisely with respect to the sulphuric acid and oxide of iron, but there was rather less alumina: the difference was not, however, sufficient to induce me again to repeat the analysis.

I have stated on a former occasion my reasons for believing, that hydrogen = 1, alumina is 27; and this determination is strengthened by the results of Sir H. Davy's experiments stated in p. 357 of his Elements, and to which I have only lately particularly adverted. He says that from experiments which he "made on the quantity of ammonia required to decompose saturated solutions of alumina in acids, it would appear that the number representing alumina is about 48, and supposing it to consist of one proportion of aluminum, and one of oxygen, 33 will be the number representing aluminum." If, however, the atom of oxygen be represented by 8 instead of 15, then the number for alumina will be, according to Sir H. Davy's experiments, 25·6; now this sufficiently approaches 27 to assist in deciding the question whether alumina should be represented by 27, or by 18, which latter number Dr. Thomson considers to be the weight of its atom.

Representing then sulphuric acid by 40, protoxide of iron by 36, alumina by 27, and water by 9, it will appear that the sulphate of iron and alumina in question is composed of

4 atoms of sulphuric acid..	$40 \times 4 =$	160
3 atoms of protoxide of iron	$36 \times 3 =$	108
1 atom of alumina. ....	$=$	27
25 atoms of water. ....	$9 \times 25 =$	225
		<hr/> 520

We may then consider the salt as composed of

3 atoms of sulphate of iron.	$76 \times 3 =$	228
1 atom of sulphate of alumina. ....	$=$	67
25 atoms of water. ....	$=$	225

Weight of the atom. .... 520

On this view of the subject, the theoretic composition of the salt will be as follows, which, it will be observed, agrees very nearly with the analysis:

Sulphuric acid .....	30·76
Protoxide of iron .....	20·76
Alumina: .....	5·19
Water. ....	43·26
Loss .....	0·03
	<hr/> 100·00

Or,

Sulphate of iron .....	43.84
Sulphate of alumina. ....	12.88
Water. ....	43.28
	<hr/> 100.00

## ARTICLE IX.

*On the Crystalline Forms of Artificial Salts.*

By H. J. Brooke, Esq. FRS.

(To the Editor of the *Annals of Philosophy*.)

DEAR SIR,

THE introductory volume to the Science of Crystallography on which I have been for several months engaged, having passed through the press, I propose now to resume an examination of the crystalline forms of the artificial salts, a subject which has been hitherto much neglected, and of which, during the last two years, we have frequently spoken.

As an evidence of the neglect with which the crystallographical characters of the productions of the laboratory have been treated, I may refer to the recent edition of Dr. Henry's Chemistry; and I do this, not to impeach in the slightest degree the value of that work, but merely to remark, that instances of imperfect and useless descriptions of crystalline forms are admitted into volumes otherwise of great worth.

The crystalline characters of the artificial salts will, if strictly attended to, frequently assist the researches of the chemist.

An examination of the forms, and measurements of the angles of the crystalline deposits from his experimental processes, will immediately inform him whether his experiments have produced such results as he had anticipated, or whether his compounds are new and unexpected. For this purpose, however, the reflective goniometer must be added to his other implements, and he will not fail soon to discover its value in reference to his pursuits.

But to be provided with the means of effectually applying this instrument, he must be acquainted with the forms, and the measurements of the angles of all the known crystals of those salts. During the last summer, I measured a considerable number of these, most of which I have to thank you for procuring for me, and for some others I am indebted to the kindness of Mr. Teschemacher. Several I also prepared myself; and I shall still feel obliged to you, or to any of your friends, for measurable crystals of any of these artificial compounds.

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With a view to render the descriptions of these as simple and as practical as possible, it is not my intention to consider them mathematically, and in relation to the theory of decrements. The information the chemist requires to be possessed of concerning the crystals which may be formed during his operations, is the character of their simplest or primary forms;\* their cleavages where they can be given; their modified or secondary forms; the angles at which their planes severally incline to each other; with occasional notices on their predominating characters, and on any peculiar habitudes which may be observed to belong to particular crystals.

Descriptions of several of the artificial salts, founded on these characters, will form the substance of this and some following communications. These will be accompanied by figures which are not drawn with geometrical truth, and are intended merely as diagrams to which the measurements of the crystals may be more conveniently referred, and which will, at the same time, convey a general idea of the form of the substance described.

#### *Crystals deposited from the Oil of Cubebs.*

Of the chemical nature of the substance of these crystals, which I received from Mr. Teschemacher, I know nothing.

The predominating form of the crystals is that of an octahedron with a rhombic base, as shown in fig. 1, measuring as follows :

P on P' about . . . . .	115° 45'
P over plane v . . . . .	74 56
P on P'' . . . . .	145 40
n on o . . . . .	165 0
n on v . . . . .	151 0
n on a . . . . .	90 0

Fig. 1.



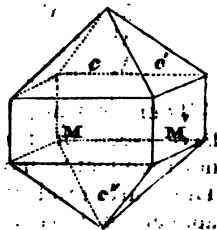
#### *Arsenate of Potash.*

The primary form of this substance may be regarded as a right square prism.

In the crystals I have measured, the terminal edges of the prism are replaced, as seen in fig. 2, measuring as follows :

M on c } . . . . .	133° 15'
M' on c' }	
c on c' . . . . .	122 2

Fig. 2.



The predominating form of most of the

\* The nomenclature of forms, and the letters placed on the crystals, are those which are used in the introductory volume already alluded to.

crystals I have seen is exhibited in fig. 3, where the plane  $M'$  is so much less than  $M$  as to confer on the crystal an appearance of the base being rectangular, but not square; thus affording one of the numerous instances which will be found among crystals, of deviation from regularity and symmetry in their natural forms, by a disproportionate extension of some of their planes; a character which would frequently lead to an inaccurate determination of their forms, if the goniometer were not resorted to. But the goniometer will generally correct the erroneous conclusion derived from the appearance of the crystal; as it has done in this instance, by showing that  $M$  on  $c$ , and  $M'$  on  $c'$ , measure alike, which it is highly improbable they should do if the base of the prism were not square.

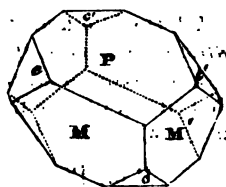
Fig. 3.



#### Chlorate of Potash.

The primary form is an oblique rhombic prism, some of the crystals being modified as in fig. 4. The cleavage is easy parallel to the planes  $M$  and  $M'$ , and the cleavage planes are brilliant, but the only crystals I have are too thin to obtain a cleavage plane parallel to  $P$ . The measurements are as follows:

Fig. 4.

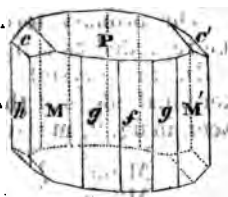


P on M, or $M'$ . . .	$105^{\circ} 30'$
M on $M'$ . . . . .	104 0
P on $c'$ . . . . .	106 45 dull
P on $c$ , or $c'$ . . . .	129 45

#### Tartrate of Potash and Soda.—Rochelle Salt.

The form derived from cleavage is a right rhombic prism. This is modified in the crystals measured, as shown in fig. 5.

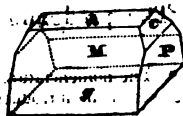
Fig. 5.



P on M, or $M'$ . . . .	$90^{\circ} 0'$
P on $c^*$ . . . . .	138 50
M on $M'$ . . . . .	100 0
M on $g$ } . . . . .	163 0
$M'$ on $g'$ }	

There is a peculiarity in all the crystals I have seen of this substance which I do not recollect to have observed in any others. They are produced nearly in halves, and appear to have rested or been formed on planes which would have passed through the middle of the entire crystal. One of these natural segments is shown in fig. 6. In

Fig. 6.



\*  $c$  is a dull plane, and occurs on only one of the crystals out of several that I have seen.

some of these, the front half of fig. 5 is the portion produced, the plane *f* being then uppermost. In some of the segments, however, there is a slight deviation from this exactness of position of the planes *f* or *h*.

*Nitrate of Soda.*

The primary form is an obtuse rhomboid.

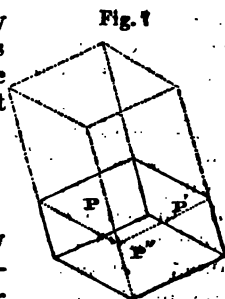
P on P'	106° 30'
P on P''	73 30

and there are not any modifications on the crystals measured. Some of these are lengthened into apparently oblique rhombic prisms, as shown by the produced dotted lines in fig. 7, but this disproportionate extension of some of the primary planes has been already stated not to be unfrequent among crystals.

I am, dear Sir, yours truly,

H. J. BROOKE.

I have just learned that Mr. Levy has very recently taken up this subject, and has measured and determined with a view to publication, the forms of many of the artificial crystals, without being aware of my having previously occupied myself in a similar manner. He has proceeded mathematically, and will probably still give his results to the public; and there can be no doubt that he will confer an additional interest on the inquiry.



ARTICLE X.

*Astronomical Observations, 1823.*

By Col. Beaufoy, FRS.

*Bushey Heath, near Stanmore.*

Latitude 51° 37' 44.3" North. Longitude West in time 1° 20.93".

April 20. Emerison of Jupiter's first satellite.....	{ 8 <sup>h</sup> 19' 43"	Mean Time at Bushey.
	{ 8 21 04	Mean Time at Greenwich.
May 6. Emerison of Jupiter's second satellite.....	{ 8 26 32	Mean Time at Bushey.
	{ 8 27 53	Mean Time at Greenwich.

## ARTICLE XI.

## ANALYSES OF BOOKS.

*An Essay on Magnetic Attractions, and on the Laws of Terrestrial and Electro-Magnetism, &c.* By Peter Barlow, Associate in the Society of Civil Engineers, and of the Royal Military Academy. *Second Edition, much enlarged and improved.*

WE have to apologize to Prof. Barlow, and to our readers, for our tardiness in noticing this much improved edition of his work; but we shall now endeavour to compensate for the delay, by giving a more complete account of it than various circumstances, which it is unnecessary to particularize, would have permitted us to have done at an earlier period.

The leading object of this edition, as of the former one, although a considerable portion of the work is devoted to electromagnetism, is the developement of the mathematical principles of magnetism, and their application to the correction of the local attraction of vessels, "which is of more and more importance," the author remarks, in the preface, "as every year is leading to some new application of iron in the construction and equipment of ships of war, and which, if persevered in without some mode of correction, would soon render the compass worse than useless as a nautical instrument."

"It may be observed, for example," he continues, "that besides there being at present considerably more iron ballast than formerly, the water-casks are now replaced by iron tanks presenting an immense attracting surface; iron knees, sleepers, plates, and, in some cases, the riders, have been introduced in lieu of those of timber; even the hempen cables have been put *hors de service* by the patent cables of iron,—gun-carriages of this metal are at this moment supplanting those of the usual material: the ingenious patent capstan of Captain Phillips, which will doubtless soon become generally applied, is principally of iron; and, although of no considerable mass, is so situated as to affect the compass very sensibly; and, lastly, it seems probable that even the masts are to be attempted in this material."

The work is now divided into three parts; the first containing the greater portion of the matter constituting the former edition, of which some account was given in the *Annals*, O. S. vol. xvi. p. 294—307, with the addition of some experiments on the local attraction of vessels, which, by the favour of the Lords of the Admiralty, the author has been enabled to make on board several of his Majesty's vessels, and of the results on the same subject obtained by a series of observations in his Majesty's ship *Leven*, during a voyage to, and survey of, the western coast of Africa. Prof. Barlow has also appended to this divi-

sion of the work, his papers, "*On the Effects produced in the Rates of Chronometers by the Proximity of Masses of Iron*," and "*On the anomalous magnetic Action of hot Iron between the bright and blood-red Heat*," first published in the *Phil. Trans.* for 1821 and 1822; and already noticed in the *Annals*.

After describing the additional experiments and observations on the local attraction, which fully confirm his former conclusions, Mr. B. concludes the consideration of that subject in the following manner:

"Such is the present state of this method of correcting the local attraction of vessels; and here I must take my leave of the subject, on which I have already bestowed much time, and have incurred some pecuniary charge in carrying the experiments into execution. I have, I trust, shown very clearly by the results reported in the preceding part of this section, particularly in those of the *Barracouta*, that the errors arising from the local attraction are of such a nature and amount, as to require correction. I have also explained a simple method by which this is proposed to be effected; and I have given testimonials of its efficacy, in a case where it was submitted to trial during a voyage of sixteen months."

The second part of this Essay contains "*A Theoretical Investigation of the Laws of Induced and Terrestrial Magnetism*," consisting of the following sections: "I. Investigation of the laws of magnetism peculiar to iron bodies—Of the horizontal needle—Of the dipping needle—General results. II. On the change of magnetic intensity of a needle as affected by iron spheres. III. On the magnetic action of bars of iron—Supplementary experiments on the action of iron plates. IV. Application of the preceding formulæ to the magnetism of the terrestrial sphere. V. On the situation of the terrestrial magnetic axis, and on its annual motion."

Mr. Barlow founds the investigation in the first section upon the following hypothesis, which is a modification of that proposed by Mr. Bonnycastle in the *Philosophical Magazine*, vol. iv. p. 132, 446.

"V. Magnetic phenomena are due to the existence of two fluids in a greater or less degree of combination, and such, that the particles of the same fluid repel, and those of an opposite nature attract, each other.

"2. These fluids in iron bodies exist naturally in a state of combination and equilibrium, till that state is disturbed by some exciting cause.

"3. But if a body, already magnetic, i. e. one in which these fluids are held in a state of separation, be brought within the vicinity of a mass of iron, such as is supposed above, the concentrated action of each fluid in the magnetized body will act upon the latent fluids in the quiescent body, by repelling those of the same, and attracting those of the contrary kind, and thus impress upon the latter a temporary state of magnetic action, which will

remain only while the two bodies maintain their respective situations.

"4. The quantity of action thus impressed upon the iron body will depend, *first*, upon the intensity of the exciting magnet; *secondly*, upon the capacity of the quiescent body for magnetism; or the quantity of those fluids contained in it; and, *thirdly*, upon the cohesive power of the iron, which latter quality determines the depth to which the exciting magnet is able to disengage the two fluids.

"The above embraces every case; viz. of any magnet, natural or artificial, developing the magnetism in any given iron body; but in that to which our attention will be principally directed, namely, the displacement occasioned by the magnetic action of the earth on spheres of iron, we shall find more limited in its results, and more susceptible of correct mathematical investigation.

"5. In this case, for instance, we may suppose the action to take place on every particle of the mass in lines parallel to each other, and corresponding with the direction of the dipping needle; also that every particle is at the same distance from the centre of the disturbing force, and consequently that the displacement in each particle is equal also; conditions which throw great facilities into the analytical investigation of the laws of action.

"6. For the sake of illustration, let  $ABCD$ , fig. 9 (PL XX), represent a sphere of iron in its non-magnetic, or quiescent state, and let  $CM$  be the line in which the terrestrial magnetism is exerted from a centre of action,  $M$ , which is at such a distance that the diameter of the sphere is inconsiderable in comparison with it; then every particle on its surface, and to a certain distance within it, will be acted upon by equal powers, and in directions parallel to each other; whereby the fluids in the quiescent body, before in a state of combination, will be separated in each particle; and the two fluids may now, therefore, be conceived to form two spherical shells,  $AeBd$ ,  $Ae'Bd'$ , whose centre of action will be in  $c$ ,  $c'$ , their distance from each other being greater or less, according to the circumstances stated in No. 4.

"7. Therefore, in computing the action of such a mass of iron in its temporary state of magnetism upon a distant particle of magnetic fluid, we may refer it to those centres; we shall also assume, that the law of action in this, as in all other cases of central action, is inversely as the square of the distance."

The limits to which we are necessarily restricted will not allow us to proceed with our author to the mathematical deduction from the foregoing hypothesis of the laws which he had previously drawn from experiment; nor to his computations respecting the horizontal and dipping needles; suffice it to say, that the approximative agreement of their results with actual observation, is of a very satisfactory nature. We are likewise compelled to pass over the second, third, and fourth sections.

From the calculations in the fifth section, it appears, that, according to the hypothesis, "the dip has not an uniform increase, but is changing now more rapidly than it has ever before done since magnetical observations have been made. Its decrease during the last five years has been nearly half a degree; and if our principles be correct," Mr. Barlow continues, "it ought to decrease nearly the same during the next five years; a short time, therefore, will either confirm or refute the hypothesis on which we have founded the preceding computations. Agreeably to which we ought to find in

1828 the variation	24°	29'	dip	69°	43'
1833 .....	24	26	..	69	21

"The dip, therefore, is at present changing more rapidly than the variation; and it will continue to decrease with the latter for about 260 years, when the longitude of the magnetic pole will be 180°; the variation will, therefore, then be nothing, and the dip only 56°, which will be its minimum; they will then both increase together for the next 260 years, when the needle will have its greatest easterly variation, and will then again return towards the north, the variation decreasing, but the dip still increasing, for 165 years longer; viz. till about the year 2510, when the magnetic pole will be again on the meridian of London; the variation will be zero, and the dip being then at its maximum will amount to 77° 43'."

### PART III.—On Electromagnetism.

This portion of Mr. Barlow's work consists of three sections, the first being a sketch of the present state of the science, in which, after mentioning the long known facts respecting the magnetic agency of lightning, and the early experiments of Ritter, he concisely describes the late researches and experiments of Prof. Oersted, M. Ampere, M. Arago, Sir H. Davy, and Mr. Faraday. The second section relates to the mathematical laws of electromagnetism. "In the preceding parts of this work," the author observes, "I have attempted to reduce the laws of induced magnetism to mathematical principles; . . . and as soon as I heard of M. Oersted's discovery, I was desirous to establish, on similar principles, the law of electromagnetism; but it was some time before I was able to construct an apparatus convenient for the purpose. Having, however, at length effected this necessary preliminary to my satisfaction, I proceeded to make the course of experiments, and to undertake the investigations which form the subject of the present section."

"My first object was to repeat very carefully all the experiments of M. Oersted, MM. Ampere and Arago, of Sir H. Davy, and Mr. Faraday, with some others suggested by the results thus obtained; and having attentively considered all the peculiarities of action thus developed, I was led to consider that all the apparently anomalous effects produced on a magnetized

needle by the action of a galvanic wire, might be explained by the admission of one simple principle; viz, that every particle of the galvanic fluid in the conducting wire acts on every particle of the magnetic fluid in a magnetized needle, with a force varying inversely as the square of the distance; but that the action of the particles of the fluid in the wire is neither to attract nor to repel either poles of a magnetic particle, but a tangential force which has a tendency to place the poles of either fluids at right angles to those of the other; whereby a magnetic particle, supposing it under the influence of the wire only, would always place itself at right angles to the line let fall from it perpendicular to the wire, and to the direction of the wire itself at that point."

"I pretend not to illustrate the mechanical principles by which such an action can be produced; I propose only to show, that if such a force be admitted, all the results obtained from the reciprocal action of a galvanic wire and a magnetized needle may not only be explained, but computed, and that the results agree numerically with experiments."

The galvanic instrument employed by Mr. Barlow differs from Dr. Hare's calorimotor merely in the mechanical contrivance for lowering it into and raising it out of the fluid; "that part of the apparatus which peculiarly appertains to the experiments I am about to detail," he says, "is represented in fig. 10, A B is an upright stand, placed near the poles of the battery; *a b, c d*, are two staples of stout copper wire, driven into the upright, the two ends at *b* and *c* passing quite through, as shown at C and Z; and on which two wires are fastened by spiral turns, and with which the communication is made with the poles of the battery; *e f, g h*, are two copper wires of the same dimension as the staples, each four feet long, having their ends flattened and drilled so as just to enable them to slide freely upon the wires *a b, c d*, and the vertical wire *f h*, also four feet in length, which passes through a hole in the top of the table E G H I, and so tight as to render it perfectly fixed. On the plane of the table, which is two feet in square, the circle N E S W is described about the centre *o*, and divided into the points of the compass and smaller divisions; N S is an index or box ruler, through which the wire *f h* passes, so that the former may be turned freely about the latter, and set to any proposed azimuth. On this ruler is placed the small compass *c'*, by means of which the deviation at any given time may be taken; *c''* is another compass placed on the top of the support L *c''*, and is intended to remain fixed in its place, in order to serve as a standard for estimating and comparing the power of the battery at different times."

"For the principal experiments, this apparatus is placed so that the plane of the rectangle of wires is perpendicular to the magnetic meridian; because in this position the horizontal wires being east and west, they have no effect in deflecting the

needle from its direction (at least there is only one exception to this, which will be noticed hereafter), and consequently all the effect produced upon the needle during the rotation of the index in the circle N E S W, is due to the vertical wire only, except so far as the horizontal wires may increase or diminish the directive power of the needle. This, however, in the cases to which we shall refer, is very inconsiderable."

"But in order that we may know precisely what part of the change of deviation between one situation of the compass and another, is actually due to that change of position, recourse must be had to the standard compass, which, always remaining fixed in its position, may be used as a constant indicator of the strength of the battery. But as the application of this measure to computation is involved in principles not at present explained, it will be proper first to inform the reader of the means which I employ in the first instance to preserve a uniformity of action during every separate course of experiments. These were as follow:—

"The vessel which contains the dilute acid, into which the plates are immersed, holds nearly 20 gallons; and I begin the experiments with little more than 12 gallons; moreover the plates are not, in the first instance, let down to their lowest point. The intensity shown by the standard compass after the connexion has been made some minutes, is noted; and by breaking off and making the contact anew, this same intensity occurs again, the power being always strongest when the contact is first made; then when the standard compass returns to its former bearing, the observation with the other compass is taken; the contact broken and renewed, and so on as long as the battery retains sufficient power. When this fails, the plates are lowered a little more; the power thus increased, and the observations resumed, till at length the plates being wholly down, and the power too weak, recourse is had to a supply of more dilute acid; by which means a tolerably steady action is kept up longer than is necessary for any series of experiments of this kind. It will be observed here, that in this case the only use made of the 'standard compass' is to indicate the *same intensity of action*, and consequently involves no theoretical principle that will be objected to by the most scrupulous theorist or observer, but it will be seen in subsequent articles that this indicator is susceptible of a more extensive application."

Prof. Barlow having thus described his method of experimenting, now proceeds to explain the principles of computation, and to compare the numerical results thus obtained, with those derived from experiments.

"According to the hypothesis, if we conceive the wire in the first instance to be vertical, and the compass placed to the north or south of it, and opposite its middle point, the centre of action will lie in the horizontal plane, and at right angles to the

natural horizontal direction of the needle. The latter, therefore (which, for simplicity sake, we shall at present consider as indefinitely short with regard to the distance), will, at either of those points, be acted upon by two rectangular forces; viz. the galvanic force in an east and west direction, and which we may denote by  $f$ , and the natural magnetic or directive force  $m$ ; consequently, according to the principle of forces, the resultant will be expressed by  $\sqrt{f^2 + m^2}$  and the angle which it makes with the natural direction of the needle, being called  $\Delta$ , we shall have

$$\tan. \Delta = \frac{f}{m} \dots \dots \dots (1)$$

Hence the magnetic force being constant, the tangent of the needle's deviation at the north or south will be a correct measure of the galvanic power."

"We have thus a principle by means of which we may verify a part at least of our theory by experiments. For example; since by the supposition every particle of the galvanic vertical wire acts inversely as the square of its distance from a given point, we ought to find a determined relation between the tangent of deviation and the length of the wire; or the length of the wire remaining constant, between the tangent of deviation and the distance, provided always that the intensity of the battery remain constant."

"The apparatus already explained furnishes us with the opportunity of making both these comparisons. For by means of the sliding horizontal rods, the vertical conducting part of the wire may be shortened in an instant; and, in the second case, it is only necessary to slide up the compass to different distances, which may likewise be done so quickly, that it will not be necessary even to have recourse to the standard compass."

"It is fortunate also that the calculation here alluded to is of the simplest kind. For denoting the length of the wire by  $2l$ , and the distance of the compass by  $d$ ; assuming also  $x$  as any variable length, the corresponding elementary action at this distance will be  $\frac{x}{d^2 + x^2}$ , and the sum of these actions will be

$$\int \frac{x}{d^2 + x^2} = \frac{1}{d} \text{arc. tan. } \frac{x}{d},$$

which vanishes when  $x$  vanishes; and which, therefore, when  $x = l$ , and the two lengths are included, becomes  $\frac{2}{d} \text{arc. tan. } \frac{l}{d}$ , consequently if we denote the deviation, as we have done above by  $\Delta$ , we ought to find this force vary inversely as  $\tan. \Delta$ , or

$$\cot. \Delta \left\{ \frac{1}{d} \text{arc. tan. } \frac{l}{d} \right\} = \text{a constant quantity.}$$

"The following are a few out of numerous experiments of this kind which I have made, and which have been all found equally satisfactory.

"Experiments to determine the Magnetic Deviation caused by a galvanic vertical Wire at different Distances. Length of vertical Wire 36 Inches.

Deviation by standard compass,	Distance of the other compass from the wire.	Mean & observed deviation $\Delta$ .	Value of $\frac{2}{d}$ arc. $-\tan \frac{1}{d} = A$ .	Constant product. $A \cot \Delta$
25° 0'	12 inch.	5° 37'	18.772	190880
Ditto	8	11 15	34.100	171432
Ditto	6	16 30	47.712	161062
Ditto	4	26 30	77.500	154440
Mean				164728

"When it is considered that these observations were made on a compass needle only one inch in length, and that the divisions extended only to quarter points, it is impossible to expect a closer approximation. The needle and card, however, being delicately suspended, and the latter very distinctly divided, I could depend upon my observations to the nearest *degree*; for by means of a strong magnifying power I could always bisect and trisect the quarter points without any very sensible error."

Mr. Barlow next gives some *experiments to determine the magnetic deviation caused by a vertical galvanic wire; the length being varied, but the distance constantly nine inches*; and having thus far verified his hypothesis by experiment, he proceeds to the consideration of the deviation in different azimuths; but as the limits to which we are confined will not allow us to follow him, we must terminate our notice of the section with some of his concluding remarks.

"My results," he says, "are necessarily only approximate; because I have throughout supposed the needle indefinitely short in comparison with the distance and length of the wire; but by this means I have rendered the subject perfectly intelligible to every one; whereas had I taken the actual case of the reciprocal action of every particle of the fluid in the wire upon every particle in the needle, and had been able to complete the investigation, it could only have been understood by a few mathematicians; at the same time the minute corrections thus introduced would not have been appreciable in the comparison of the results with experiments; these latter being necessarily both liable to small irregularities, and difficult to observe."

"It will have been noticed that I have only attempted to illustrate the nature of the action which has place between a gal-

"That is, the mean of two observations at each station of the compass; the contact being changed. The same is to be understood of the deviation with the standard compass."

vanic wire and the compass, and not that of one galvanic wire on another. What modification the hypothesis may require to explain the latter class of phenomena, will be examined hereafter. I have hitherto supposed only one species of action in the galvanic wire, but it is highly probable that it is compound; and that while the north end of the needle is carried in one direction by the action we have supposed, the south end is carried in an opposite direction; not merely as a consequence of the first force, but by a distinct power. This will not, however, in any respect, affect our investigation; because both forces lead to similar results. . . . . I am well aware of the difficulty of conceiving the mechanical principles by which such a tangential force, as is here assumed, can operate; but, on the other hand, it must, I think, be conceded, that the simple power of attraction is equally difficult to conceive, and that we admit it, not from having any idea of the *modus operandi*, but because we find that it leads to results that are consistent with actual observations; and I have endeavoured to show, in the preceding pages, that the force we have assumed is admissible upon precisely the same ground."

The third section of this part contains a course of electromagnetic experiments, "due to the several ingenious philosophers who have interested themselves in this pursuit;" and in which Mr. Barlow endeavours "to show their mutual dependencies on each other, and their general agreement and particular connection with the mathematical theory advanced in the second section."

These are succeeded by addenda to Sect. 12 and 13, Part I. "On the Magnetic Effects of Iron Masts on the Compass," which terminate this valuable work, and from which it appears, that the contemplated employment of hollow iron masts in ships of war, in lieu of those at present in the service, will be productive of no disturbance on the compass, under any circumstances, but what may readily be corrected by the method Mr. Barlow has proposed.

## ARTICLE XII.

### *Proceedings of Philosophical Societies.*

#### ROYAL SOCIETY.

**April 17.**—On the Application of the Liquids produced by the Condensation of Gases as Mechanical Agents. By Sir H. Davy, Bart. PRS.

"It is well known that the elasticity of vapours in contact with the liquids from which they are produced at high temperatures, increases in a much higher ratio than the arithmetical progres-

nion of the temperatures; but the great liberation of latent heat by compression, and its absorption by expansion, seem to render the mechanical application of them at temperatures greatly elevated above the boiling points of the respective liquids of doubtful economy. No doubt, however, in this respect, observed the President, can be entertained of those vapours which can only be produced under high pressures, and at common temperatures. To illustrate this subject, Sir Humphry described the results of some experiments on the increase of elastic force in the vapours of several of the condensed gases; one of them, that of liquid sulphuretted hydrogen, which exerts a pressure of 14 atmospheres at  $3^{\circ}$ , becomes equal in pressure to 17 atmospheres at  $47^{\circ}$ .

Among other experiments described in this communication, were some in which the author had liquefied prussic gas and sulphurous acid gas, by confining them with a portion of ether in glass tubes over mercury, and then raising the ether in vapour:—as this vapour condensed, the liquefied gas resumed its aeriform state, and cold was produced.

Sir Humphry concluded with some remarks on the probable applications in the arts of the condensed gases; among which, besides their employment as mechanical agents, he suggested those of impregnating water with large quantities of carbonic acid and other gases, without the aid of mechanical pressure; and the ready production of cold, in consequence of the rapidity with which they evaporate.

At this meeting a paper was also read, On the Temperature, at considerable Depths, of Fresh-water Lakes, within the Tropics. By Capt. Edward Sabine, FRS. in a letter to the President.

Sir H. Davy had requested Capt. Sabine, while engaged in his late observations in the tropical regions, to make some experiments on the temperature of deep lakes, with a view of obtaining facts in illustration of that interesting object of inquiry, the temperature of the interior of the earth; no opportunity of instituting such researches had, however, occurred to him, but he had made one experiment on the subject, in a part of the Caribbean Sea, which, from its confined situation, nearly resembled a lake; and this experiment he proceeded to describe, from the original memorandums of it.

At the period of this experiment, Capt. Sabine was on board one of his Majesty's vessels, in lat.  $20^{\circ} 30'$  N. and long.  $83^{\circ} 30'$  W.; between the Caymans, and Cape St. Antonio, in the Island of Cuba. The weather was fine, with light airs, and the sea not swelling in a great degree. To the bottom of a line of above 1200 fathoms, a strong iron cylinder was fastened, the top of which screwed down upon leather in order to prevent the entrance of the water, by the compression of which in the cylinder the temperature might be raised: within the cylinder was a Sir's self-registering thermometer, prevented, by means of springs

from coming into immediate contact with its sides. Above this closed cylinder was fastened another, full of perforations, to allow the water free passage through it, and likewise furnished with a thermometer. The line was let out in twenty-five minutes; and, by the lowest estimation, 1000 fathoms, or 6000 feet, was the perpendicular depth to which the cylinders descended: it was drawn back in fifty-three minutes, when, owing to the circumstance that the top of the closed cylinder had not been screwed down sufficiently upon the leather, the sea was found to have entered it; in this, the thermometer marked  $49.5^{\circ}$ , and that in the perforated cylinder  $45.5^{\circ}$ ; the temperature of the surface of the sea was from  $82^{\circ}$  to  $+ 83^{\circ}$ , so that the difference of its temperature there, and at the depth above-mentioned, amounted to  $+ 37^{\circ}$ .

Capt. Sabine inferred from this result, that at a depth little exceeding that at which the experiment was made, the sea-water would be found to have attained its greatest density, supposing that point to be, as in fresh-water, a few degrees above the freezing point. He terminated the communication by some remarks on an experiment made by Peron, the result of which, as far as it was satisfactory, agreed with his own.

At this meeting, likewise, the reading was commenced of A Continuation of Prof. Buckland's Account of the Bones discovered in Caves in various Parts of England.

April 24.—An Account of Experiments made to determine the Length of the Invariable Pendulum at various Places on the South American Station. By Capt. Basil Hall, FRS. in a letter to Capt. Kater, FRS.

The places at which Capt. Hall had ascertained the Length of an Invariable pendulum belonging to the Board of Longitude, as detailed in this paper, were the following: The volcanic island of Abingdon, one of the Gallapagos; the sea-port of San Blas on the coast of Mexico; and Rio de Janeiro on the coast of Brazil. He stated, among various other circumstances, that he had unsuccessfully endeavoured to simplify Capt. Kater's method of determination; and he intimated that he purposed to make some experiments on the actual effects of heat upon the pendulum, independently of all theoretical considerations.

May 1.—On the Expansion by Heat of Gases in various States of Condensation and Rarefaction, being an Appendix to a former paper on the Application of the Gases, condensed into Liquids, as Mechanical Agents. By Sir H. Davy, Bart. FRS.

The experiments of MM. Gay-Lussac and Dalton, have shown, that the gases expand equally for equal increments of heat, at all temperatures between  $32^{\circ}$  and  $212^{\circ}$ ; but the President was not aware that any direct experiments had been instituted with the view of determining whether the same law prevailed with regard to the respective aeriform bodies at differ-

ent degrees of density; and had, therefore, been induced to make some researches on the subject. A portion of atmospheric air was heated in a glass tube from  $32^{\circ}$  to  $212^{\circ}$ , and the space which it then occupied accurately marked; the experiment being made with air, confined by the pressure of 30 and 65 inches of mercury, it was found to occupy the same space as the air under common pressure; the same result was obtained when the air was six times condensed, and also when it was once, twice, thrice, and fifteen times rarefied.

At this meeting, the reading of Prof. Buckland's paper, which had been begun on the 17th of April, as above stated, was resumed and concluded.

Mr. Buckland had mentioned, at the end of his former paper that another cave, similar to the one he had examined at Kirkdale, had been discovered at Kirby Moorside, and that it had been closed up by the proprietor C. Duncombe, Esq. until some qualified person should be present to inspect it in its undisturbed state. The author went into Yorkshire to examine it, last July, in company with Sir H. Davy and Mr. Warburton; and though it contained not a single bone, yet its circumstances with respect to diluvial sediment and stalagmite were precisely analogous to those of the cave at Kirkdale, and fully confirmed his account of, and reasoning upon them. The second part of this paper related to a fissure of postdiluvian origin in Duncombe Park; the existence of which had not been known to Mr. Duncombe until the author's late visit. It lies open, like a pit-fall (partially concealed by bushes), across the top of a limestone hill on the west side of the valley of the Rye; its direction is oblique, and it has several ledges, at different depths, and various irregular lateral openings. It contains neither mud nor pebbles, but upon the ledges lay the dislocated skeletons of various animals that had recently fallen in and perished; comprising those of dogs, sheep, deer, goats, and hogs, &c. They were not imbedded in loam or covered with stalactite; the bones did not adhere to the tongue; retained much of their animal matter; and were evidently much more recent than those found in the cave at Kirkdale.

From the circumstances of this fissure and its contents, Prof. Buckland proceeded to illustrate the origin of the assemblages of bones in the Plymouth and other fissures and caves connected with them. The number of such fissures which are met with, filled with diluvian detritus, he observed, evinces that open fissures must have been more numerous in the antediluvian state of the earth than at present; and as it is the habit of gaminivorous animals to be constantly traversing the ground, in all directions, in the act of cropping their food, they would often be liable to fall into them, and actually do so in Derbyshire, and the limestone districts of South Wales; while carnivorous animals

would, from their different habits, be less exposed to such accidents; and this circumstance affords a probable reason why the bones of the latter are less abundant in fissures than those of the former class of animals.

In the third section of this communication, Mr. Buckland described the caves discovered at Plymouth in 1822, referring to Mr. Whidbey's account already before the Society (and reported in the *Annals* for March, p. 233—235): the Plymouth caves consisted of fissures traversing the rock in all directions, some vertical, some oblique; and the reason why several of them appeared at first to Mr. Whidbey to have had no communication with the surface, was, that they were first opened at their lower extremity, where they ended in a cul de sac, and traversed the rock so obliquely as not to reach the surface, but at a distance from the working face of the quarry. A cave discovered at Wirksworth, in Derbyshire in Dec. 1822, containing the entire skeleton of a rhinoceros, and the bones and horns, &c. of deer, and another at Paviland, near Swansea, containing bones of the elephant, rhinoceros, bear, hyæna, wolf, ox, horse, and a human female skeleton, with various indications of human habitation, were likewise described; and the paper concluded with an examination of several hypotheses by which the phenomena of the various assemblages of animal remains adverted to, might be explained, showing, that the only satisfactory manner of accounting for the presence of the mud and pebbles, is to attribute them to diluvial origin; and that with regard to the bones, they must be divided into five classes.

1. Those of carnivorous animals that retired spontaneously to the caves to die during successive generations in the period immediately preceding the deluge, as in the case of the bears' bones in the caves of Germany.

2. The remains of animals that were dragged in as food by beasts of prey during the same period, as in the case of the various remains in the cave at Kirkdale.

3. The remains of animals that fell into and perished in the open fissures and caves connected with them in the period preceding the deluge, as in the case of the bones at Plymouth and Gibraltar.

4. The remains of animals that were washed in together with the mud and pebbles at the deluge, as in the case of the entire skeleton of a rhinoceros, near Wirksworth.

5. The remains of animals that have entered caverns or fallen into open fissures since the period of the deluge, as in the case of the human bones in the open cave at Paviland, and the bones of dogs, deer, &c. in the open fissure at Duncombe Park.

May 8.—At this meeting, Prof. Oersted attended, and was admitted a foreign member of the Society; and he was justly complimented by the President on his brilliant discovery of the

magnetic effects of electricity, for which the Society had already awarded him the Copleyan medal.

The reading of Prof. Buckland's Account of Bones discovered in Caves and Fissures in various Parts of the Continent, was commenced.

*May 15.*—At this meeting, the reading of Prof. Buckland's paper was resumed and concluded.

Mr. Buckland examined the caverns alluded to in the summer of 1822, and found that all their characters and phenomena confirmed his former conclusions respecting them and the English caves: they all contain either diluvian mud, or diluvian sand and pebbles, covered with one coat of stalagmite only; the bones are imbedded in the mud, &c. and are often united with it by the infiltration of stalagmite, into an osseous breccia, resembling that of Gibraltar and the coasts of the Mediterranean. The caverns are in limestone rocks of different ages and formations, and all their circumstances concur to show that the bones they contain had existed in them previously to the inundation by which the mud and pebbles were introduced.

The cave at Scharzfeld, in Hanover, on the west border of the Hartz Forest, is in magnesian limestone analogous to that of Sunderland, being the first floetz limestone of Werner; it is situated at the elevation of 500 feet above the level of the nearest river, and it consists of one large chamber, with numerous smaller lateral connexions. Its floor of stalagmite has been much broken up by visitors in search of bones and teeth of bears and hyænas, but principally of bears. The lower cavities and under vaultings of this cavern have been wholly filled up with a mass of mud, pebbles, and bones, in which artificial excavations have been made for the purpose of extracting the bones, and it is only in these artificial cavities that any bones or teeth are found adhering to the sides or roof. In one of the smaller cavities, Prof. Buckland found the fractured head of a bear imbedded in mud, and having a large pebble lodged in the cavity of the skull.

The Bauman's cave situated on the north-east side of the Hartz, near Elbingrode, is so called from a miner, who, in 1670, went into it in search of ore, and having wandered about in it three days and nights, came out, so exhausted, that he almost immediately expired. It is in transition limestone, and is elevated about 100 feet above the river Bode, and as that river could not rise ten feet without inundating the adjacent village of Rubeland, the mud, &c. which the cave contains could not have been deposited by the floods of the river. This cavern contains a great quantity of large pebbles. The bones in it are partly imbedded in loose sand and mud, and partly united with the large pebbles into a solid breccia. Those in the breccia have been much broken, and some of them crushed to pieces (as if

in a mortar) by the movement of the heavy pebbles to which the crushed fragment still adheres united by stalagmite. The bones that lie in mud and sand in the same vault have not been thus broken. Over the whole there is a crust of stalagmite, like that in the other caverns.

The general aspect and state of the bones in all these caverns, and the circumstances attending them, are very similar to those the author had observed in cavernous fissures at Plymouth.

The caves in Franconia are situated in an elevated tract of land which forms one of the central water-heads of Europe, near the sources of the Mayn and Naab, and between the towns of Nuremberg, Bareuth, and Bamberg. They are in a bed of limestone locally called *höhlen kalk* (hole limestone), being a variety of the Jura limestone, or younger alpine limestone of the Continent.

This district is full of caverns, many of which are crowded with bones (principally of bears), while others are wholly destitute of them; but in all, there is an accumulation of diluvial sediment covered for the most part with a single crust of stalagmite. Those selected for description are the caves of Forster's Hole, Rabenstein, Zahnloch, Gailenreuth, and Kühloch. The author has attended particularly to the evidences of diluvial action within them all, and finds that in each case the mud and pebbles were superinduced upon the bones of animals which had died and become accumulated in their dens in the antediluvian period when wild beasts inhabited these countries. Forster's Hole is most remarkable for the beauty of its stalagmite and roof. In Rabenstein, the bones and mud have been but little disturbed. From Zahnloch they have been extracted for centuries, and used under the name of bones of the licorne, or fossil unicorn, for imaginary medical virtues. In this cave there is a block of stone which is polished, apparently by friction from the skin and paws of the antediluvian bears. At Gailenreuth, there is an accumulation of bony breccia 30 feet deep or more, as the bottom has not yet been penetrated; and in Kühloch, so great a mass of black animal earth with bones dispersed through it, that, allowing two cubic feet of matter for the exuviae of each individual, this single cave must contain the remains of at least 2600 bears, a number which may have been supplied in 1000 years by a mortality of two and a half per annum. The cave in which this singular mass is found was probably the lowest part of a large and connected series of caverns inhabited by bears, and into which, during successive generations, these animals retired to die. The animal matter covers the entire floor to the depth of about six feet, which, being multiplied by the length and breadth of the cave, shows the total quantity to be not less than 5000 cubic feet: the bones and teeth dispersed through this dust are much decayed, and readily crumble into the same dark umber-coloured powder as that which forms the greater

part of the matrix in which they are imbedded. In this cave there is no stalagmite, neither are there any pebbles; its animal earth also is peculiar, but the author points out in its situation and circumstances sufficient causes to account for these apparent anomalies.

Professor Buckland concluded this communication with some general remarks on the caves in Germany, among which were the following: 1. The present entrances to them were not their original openings, but are only truncated portions of their lower branches laid open by diluvial denudation: 2. The diluvium they contain is either loam, sand, or pebbles, but more commonly a mixture of all three, through which the bones lie interspersed, and the whole mass has often been indurated into an osseous breccia, like that of Gibraltar. 3. The loam has not been produced by the decay of the flesh or bones, or of the rock in which the cave exists, but it agrees, in chemical constitution, with that of the diluvial beds of the adjacent country. 4. The number of caves in which any bones at all are found, is comparatively very small, but where they occur it is usually in enormous quantities. 5. Every circumstance tends to evince, that the mud, pebbles, &c. were washed in by the deluge upon the bones already existing in the caves: if, on the contrary, all the bones had been drifted in by the diluvian waters, they would be found dispersed in small quantities only, and in numerous caves. 6. There is only one superficial crust of stalagmite in any of the caves, and no alternations of mud, pebbles, and bones, but simply one confused mass covered by a single crust of stalagmite. 7. The identity of the period in which the animals lived whose remains occur in caves, fissures, and diluvial gravel or loam, is shown by the agreement of the species of animals whose remains they contain; since it appears that the extinct hyæna, bear, elephant, and rhinoceros, occur, with many other animals, in diluvial gravel beds, as well as in caves; while the extinct tiger is found, together with the remains of horses, oxen, deer, &c. in fissures and caverns, as well as in superficial beds of diluvial gravel. The period also in which the animals lived, whose remains are found in the breccia of Gibraltar, is shown to be the same as that in which the hyænas inhabited the den at Kirkdale, and the bears the caves of Germany, viz. that immediately preceding the deluge. 8. The author concludes that the inundation which destroyed these animals was transient and universal; that it also covered the highest mountains; and that it took place at a period which cannot have exceeded a few thousand years ago. To these are added some important examples of the effect of the diluvian waters in the excavation of valleys, and of the accumulations of diluvial gravel in Britain, and in other parts of the world.

At this meeting, the reading of the following paper was also commenced, and the completion of it postponed to another

meeting:—An Account of a Magnetic Balance, and of some Experiments on Magnetism recently made with it. By William Snow Harris, Esq. Communicated by the President.

In consequence of the approaching festival, the Society then adjourned over one Thursday, to meet again on the 29th of May.

#### LINNEAN SOCIETY.

*April 1.*—Remarks on a Minute Luminous Insect frequently observed in the course of a voyage to India. By Major-Gen. Hardwicke, FLS. &c.

This insect, to which the author would not venture to give a name, is three lines in length, by one and a half in width; is oblong, ovate, depressed, and so thin as to be semitransparent: it consists of nine segments, which are all provided with hairy tufts, apparently legs; the first segment contains the head and the thorax. After being taken up from the sea, it remained luminous for an hour, in a bucket of salt-water; and, for some minutes, in the hand. Some of the small *cancræ* are luminous, as well as some of the *onisci*; Gen. Hardwicke considers that the insect above described approaches more to the former genus than to the latter one.

At this meeting, the reading of the following paper was commenced:—Commentary on the second Part of the *Hortus Malabaricus*. By Francis Hamilton, MD. &c.

On *April 15*, and *May 6*, the reading of Dr. Hamilton's paper was continued.

#### GEOLOGICAL SOCIETY.

*May 2.*—A paper was read, on the Geology of Upper Canada.

A notice was read, on the Discovery of a large Fossil Elephant's Tusk, near Charmouth, Dorset. By H. T. de la Beche, Esq. MGS.

A paper was read, entitled, "Observations on the Genus *Actinocamax*." By J. S. Miller, Esq. ALS.

A paper was read, on the Belemnites of the Chalk and alluvial Strata of Norfolk and Suffolk, with Notices on their Localities, and accompanying Fossils. By Richard Taylor, Esq.

*May 16.*—A memoir was read, on the Geology of Southern Pembrokeshire, from the observations of H. T. de la Beche, Esq. FRS. FLS. MGS. and the Rev. W. D. Conybeare, FRS. MGS. &c. Drawn up and communicated by the former.

This memoir is accompanied by a map, and extensive sections of the coast. The constituent formations occurring in this district are as follows, beginning with the lowermost: 1. Trap. 2. Greywacke. 3. Old red sandstone. 4. Carboniferous limestone. 5. Coal measures.

## ARTICLE XIII.

SCIENTIFIC INTELLIGENCE, AND NOTICES OF SUBJECTS  
CONNECTED WITH SCIENCE.I. *Action of Phosphorus and Water.*

It is well known that when phosphorus is kept in water, the sticks, although at first transparent, are covered with a white opaque crust, which eventually becomes of a reddish-yellow colour.

Wishing to determine the nature of this change and the circumstances by which it is accelerated or retarded, I placed translucent sticks of phosphorus in two vials nearly filled with distilled water. One of the vials was exposed to day-light, and occasionally to the direct rays of the sun. In a few hours, that side of the cylinder of phosphorus which was exposed to the light became opaque, and the water acquired a peculiar smell. On putting litmus paper into the water, it became of a red colour, indicating the presence of an acid, and which was probably the phosphorous acid, or a mixture of that and the phosphoric: this I did not determine. I now added some of the water to solution of nitrate of silver, and another portion to solution of proto-nitrate of mercury; in both cases, a dark-coloured precipitate was formed, which is a well-known characteristic of the presence of phosphuretted hydrogen. By exposure to air, the solution loses its power of giving a dark-coloured precipitate with silver and mercury, owing partly to the evaporation of the phosphuretted hydrogen, and, probably more to its conversion into phosphoric acid and water by absorbing oxygen. After nearly a month had elapsed, I examined the pieces of phosphorus which had been excluded from the action of the light; the water in which they had been immersed was slightly acid, and gave indications of the presence of phosphuretted hydrogen in a slight degree; the action, however, which had taken place was so trifling that the transparency of the phosphorus was scarcely at all diminished.

From this statement, it is evident, that phosphorus has the power of decomposing water; that oxide of phosphorus is first formed, and eventually phosphorous or phosphoric acid; and that the hydrogen of the water forms phosphuretted hydrogen, with a portion of the phosphorus; and, lastly, that these effects are much accelerated by the action of light.—*Edit.*

II. *Phosphate of Uranium.*

In the *Annals* for January last, I gave an analysis of the green uranite from Cornwall, and I stated my reasons for supposing that the uranite from Autun, which has the same crystalline form as the Cornish, was also a phosphate of uranium. Mr. Heuland having had the kindness to present me with some of the French uranite, I subjected it to examination, and found, as I had supposed, that it is a phosphate. Joseph Carne, Esq. of Riviere, has also been so good as to supply me with a new variety of the uranite from Cornwall; this, instead of being crystallized in square plates, and of a green colour, is composed of fibres radiating from a centre; this I also submitted to examination, and found it to be similar to the specimens above alluded to.—*Edit.*

III. *On the Question as to the Existence of Metallic Veins in the Transition Limestone of Plymouth.* By the Rev. Richard Hennah.

(To the Editor of the *Annals of Philosophy*.)

SIR,

Citadel, Plymouth, May 13, 1823.

I have been prevented by illness and other circumstances from attending so early as I wished to your note annexed to my letter, on the subject of metallic veins in the Plymouth limestone, inserted in the 28th number of the *Annals of Philosophy*.

I have called on Edmund Moss at Cat-Down this day, not with a view to remove any doubts on the subject existing in my own mind, for there were none, so much as to satisfy your correspondent on the point in question. In answer to my inquiries, he said, that he had worked in the lime-quarries at Cat-Down and the neighbourhood upwards of 40 years, and had never observed any mineral whatever, or quartz, in mass in any of them. He recollects, however, selling some time ago to a dealer in Plymouth, a few specimens from the lime, and, among them, one or two from the vicinity of Tavistock; but that the latter were totally different from any thing we had in our quarries.

I have also asked several other quarrymen, some of whom have been so employed for 12 or 15 years, whether they ever met with, in their workings, any veins of any of the metals, or quartz in quantity among the limestone of Plymouth? To which they have invariably answered, No, and seemed to wonder at my asking such a question, as at a thing never heard of.

I shall feel obliged, therefore, by your inserting the above, not only for the purpose of satisfying your correspondent, but because it may also be not unacceptable to your numerous readers in general.

I remain, Sir, your obedient servant,

RICHARD HENNAH.

## ARTICLE XIV.

### NEW SCIENTIFIC BOOKS.

#### PREPARING FOR PUBLICATION.

Mr. E. W. Brayley, Jun. is preparing a work on The Natural History of Meteorites, or of those remarkable masses of iron and of earthy and metallic compounds, which, at different periods, have fallen from the atmosphere, as well in England, as in many other countries; including remarks on their probable origin. With a Historical Introduction, showing that the worship of them was widely prevalent in former ages, and that it still continues in certain Pagan countries; and an Appendix of Tables, &c. In 1 vol. 12mo. illustrated by Plates and Diagrams.

Mr. J. F. Daniell has in the press a volume of Meteorological Essays.

Mr. Patrick Syme is preparing a work on British Song Birds, to be illustrated with 15 coloured Engravings.

A New Edition of Dr. Gordon's Forensic Medicine, which will contain an accession of much valuable matter.

Another Edition also of Mr. T. Peckston's work on Gas-Lighting will shortly appear, and will contain several alterations, and considerable additions.

Mr. T. Tredgold is about to publish an Essay on the Principles and Practice of Heating by Steam.

Illustrations of the Mode of maintaining Health, curing Diseases, and protracting Longevity, by Dr. Forster, will shortly be published.

In the press, Practical Remarks on Fractures at the upper Part of the Thigh, and particularly Fractures within the Capsular Ligament, by Mr. Earle.

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## ARTICLE XV.

### NEW PATENTS.

P. Chell, of Earle's-court, Kensington, Middlesex, engineer, for certain improvements on machinery for drawing, roving, and spinning hemp, flax, and waste silk.—Feb. 18.

A. Applegath, of Duke-street, Stamford-street, Blackfriars, Surrey, printer, for certain improvements in printing machines.—Feb. 18.

T. Bury, of Salford, Manchester, dyer, for improvements in dyeing or producing a permanent nankeen colour on cotton, wool, skein-yarn, and certain other articles.—Feb. 18.

P. Deakin, of Birmingham, sword-maker, for improvements in piano-fortes, and other stringed instruments.—Feb. 18.

W. Church, of Nelson-square, Surrey, gentleman, for an improved apparatus for printing, to be used by type, block, or plate printers.—Feb. 18.

## ARTICLE XVI.

## METEOROLOGICAL TABLE.

1823.	Wind.	BAROMETER.		THERMOMETER.		Evap.	Rain.	Daniell's hyg. at noon.
		Max.	Min.	Max.	Min.			
4th Mon.								
April 1	S W	30·16	29·86	66	46	—		
2	S W	29·90	29·86	56	39	—	02	
3	W	29·90	29·54	52	44	—	15	
4	N W	29·54	29·18	54	43	—	22	
5	N W	29·29	29·18	55	39	—	35	
6	N W	29·83	29·29	47	40	—	18	
7	N	29·98	29·83	53	30	—	07	
8	N E	29·98	29·93	51	38	—		
9	N E	30·21	29·93	56	31	—		
10	E	30·32	30·21	55	29	—		
11	E	30·30	30·28	56	25	·81		
12	N E	30·28	30·23	46	30	—		
13	N E	30·23	30·22	50	39	—		
14	E	30·45	30·22	52	39	—	—	
15	Var.	30·46	30·29	56	35	—		
16	W	30·29	30·10	56	46	—	02	
17	N W	30·10	29·75	64	40	—		
18	N W	29·75	29·60	55	33	—	—	
19	N W	30·01	29·62	50	28	—	05	
20	N W	30·07	30·01	50	35	—		
21	S W	30·01	29·93	50	28	·82		
22	E	29·93	29·54	60	39	—		
23	E	29·65	29·40	60	42	—	24	
24	N E	29·99	29·65	55	30	—		
25	S W	29·99	29·84	55	44	—	03	
26	S E	30·10	29·84	53	36	—	48	
27	N E	30·28	30·10	54	27	—		
28	N W	30·35	30·28	55	34	—		
29	N E	30·53	30·33	60	30	—		
30	S E	30·53	30·49	65	30	·72		
		30·53	29·18	66	27	2·35	1·81	

The observations in each line of the table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes that the result is included in the next following observation.

## REMARKS.

*Fourth Month.*—1. Fine. 2. Cloudy: windy. 3. Morning fine: rain in the evening. 4, 5. Rainy. 6. Cloudy. 7. Showery. 8. Cloudy. 9—11. Fine. 12, 13. Cloudy. 14. Cloudy: a few drops of rain. 15, 16. Cloudy. 17. Fine. 18. Fine: a shower of hail about noon. 19. Fine: a shower of hail about four, p.m. 20. Fine: wind cold. 21, 22. Fine. 23. Cloudy. 24, 25. Fine. 26. Rainy. 27. Fine. 28. White frost in the morning. 29. Fine. 30. White frost, with fog in the morning.

## RESULTS.

Winds: N, 1; NE, 7; E, 5; SE, 2; SW, 4; W, 2; NW, 8; Var. 1.

## Barometer: Mean height

For the month..... 29.882 inches.

For the lunar period, ending the 3d ..... 29.964

For 14 days, ending the 8th (moon south) ..... 29.900

For 13 days, ending the 21st (moon north) ..... 30.110

## Thermometer: Mean height

For the month..... 45.483°

For the lunar period ..... 42.266

For 31 days, the sun in Aries ..... 45.096

Evaporation..... 2.35 in.

Rain..... 1.81

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